THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON,

FROM THEIR COMMENCEMENT, IN 1665, *TO THE YEAR* 1800;

Abridged,

WITH NOTES AND BIOGRAPHIC ILLUSTRATIONS,

BY

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CONTENTS OF VOLUME ELEVENTH.

THE CONTENTS CLASSED UNDER GENERAL HEADS

Class I. MATHEMATICS.

1. *Arithmetic, Annuities, Political Arithmetic.*

2. *Algebra, Analysis, Fluxions.*

3. *Geometry, Trigonometry, Surveying.*

Class II. MECHANICAL PHILOSOPHY.

1. *Dynamics.*

2. *Statics.*

3. *Astronomy, Chronology, Navigation.*

3. *Mineralogy.*

4. *Geography, and Topography.*

5. *Hydrology.*

Class IV. CHEMICAL PHILOSOPHY.

1. *Chemistry.*

2 *Meteorology.*

3. *Geology.*

Class V. PHYSIOLOGY.

1. *Anatomy.*

2. *Physiology of Animals.*

3. *Physiology of Plants.*

4. *Medicine.*

5. *Surgery.*

Class VI. The Arts.

1. *Mechanical.*

2. *Chemical.*

3. *Fine Arts.*

4. *Antiquities.*

Class VII. Biography; *or, Account of Authors.*

REFERENCES TO THE PLATES IN VOLUME XI.

See p. 1.2.3.

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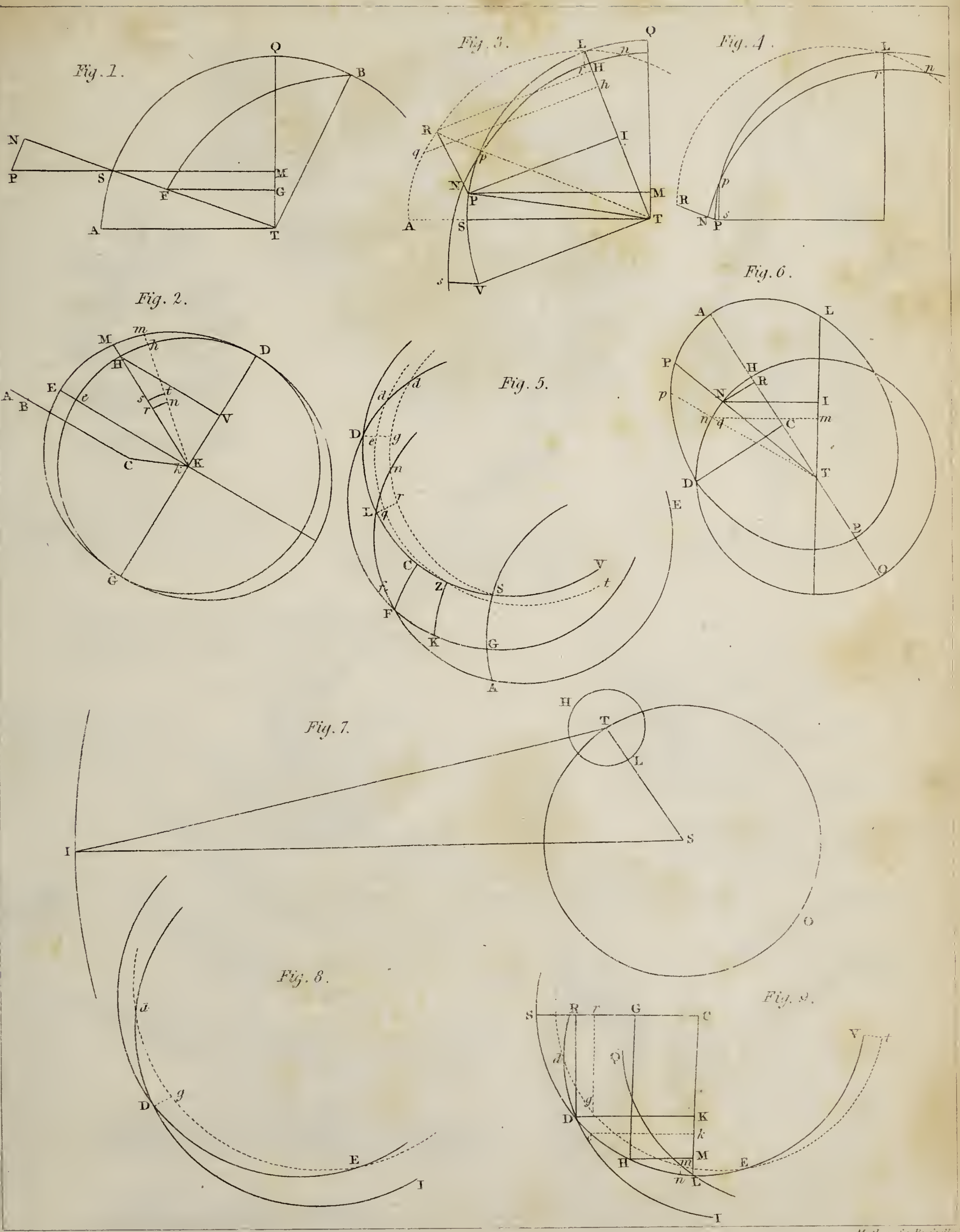

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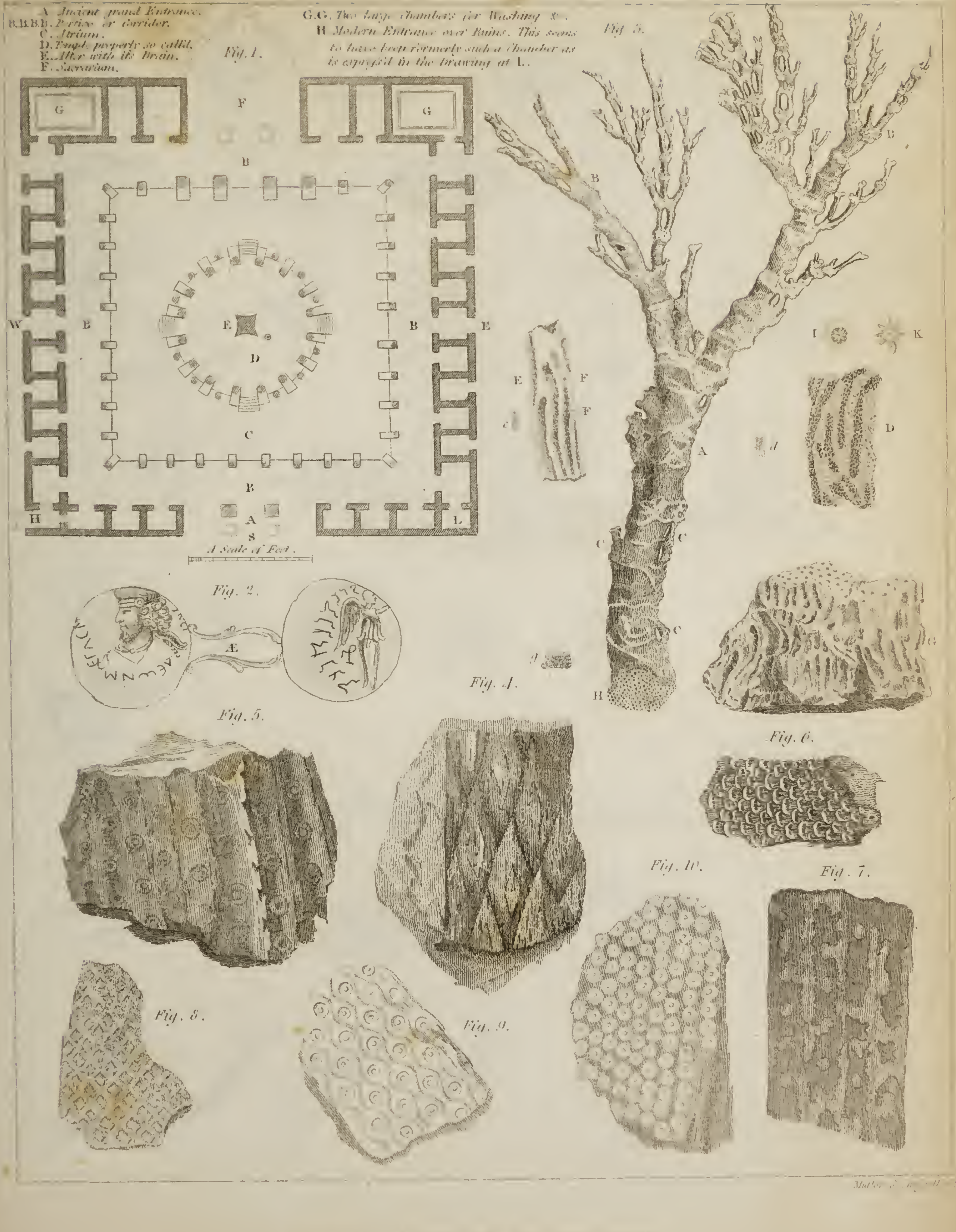

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Mutlow Sc. Russell del.

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Fossils, Fruits &c. found in Shepy Island, described p. 168.

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Fig. 1.
p. 136.

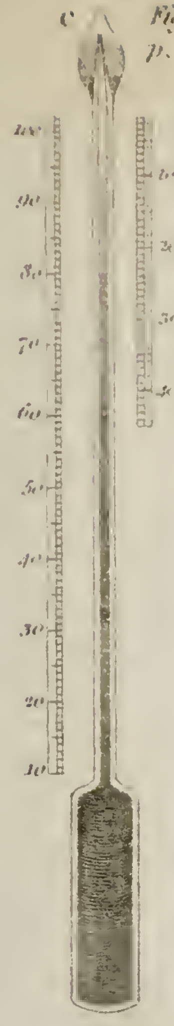

Fig. 2.
p. 140.

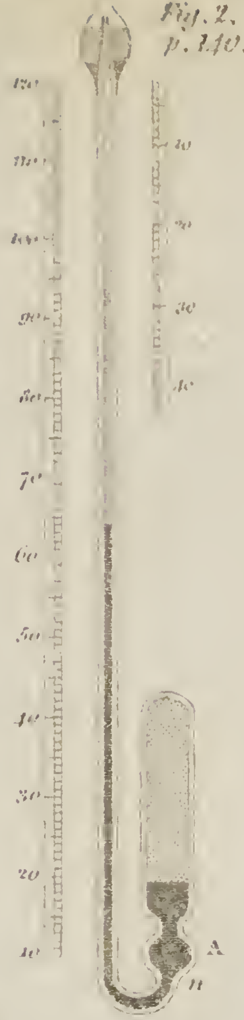

Fig. 3.
p. 140.

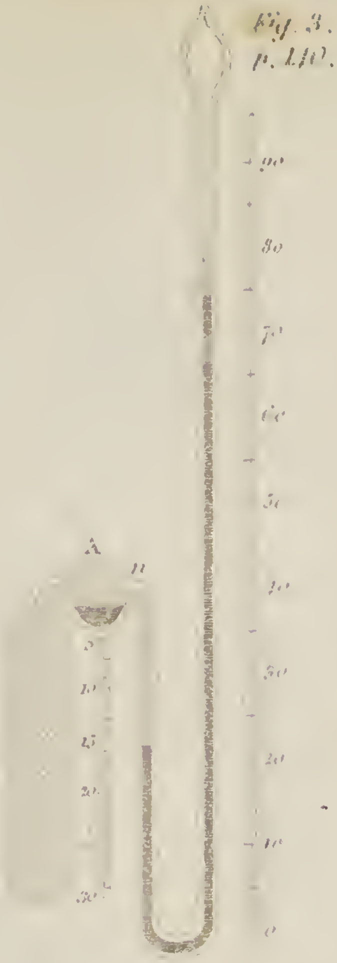

Fig. 4.
p. 141.

Fig. 5.
p. 184.

Rhus Sinense foliis alatis foliolis oblongis Acuminatis ad basin subrotundis et dentatis.

Fig. 6.
p. 186.

Fasi-noki.

Toxicodendron foliis alatis fructu Rhomboide H. Elth. from Japan.

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Fig. 1.
p. 216.

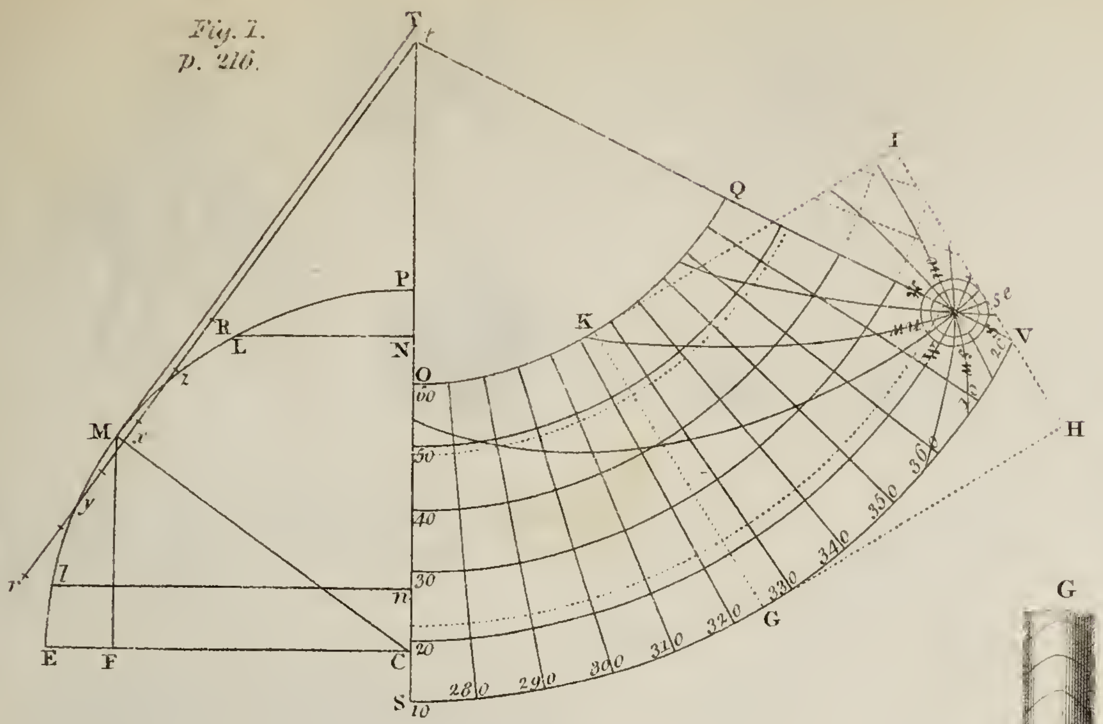
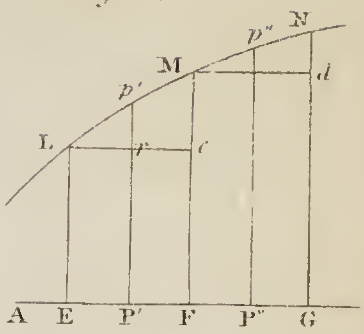


Fig. 2.



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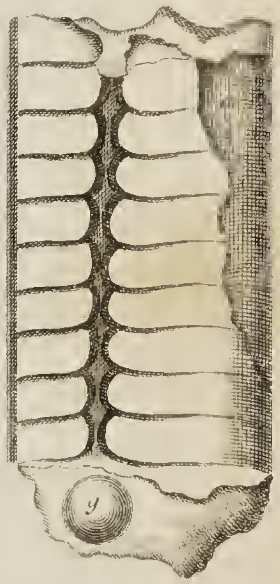

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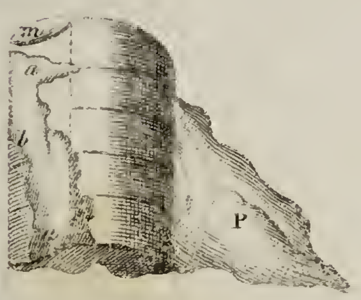
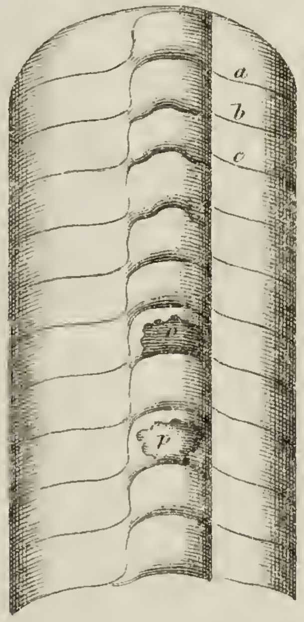


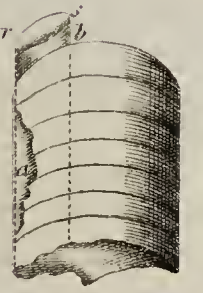
F



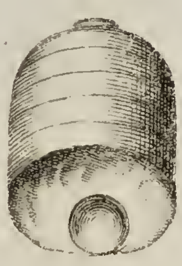
H

I

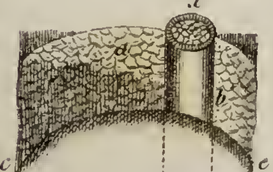
B



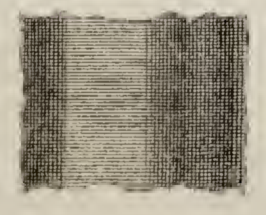
B C



D



C



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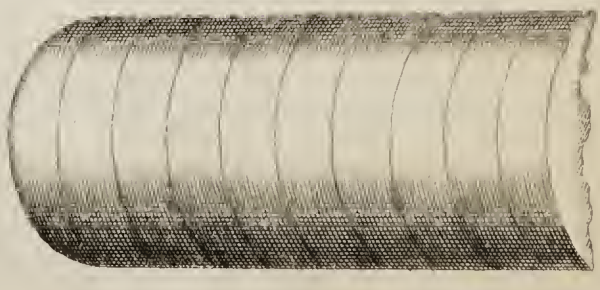

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Fig. 4.

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Fig. 2.

Fig. 3.

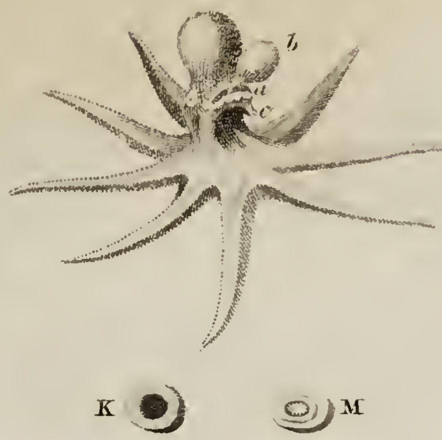


Fig. 7.

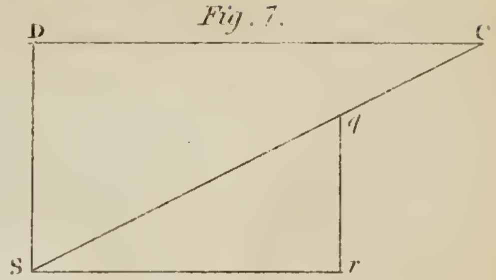
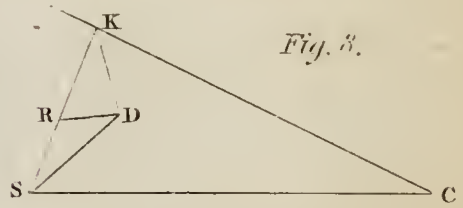


Fig. 8.



Part of the Fossil Skeleton of an Animal as it appeared on and united to the Allom Rock near Whitby Jan.y 3.d 1758.

a. a. &c. The Ammonitæ or Snake Stones.

Fig. 5.

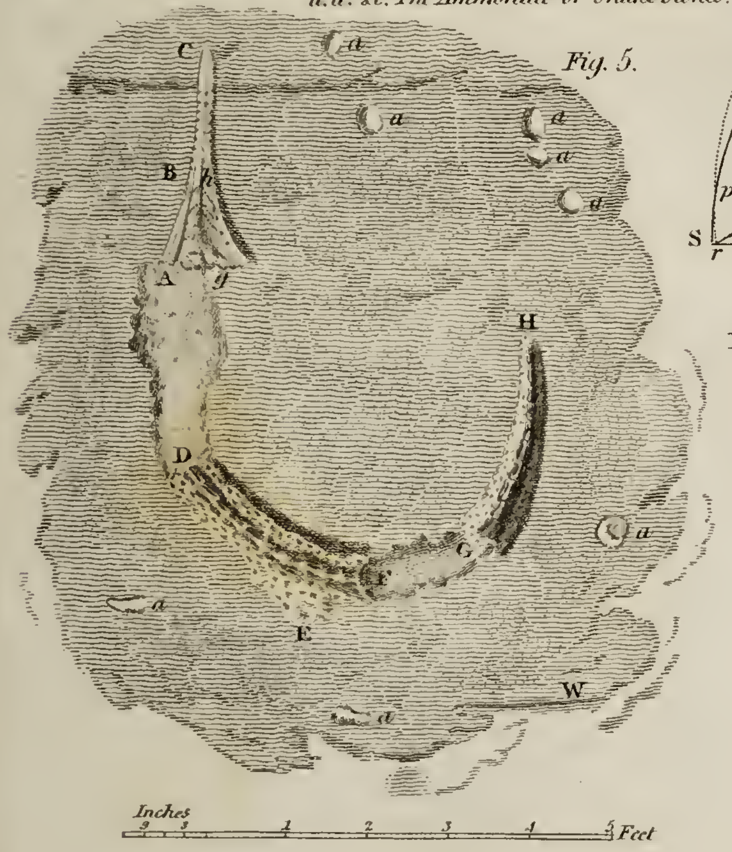


Fig. 9.

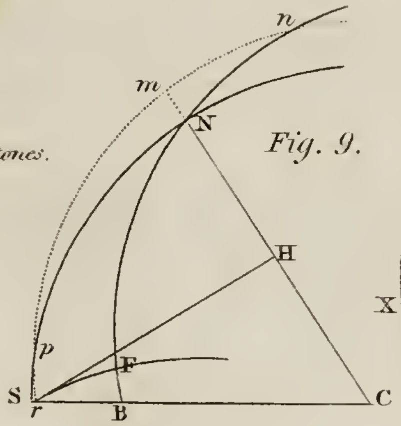


Fig. 12.

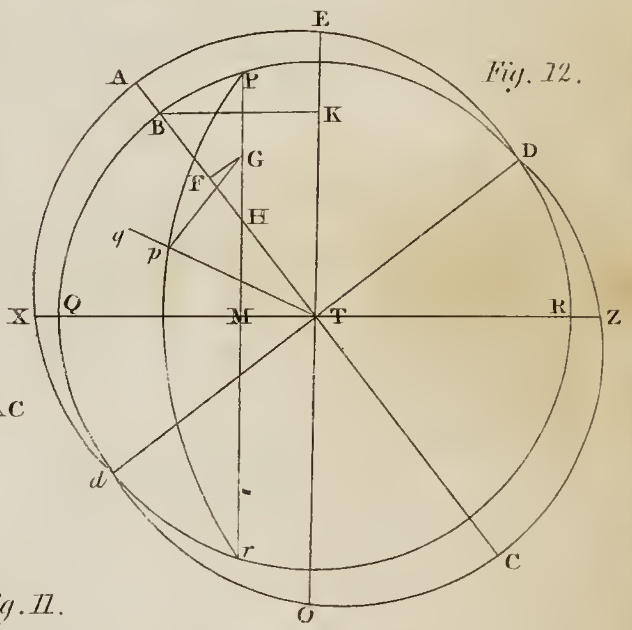


Fig. 11.

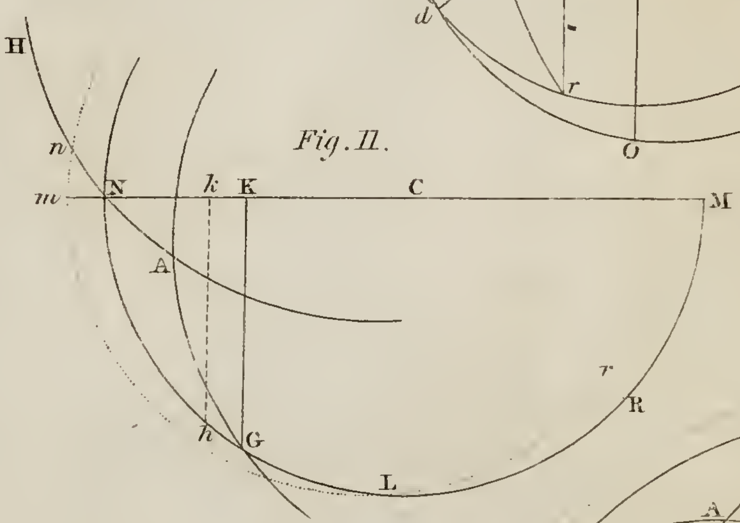


Fig. 10.

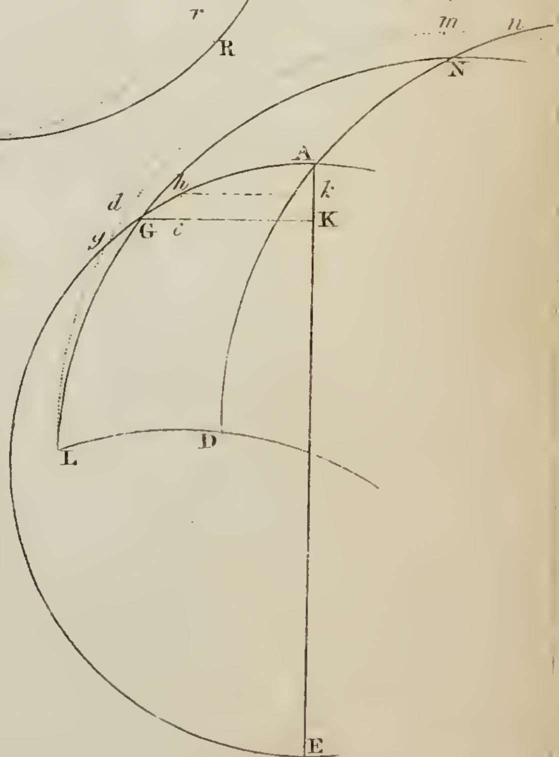
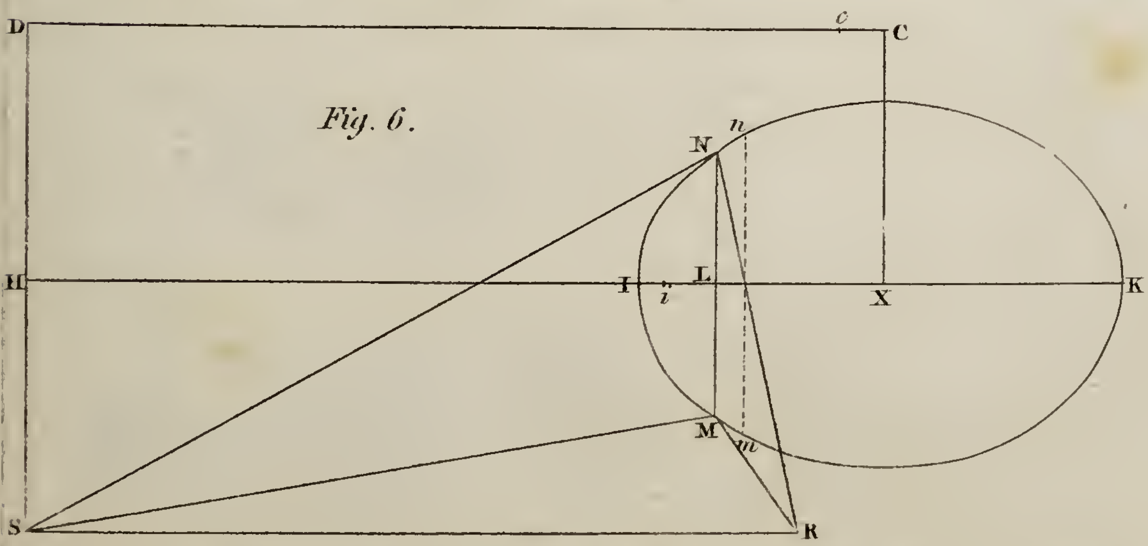


Fig. 6.



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PHŒNICIAN COINS.

Fig. 2.

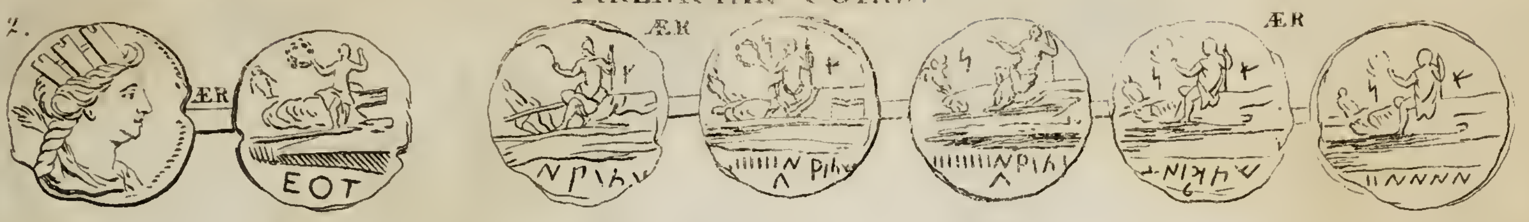


Fig. 3.

Fig. 1.

Fig. 4.

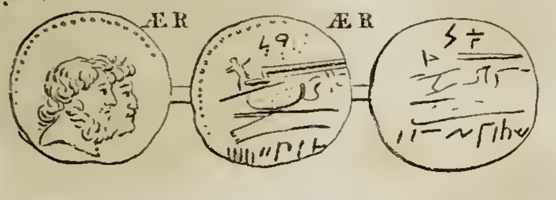


Fig. 5.

PHŒNICIAN Numerals antiently used at SIDON. from One to a Thousand.

Fig. 6.

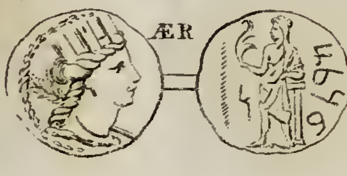


Fig. 7.

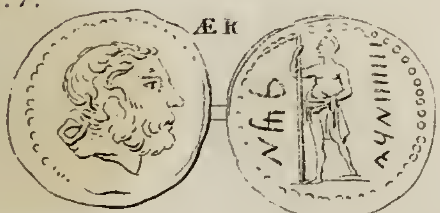


Fig. 8.

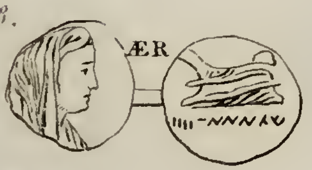


Fig. 9.

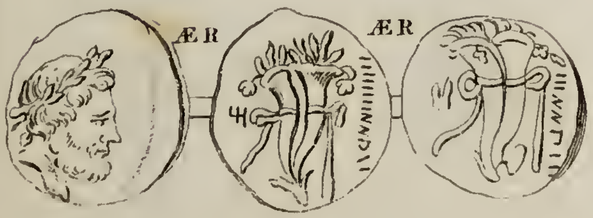
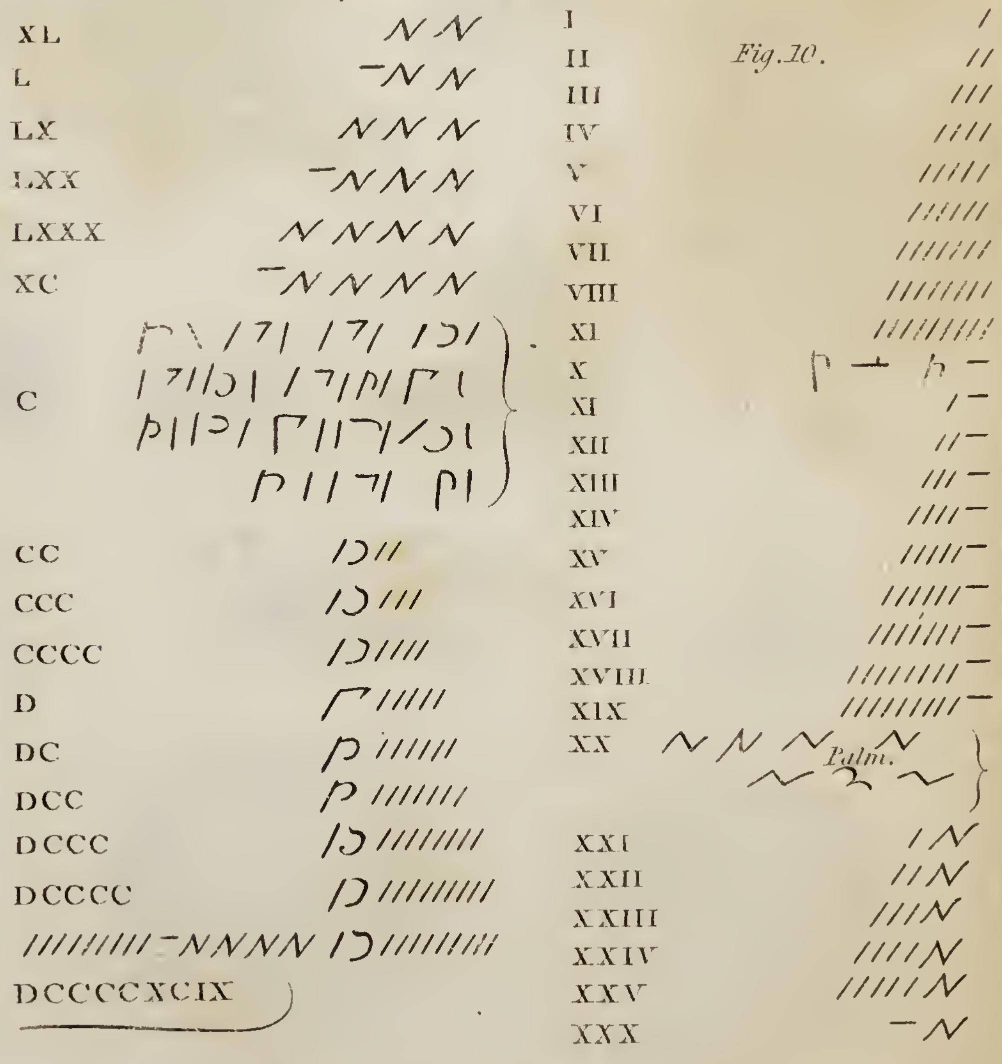


Fig. 10.



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Rare Species of Barnacles.

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The Cinnamon, reduced to one half.

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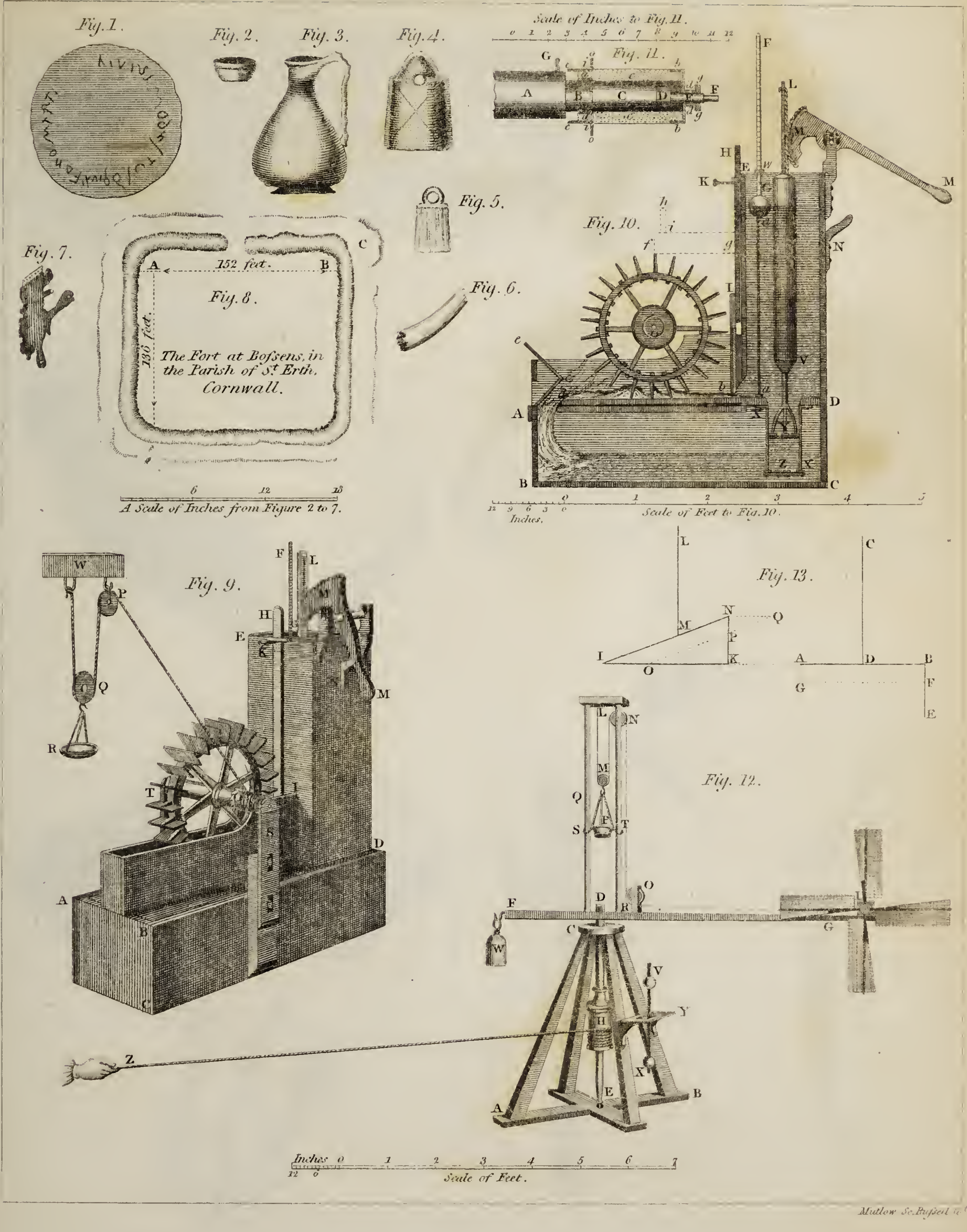

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Fig. 1.

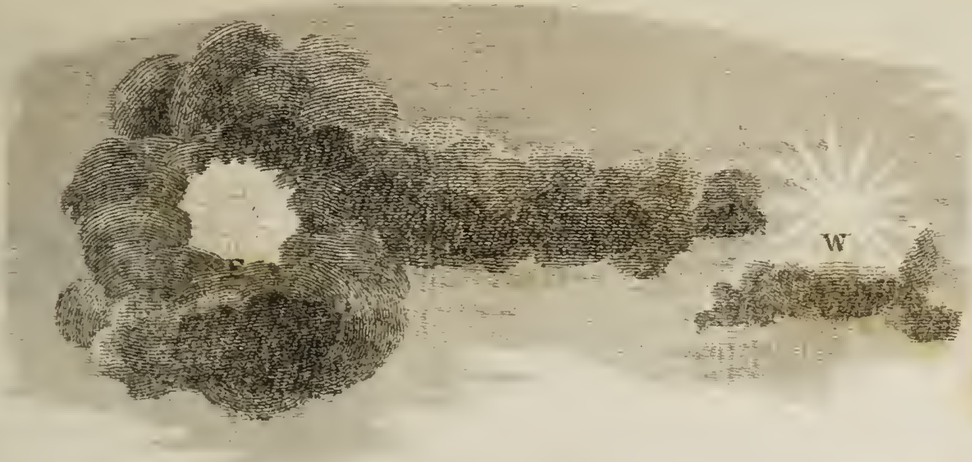
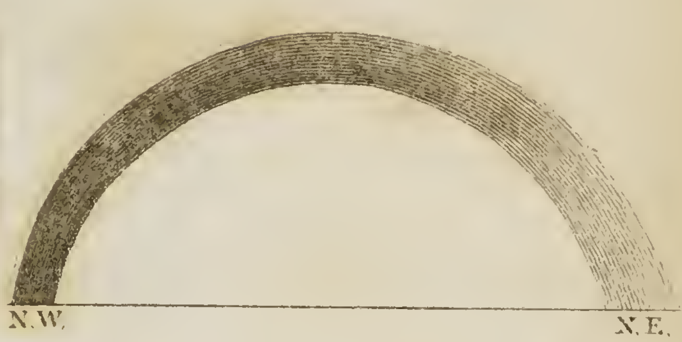


Fig. 2.



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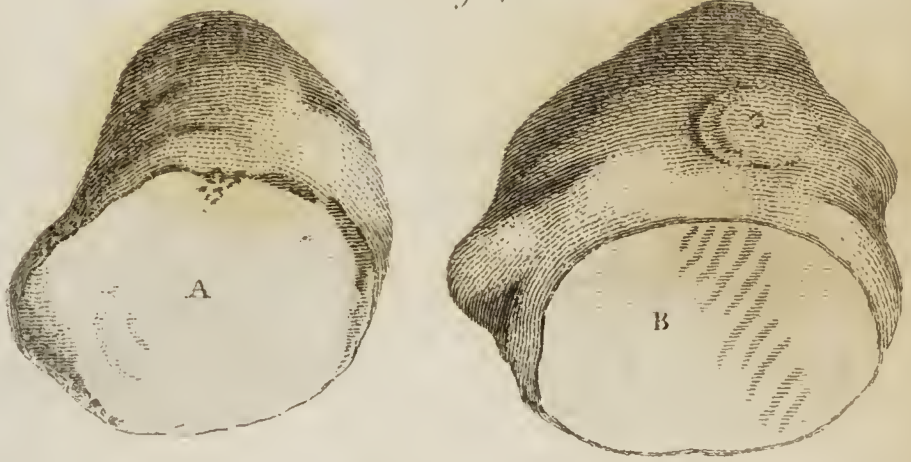

Fig. 5.

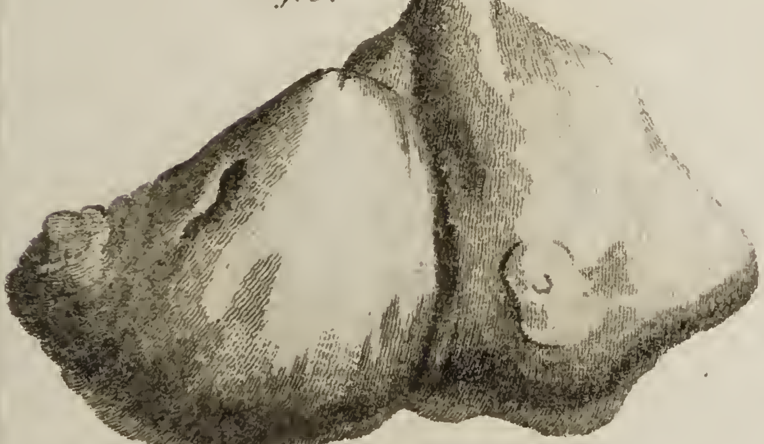

Fig. 7.

Fig. 6.

Fig. 8.

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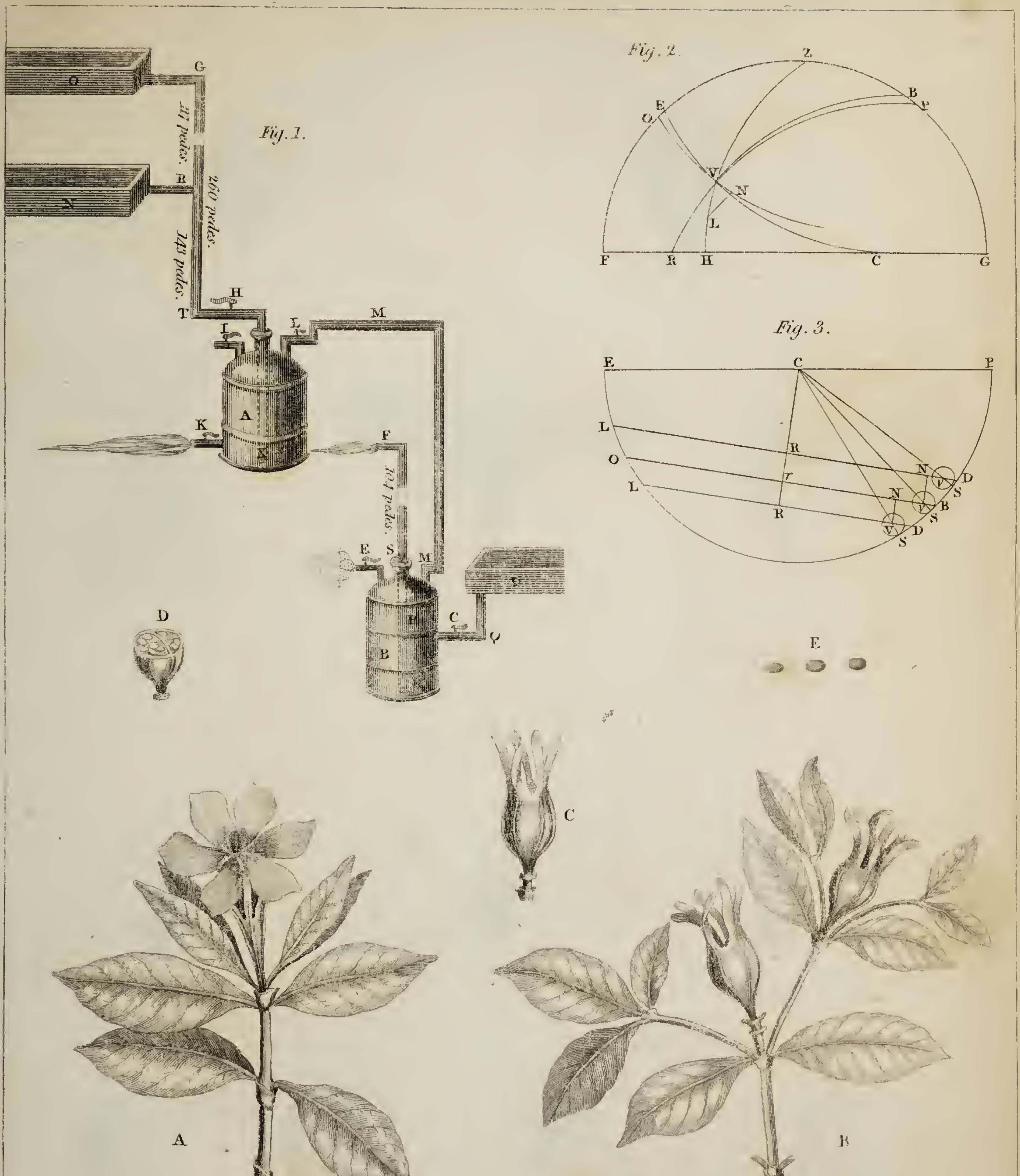

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The Cochineal Insects.

Mutlow Sc. Russ. Ct.

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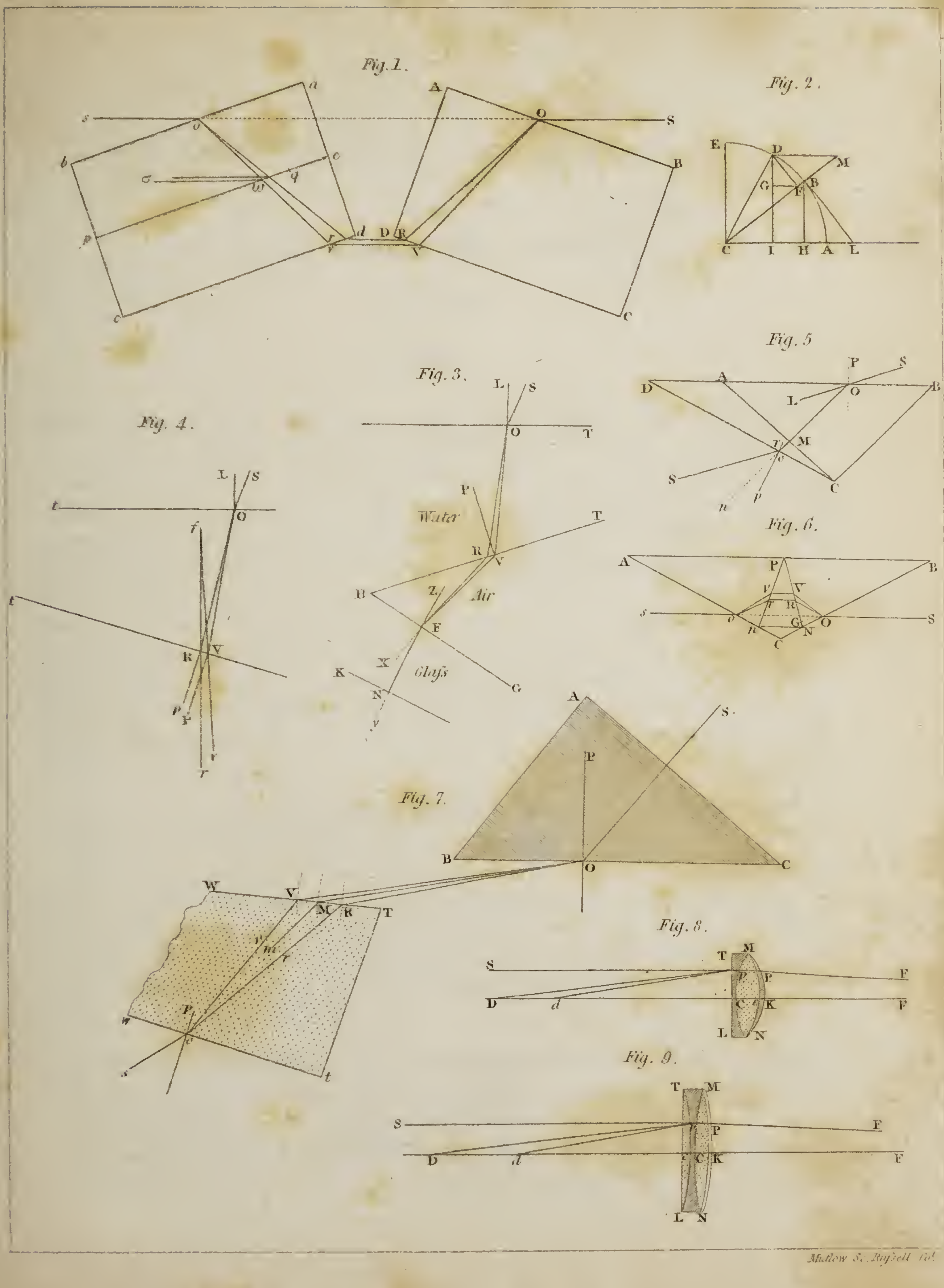

Mutlow Sc. Russell Ct.

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THE
PHILOSOPHICAL TRANSACTIONS
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ABRIDGED.

XCVI. Of an Unusual Agitation of the Sea, at Ildfracombe, in Devonshire, Feb. 27, 1756. By the Rev. Mr. Prince of Barnstable. p. 642.

On Friday, Feb. 27, 1756, at 6 in the evening, the weather being then extremely fair, as it had been for some time before, and continued for some days afterwards, the sea being exceedingly calm, a rumbling noise was heard like that which usually precedes what the sailors call a ground sea, only it was much louder. The tide, at that time, was above half ebbed, and retired as far as the head of the key, leaving the vessels within the pier on dry ground; when on a sudden the sea came on with a great run, filling the quay to the height of 6 feet perpendicular; and the water remained at the same height near half an hour, but was all the time agitated as in a storm. The like phenomenon also happened Nov. 1st, last year, and the waters then rose to the same perpendicular height.

XCVII. Extract of a Letter from the Rev. Mr. Holdsworth, at Dartmouth, relating to the Agitation of the Waters observed there Nov. 1, 1755. p. 643.

There was a surprizing agitation in the waters about 9 in the morning of Nov. 1, 1755, when there was a great and sudden swell, though there was but little wind. During this fermentation, though it was 4 hours ebb, the waters rose as high, or higher than they usually do on the highest spring tide. This violent motion lasted about $\frac{3}{4}$ of an hour, and then the waters fell to their usual height at that time of the tide.

XCVIII. A Method of Observing the Wonderful Configurations of the Smallest Shining Particles of Snow, with several Figures of them. By John Nettis, M. D., Middleburg, &c. Translated from the Latin. p. 644.

The weather being intensely cold in the year 1740, the snow which fell was

hard, entire, and pellucid, and some particles being received on a pencil, were placed on a plane glass plate under the object glass of the best microscopes: the greatest care was taken that the smallest particles might not be dissolved, either by the breath or perspiration of the hands, lest the little angles might by the least degree of warmth disappear. And thus, with this apparatus and these precautions, the extreme exactness and equality of the figures of their most minute particles might be observed and delineated.

Some consisted of long round spiculæ; others approached to a round figure made up of small globules; but these were observed to be opaque, as the air was disposed to thaw; but when the air was frosty, many slender hexangular figures appeared, some of equal, others of unequal sides; such as are exhibited by Scheuchzer in his Herbarium Diluvianum, and by Swedenburg in his Prodromus Principiorum, p. 21; and such as I have seen in a pitcher, which was covered, in which the water was frozen; and such figures of the concretions of vitriol, salts, &c. as may be seen in the works of Leuwhenhoek, whom I find to be the most faithful and expert in delineating and describing the minutest natural bodies; and also such as are published by Capellar in his Prodromus Crystallographiæ.

Several little stars seemed to consist of 6 oblong, round, hexangular lamellæ, or indeed of 6 rays terminating in points; which little stars appeared to be formed of 6 plane rhomboidal particles. Several plane hexangular particles of equal sides, or oblong hexangulars, adhered to several of these stars, either at their extremities, or at each side of every ray. Some hexangular lamellæ of equal sides were adorned all round with 6 other lamellæ of the same figure and size, or with hexangular oblong lamellæ, and to these sometimes adhered several others, more or less. Many of these hexangulars were ornamented with 6 rays; and to these were fixed the most slender lamellæ, which were also hexangular, of equal or unequal sides: but of equal angles of 60 degrees; and to these lamellæ others like them adhered, some greater and some less, but most of the latter; and various others like the fortifications of cities appeared to be joined to long hexangular spiculæ, and plane hexangles of equal sides. In one day and night he found 15, 20, or more particles of snow differently formed; such as Olaus Magnus mentions; and in the year 1740, on the 11th, 12th, 13th, 21st, and 23d of January, and also on the 6th, 23d, and 24th of February, he had an opportunity of delineating 80 different admirable figures of snow, and of observing their numberless varieties.

And though a vast variety of these configurations of snow may fail or vanish in the same moment, yet the smaller particles, from their various combinations with each other, constituting this wonderful variety of configurations of the snow, were observed by him to be comprehended under these following forms; viz. of parallelograms, or oblong, straight, or oblique quadrangles, rhombs, rhomboids, trapezia, or of hexangular forms of equal or unequal sides, whose

angles are 60 degrees: and these hexangular particles were far more numerous than those of any other form.

The natural size of most of the shining quadrangular particles, and of the little stars of snow, as well the simple as the less compound ones, does not exceed the 20th part of an inch: nor do the more compound particles the 5th of an inch. For the natural magnitude, or rather smallness, see pl. 1, where these beautiful various configurations are exhibited, to the number of 91. N° 57 and 84 are anomalous figures of snow; of which there is an infinite variety, that may be observed.

XCIX. Of the Copper-springs lately discovered in Pennsylvania. By John Rutty, M.D. of Dublin. p. 648.

In the province of Pennsylvania is a copper mine, which affords a spring that appears to have the same qualities as that Irish water lately described by Dr. Wm. Henry and Dr. Bond, in the 47th and 48th volumes of the Philos. Trans., but is much sharper, for it will dissolve iron in a quarter part of the time; and we are assured, by the accounts transmitted from the proprietors of it, of the trials they have made, that it yields the same copper-mud or dust as our Cronebaun-water, of the county of Wicklow, which being collected from bars of iron immersed in it, for the purpose of extracting the copper from the Pennsylvania water, it produced above half pure copper on being melted in a crucible; an experiment that requires to be repeated in order to ascertain the proportion of copper contained with accuracy; our copper-spring of the county of Wicklow yielding a proportion considerably larger than this, viz. 16 parts of copper out of 20 of the mud.

In the neighbourhood is a great abundance of the ores of vitriol and sulphur, and the spring comes through an immense body of vitriol-ore, and the supply of water is very large, 700 or 800 hogsheads flowing in 24 hours.

The water is of a pale-green colour, of an acid, sweet, austere, inky and nauseous taste. It is very ponderous, and instantly betrays the great strength of the metallic impregnation by the hydrometer; which, immersed in this water, presently mounted above the ball, and stood in it nearly at the same height as in a solution of one oz. and six drams of English vitriol in a quart of water. A little of the solution of pot-ashes instantly precipitates the metallic parts of this water, in grains of 3 different colours, viz. ochre-coloured at the top, green in the middle, and white at the bottom: and the appearances with spirit of hartshorn were much alike, except that the grumes at the bottom participated of a mixture of a blue colour with the white, indicating more clearly the mixture of copper.

But iron immersed, above all other things, renders the contained copper conspicuous to the eye; for a clean knife, kept in it a few minutes, is covered with

a bright copper-colour; and needles and nails kept immersed in it a month in a phial are covered with a rust, partly yellow and shining, which seems to be the copper, and partly a ferrugineous matter, as appeared by the magnet: and that it was partly cupreous appeared by the bright blue tincture extracted by spirit of hartshorn from such parts of the rust as did not readily fly to the magnet; and, if one might rely on the Philadelphia experiment above-mentioned, the proportion of copper should be very large.

It is however certain, that as in other copper-springs, so in this, here is a very considerable proportion of the vitriol of iron combined with it, and by all experiments a much greater than of the vitriol of copper; and accordingly, galls added to this water turned it first blue (the characteristic of martial vitriol) and then of a dilute ink-colour; and the corks in the bottles were blackened. But the genuine quality, as well as a large proportion, of the impregnating salt, will further appear by the following analysis of this water, viz. a pint of it, exhaled by a slow fire, left 400 grains of solid contents, which were partly green and partly ochre-coloured, with an intermixture of bluish, and of a rough, sweetish taste, like that of sal martis, and appeared to be chiefly saline, not leaving above 4 grains of indissoluble matter on dissolving 196 grains of it, and filtring.

Thus it appears, that the proportion of vitriolic parts in this water is very large, viz. above 6 drams to a pint or 3200 grains to a gallon; and consequently it is a stronger solution of vitriol than sea-water is of marine salt; and is considerably the strongest of all the vitriolic waters, that have yet occurred to my observation; for our Cronebaun water, in the county of Wicklow, gives but 256 grains from a gallon; Haigh in Lancashire, (the strongest in Britain, that I know of) 1920 grains; Shadwell 1320; Kilbrew, in the county of Meath, 1530 from the same quantity; so that besides the copper to be obtained by immersing bars of iron, as in our county of Wicklow water, this water offers to its proprietors another peculiar advantage, viz. an opportunity of erecting a copperas-work or manufacture of vitriol, like the Hungarian vitriol; especially the vast supply of water and plenty of fuel in the place considered.

C. Extract of a Letter from the Abbé Mazeas, F.R.S. concerning an Ancient Method of Painting, Revived by Count Caylus. Translated from the French by James Parsons, M. D., F. R. S.* p. 652.

The Count de Caylus, a member of the Academy of Inscriptions, had under-

* A celebrated French antiquary, author of a splendid work entitled Antiquités Egyptiennes, Etrusques, Grecques, Romaines et Gauloises, in 7 vols. 4to. The last vol. contains an éloge of the author, by Mons. le Beau. He was a great encourager of artists, and wrote the Lives of several Painters and Engravers to the French Academy. In addition to the works just mentioned, there are many learned dissertations by this author in the Memoirs of the Academy of Inscriptions. Count Caylus died in 1765, in the 73d year of his age.

taken to explain an obscure passage in Pliny the naturalist. This author says in some place of his works, that "the ancients painted with burnt wax;" and we have it from tradition, that pictures of this kind were very durable. This was the passage, that the count undertook to clear up, by trying all the different ways possible to paint in wax; and after many experiments he hit upon a very simple method, of which he made a secret, in order to excite the curiosity of the public. For that time he only thought proper to show one picture at the Louvre, representing the head of Minerva, painted in the manner of the ancients, and which was much admired.

The several artists, who were desirous of knowing by what means the count came to make this discovery, made several attempts themselves; but in a great number of trials, only 2 are worth mentioning.

The first was to melt wax and oil of turpentine together, and use it for mixing the colours. But this method does not at all explain Pliny's meaning, because wax is not burnt in this way of managing it: and besides, this method has two defects; the oil of turpentine dries too fast, and does not allow the painter sufficient time to blend and unite his colours. The 2d method is very ingenious, and seems to come up to Pliny's notion very well: it is as follows: the wax is melted with strong lixivium of salt of tartar, and with this the colours are ground. When the picture is finished, it is gradually put to the fire, which increases the heat by degrees; the wax melts, swells, and is bloated up upon the picture: then the picture is removed gradually from the fire, and the colours do not at all appear to have been disordered: the colours then become unalterable by the action of the air, and even spirit of wine has been burnt upon them without doing them the least harm.

However, the following is the Count de Caylus's method, which is much more simple; according to which the head of Minerva was painted, which was so much admired by all the connoisseurs. 1st. The cloth or wood, designed for the picture, is waxed over, by only rubbing it simply with a piece of bees-wax. 2dly, The colours are mixed up with common water; but as these colours will not adhere to the wax, the whole picture is to be first rubbed over with the Spanish white, and then the colours are used. 3dly, When the picture is dry, it is put near the fire, by which the wax melts, and absorbs all the colours.

The effect produced by these colours upon wax is very singular; nor can one have any notion of it without seeing it. The colours have not that natural varnish or shining that they acquire with oil; but you are capable of seeing the picture in any light, or in whatever situation you place it: in short there can be no false glare or light on the picture for the spectators: the colours are secured, are firm, and will bear washing; and have a property, that they have smoked this picture in places subject to foul vapours, and to smoke in chimnies; and then by being exposed to the dew, it became as clean as if it had been but just painted.

CI. Observations on the Abbé Mazeas's Letter on the Count de Caylus's Method of Imitating the Ancient Painting in Burnt Wax. By James Parsons, M. D., F. R. S. p. 655.

The subject of the Abbé Mazeas's letter, concerning what he thinks the encaustic painting in burnt wax, is very difficult to understand; for though the count de Caylus has made an essay to find out the method of the ancients in that kind of painting, his success in the head of Minerva mentioned in the Abbé's letter, does not seem to explain Pliny's meaning. This author is so very short and obscure in most things, that a bare literal translation of some parts of his work would hardly be reconcileable to sense; and this is no where more evident than in this very subject.

The two principal methods tried at Paris cannot be called burning in wax, nor be counted encaustic painting; unless uro, or the Greek καιω, could signify to liquify as well as to burn, in which sense I never met them any where. And if these words mean only to burn, then encaustic painting can signify no more nor less than painting in enamel; in which wax, from its very nature, can have no share. And yet at the end of the 11th chapter of his 35th book, he seems to give uro another meaning: he is admiring the wonderful effects produced in dying stuffs, which being first scowered, are laid over with some colourless material, in whatever pattern they choose; and on being dipped in a caldron of boiling liquor, the stuffs appeared to be finely and variously painted; "Cortina pingit dum coquit; et adustæ vestes firmiores sunt, quam si non urerenter." Here uro must signify to boil; for we cannot say the burnt stuffs were become stronger, than if they had not been burnt.

In the same book he has these words:

"It appears, that anciently there were two kinds of encaustic painting, in wax, and in ivory, with a stilus; until ships began to be painted: then this third kind came up of using a brush or pencil, with wax melted by fire, &c." Now though Pliny uses the word pingendi in the first two, we cannot understand that he could mean the laying on of paint, since the instrument (the cestrum) being pointed, is incapable of such an office; and secondly, because he immediately mentions a third kind of painting distinct from, and an absolute contrast to the other two, which the paint with the melted wax was laid on with a brush; and this contrast is very strong in another passage in the same chapter, where he speaks of a famous virgin called Lala, of whom he says, "Romæ et penicillo pinxit, et cestro in ebore, imagines mulierum maxume." That is she painted at Rome with a pencil, and with a cestrum or stilus on ivory, chiefly the images or portraits of women. We cannot help thinking, that what was done with the cestrum, either on the wax or ivory, was modelling or carving; for the modellers of this day, in their compositions of wax and other materials, use pointed tools to

repair and render their figures sharp ; and the workers in ivory use such tools of various points and edges for the same purpose.

Dr. P. therefore thinks, that when Pliny mentions cera in the singular number, though he says pingere, yet as the cestrum is mentioned with it, it must be understood to mean carving or modelling; but that when it is in the plural, and of burning the picture, he must mean the true encaustic or enamel painting, and the ceris must mean a composition, which was capable of enduring the fire; for which, perhaps, the following short reasons may have some weight.

It appears in the 2d chapter of his 35th book, where Pliny is speaking of the honos imaginum, that modelling was greatly practised, especially the busts of great men, and of very ancient standing. These were made during the lives of the persons, and laid up in their armories, or other repositories, till their deaths, in order to be carried before the deceased in their funeral rites, and exposed to the public, while an oration was made by the nearest of kin, who pointed to the image, as he proceeded, in his elogium on the virtues of the person represented; and this image was modelled in wax, as our wax-work is made at this day, and painted in natural colours, in order to come the nearer to nature. Pliny's words are very clear in this; " expressi cera voltus singulis disponebantur armariis, ut essent imagines, quæ comiterentur gentilitia funera, &c." And it is also evident, that in order to take the true resemblance of the persons, whose busts they intended to make for these purposes, they took off a plaster mask from the face, and by way of mould cast melted wax into it; by which they obtained every feature, and afterwards made it perfect by repairing with proper tools. This is fully declared in his 12th chapter of the same book, which treats of plastics: where, after he has mentioned Dibutades, a potter of Sicyon, as the first inventor of forming the likeness of things in clay or plaster, and of first making images on the corners of his tiles, he gives the invention of taking off masks from the face, for making busts, to Lisistratus, of the same town, brother of Lysippus.

In short, they appear, in the sequel of this chapter, to have imitated fruits, fishes, and every thing else, by making clay moulds, and casting the wax or other matter into them. It is, by the way, remarkable, that in all these cases of casting or modelling, cera is in the singular number, and must be taken in its literal sense, as being a matter very capable of such a manufacture.

Now on the other hand, when that word is in the plural, there is some reason to conjecture, that a certain composition is meant, capable of bearing the fire, or when it is laid on ships with a brush; for we can neither suppose that wax was ever capable simply to bear being burnt, as the encausticæ picturæ expresses and denotes it; nor that the ceris igni resolutis was to be simply laid on their ships without paint, rosin, turpentine, or some other matters, both to render it ductile and fluid enough not to clog the brush as it cooled, which every one must

allow wax would infallibly do; and also to give it such a body, as that, when dry, it might stand the injuries of the weather; for the heat of the sun would melt simple wax, and make it run down in streams, without an admixtion of something else to give it the necessary firmness.

That the ancients were well acquainted with enamel painting cannot be doubted, since there are great numbers of their enamel pieces in the cabinets of the curious in many places. There is one, which is a Roman cup curiously enamelled on brass, found at Froxfield, in the possession of Lord Hertford; there is a Roman enamelled platter on the same metal, probably belonging to the cup, with figures and inscriptions curiously painted in the enamel, of Leg. ii, Aug. and Leg. xx.vv. in Britain, a drawing of which Dr. Stukely made in its colours (see Buonoroti's Osservazzioni on the Duke of Tuscany's Medallions). And the Doctor has now an enamelled fibula of the same kind of workmanship. Nor are there wanting cups with portraits of some friends enamelled at the bottoms, which were used inter pocula, to drink to their memories; and it is probable, that the enamelled ware of cups, platters, ewers, and such like, which the great Raphael was concerned in making, many of which are now in England, were made in imitation of the ancients; since in every other part of his art, he was so close a follower of their most correct works, and since the colours and appearance are exactly the same in his, that are on those ancient pieces mentioned.

CII. Of the late Earthquakes felt at Maestricht, in a Letter from Mons. Vernede, Pastor of the Wallon Church there. Translated from the French. p. 663.

The number of shocks has been very considerable here. From the 18th of February to the beginning of April, 1756, no day passed in which one was not felt, often more. Mons. Hoffman has remarked about 80 distinct ones.

There were even some successive hours, in which the earth was scarcely at all quiet; but these were only tremors. The strongest shock was that of the 18th of February. It continued about a minute and a half. The next in degree to this was that on the day after Christmas day, which lasted about a minute. There were some others no less violent, but of much less duration.

All the shocks were not of the same kind. The motion was undulatory in those of the 26th of December, and 18th of February; but the undulations on the former of these days were longer than those on the latter. At other times there were observed only a rising and sinking again, and most commonly a shaking on one side. During the most violent shakings there were some kind of flashes of lightning. The whole was preceded by a groaning under-ground, which, when the shocks were weakest, seemed most like the noise of a cart deeply loaded, heard at a distance; and when they were strongest, to that of a

coach rolling swiftly under the place. These groanings were also sometimes heard where they were not followed by any sensible shocks. These shocks have happened in all kinds of weather, dry, rainy, cold, &c. only it was always remarked, that it was calm at the time, and the wind rose afterwards. No hours have been exempt from them.

As to the consequences of these earthquakes: they were not at all fatal here. The consternation was very great. Several persons felt very singular motions, which they compared to the great electrical shock. Afterwards, they imagined every moment that there were new ones. The rattling of glass windows was the least ambiguous sign. China fell down from the chimney shelves. The dishes in kitchens struck against each other. Some chimneys were thrown down. Several walls cracked, and some arched roofs were damaged. As to the effect in the mines of Houille, in the country of Leige: In a mine of 900 feet depth, the workmen were sitting at breakfast on the 18th of February. Of a sudden they were pushed violently against each other, so that they thought that some of them were at play; but seeing that those who sat alone were shaken in the same manner, they ran to ring the alarm bell. The overseer called out to them from above, that it was an earthquake, from which they had no reason to be under any apprehensions. On the same day, Feb. 18, there was an extraordinary motion in our waters, particularly in the Meuse, which was agitated as if by a whirlwind; and the Jaur, a small river, which runs through our city, and was full before the earthquake, sunk very low immediately after. In some places the waters of wells were troubled; but they were not so with us.

CIII. On the Agitation of the Sea at Antigua, Nov. 1, 1755. By Capt. Affleck of the Advice Man of War. p. 668.

The year was ushered in here by the shocks of an earthquake, but not violent enough to do any damage. On the 1st of November last, you had a remarkably sudden flux and reflux of the sea at Portsmouth, and other parts of the coast, which was agitated in like manner, at the same time, on the coast of America, and all these islands. The tide rose here 12 feet perpendicular several times, and returned almost immediately; the same at Barbadoes. At Martinique, and most of the French islands, it overflowed the low land, and returned quickly to its former boundaries. The people at Barbadoes were never more astonished; the rising water in Carlisle bay appearing as black as ink, instead of the clear sea green. Here it began at half an hour after 3 in the afternoon, on the 1st of November last, and flowed, every 5 minutes, 5 feet perpendicular, till as much after 6, without any violent disturbance on the surface of the water. In Martinique, in that remarkable flux and reflux of the sea, it was in some places dry for a mile; and in others flowed into the upper rooms of the houses, and

destroyed much coffee. At the island of Sabia, it flowed 21 feet; and at St. Martin's, a sloop, that rode at anchor in 15 feet water, was laid dry on her broadside.

CIV. Of a Remarkable Fossil. By Edward Wright, M. D. p. 670.

This fossil Dr. W. discovered in a marble table, in an inn at Ghent. This table, the landlord said he purchased at the sale of an ancient family in the neighbourhood, and that he believed the marble was of this country, though he could not be certain. The fossil is what is called by naturalists orthoceratites,* and is one of those which he thinks is never found in its recent state, and are very rare in England. This was by much the largest he ever saw, and exceeds by many inches the longest he has read of. So as we can plainly trace it; it measures 2 feet $4\frac{7}{10}$ inches in length. It has originally been several inches longer, as may easily be traced out by continuing the straight lines, which terminate its edges, till they meet in a point. These shells are of the concamerated kind, and in this 66 partitions may be distinctly counted, and it must certainly have had a considerable number more, which are hid by the end part being immersed too deep in the marble. The marble, in which it is immersed, is of a coarse grain, and of a dusky brown colour, interspersed with a dirty white: of this colour the shell itself is tinged, and all its concamerations filled with the stalactical matter of the marble. The concamerations or partitions of these fossils, resemble those of the nautili, though it would be very improper to give them that name, for this shell is never seen in the seas, nor caught at such depths, as we have had occasion to fathom or search; hence it can never be classed among the sailors; nor indeed does it seem at all proper for exercising that function, from its long narrow pointed shape, so very different from that boat-like figure requisite for sailing. Its concamerations seem principally intended for performing the motions necessary to the animal, at the bottom of the sea, and at greater depths, from which it does not seem ever to rise to any considerable height.

CV. Of the Orthoceratites. By Edward Wright, M. D. p. 672.

In this account Dr. Wright apologises for dissenting from some of the opinions of Buffon, and other naturalists, in some particulars. The orthoceratites, says he, is one of those shells which are never found in the recent state, and is to be classed among conchæ pelagiæ of the naturalists, which never approach the shore, but continue always at great depths of the sea, contrary to the littorales, which frequent the shores and shallow places; and hence, when found fossil, are easily to be matched with recent specimens.

* *Helmintholithus orthoceratites.* Linn.

Pelagian or ocean shells are frequently found fossil very near the surface, as every naturalist knows; which proves, that such places have formerly been the sea shore. Hence it is clear that the cause which transported them thither, acted suddenly; which agrees perfectly with the account of the deluge given by Moses in the holy scripture; and, at the same time, overturns the system of Mons. de Buffon, and the author of Telliamed, who pretend, that the earth was for many ages covered with water, and that in that long course of time it was, that the shells, which we now find fossil, were gradually produced; hence that they are to be considered as the remains of innumerable successive generations of marine bodies, formerly the only inhabitants of the globe. The greatest depths of the sea, as yet sounded, have been found to be about 3000 fathoms, and the ordinary depths are about 150; which makes it evident, that were the theories of these gentlemen true, such fossil shells ought never to be found at less depths in the earth than from 150 to 3000 fathoms.

Though fossil shells are to be found in almost all the plainer parts of the surface of the earth, yet there are certain very large tracts, where such bodies are never found, viz. the mountains, which seem to be the remains of the original strata of the earth. It is true indeed, that there are many eminences, which have been by modern theorists taken for mountains, where sea-shells, and calcareous matters of every kind are to be found in great abundance: but these are very inconsiderable, and only appear as little hillocks, compared with the large mountains, which contain mines, veins of metals, and precious stones, and may be traced in immense chains, without almost any discontinuity from one continent to another; and from continents to neighbouring and opposite islands, &c. insomuch that all these chains, not only of the old, but also of the new world seem connected one with another; an observation, which alone would indicate the importance of diligently inquiring into their structure, in order to form a true theory of the earth. Buffon and the author of Telliamed, who endeavour to prove that all mountains have been formed by sea-currents, and bring one of their principal arguments in proof of this opinion from marine bodies being found in great quantities in the strata of which they are composed, seem never to have made observations on mountains; else they might have observed this remarkable difference between them and the calcareous strata of the plains, that the former contain none of those marine bodies, though the latter are almost entirely made up of them. In the Alps, Appennines, and Pyreneans, no shells nor marine bodies of any kind are to be found; in the Ochels, a branch of the large Grampian mountains in Scotland, which Dr. W. had occasion diligently to examine, he could discover no marine bodies. The same is observed of all the large mountains of Africa, and of Asia; and in the huge chain of Cordilleres in Perou Mons. de la Condamine searched in vain for such bodies. This kind of

mountains, which indeed alone deserve that name, are chiefly composed of vitrifiable matter; and if they are sometimes found to contain sea-shells, it is never at great depths, nor in their original metallic or stony strata; though such bodies are found in great abundance at the foot of mountains, and in the adjacent valleys, in which there are many eminences in some parts continued in small chains, though but of small extent, which contain marble, sea-shells, chalk, and other calcinable matter, but never any veins of metal, though we frequently find in them pyrites, ochre, vitriols, and other minerals, which have been washed down from veins of iron and other metals, with which the higher mountains abound, and have afterwards been deposited in the calcareous strata of the valleys.

Buffon pretends, that all mountains have been formed by sea-currents; and a little afterwards tells us, that all sea-currents are occasioned by sea-mountains. Is it not natural here to ask, which of these two causes pre-existed? can such reasoning as this, a circulus viciosus of the grossest kind, ever tend to improve our knowledge, or give us just views of the works of the Creator? The learned academician founds his opinion of all mountains having been formed by sea-currents, principally on two observations. The first is, that they are made up of strata composed of sea-shells, and petrified marine bodies of different kinds; the 2d, that in chains of mountains the prominent angles always correspond with the depressed ones on the opposite side of the valley, in the same serpentine way as we observe in rivers, the banks of which are alternately hollowed and prominent, according to the different resistance they give to the current of the water. This observation was first made by Bourguet, and must be owned to be curious and interesting. Buffon is of opinion, that these two essential observations put together, form an invincible argument in proof of his theory, and such as could scarcely have been expected in so seemingly obscure a point. As to the first observation, that all mountains are made up of strata composed of marine bodies, it is so far from being true, that no mountains, properly so called, contain such bodies; and as to the 2d, of the correspondence of the opposite angles of mountainous tracts, it does not at all prove, as he would have it, that sea-currents have formed these mountains, but only that there have been formerly such currents running between them, which currents have given them that form we now observe them to have. To assert, that because currents of water have given them that figure, therefore they have produced them, is as ridiculous, as if one should say, that a river had reared its own banks, merely because it had given them a serpentine form.

Buffon, who pretends that the earth was at first entirely covered with water, which afterwards dug channels for itself, and thus separated the sea from the land; and the author of Telliamed, who endeavours to prove that this water goes

insensibly off by evaporation; and who, as well as Buffon, attributes the number of sea-shells, found fossil, to the length of time he supposes the now inhabited parts of the earth to have been covered with water, seem not to have given sufficient attention to an observation of consequence, which is, that the greatest part of our fossil-shells are entirely foreign to Europe, and belong to the equator or tropics. Buffon himself seems to have been somewhat aware, how much this observation might make against his theory; for he observes in answer to it, that not to mention such shell-fish as inhabit the bottom of the sea, and hence, being difficult to be caught, are regarded as unknown and foreign, though they may be produced in our seas; by comparing our fossil shells with their analogous living shell-fish, we shall find among them more shells belonging to our own coasts than of foreign ones; for example, that pectens, pectuncles, muscles, oysters, sea-glands, buccina, sea-ears, patellæ, &c. which we find fossil almost every where, are certainly productions of our own seas. But unluckily for our ingenious theorist, these shells, he mentions as common on our coasts, are produced in all the seas of the globe, and are equally inhabitants of the equator and poles; though we frequently discover fossil species of them, which are peculiar to the warmer climates.

Since then it is certain, that all our fossil shells are foreign to our climates, except such as are common to the whole globe, we may conclude, that Buffon's theory is in this respect absolutely defective. Besides, we find not only a very great quantity of fossil shells and other marine bodies, but also a great number of impressions of foreign plants, mostly of the capillary kind, on slates and other stones; and it is now certain that all the fossil wood of Loughneagh in Ireland (as in most other places where such wood is found) has been produced in a different climate; and, if he mistook not, had been compared and found to agree with recent specimens from America. Bones, and even entire skeletons of rhinoceroses, elephants, and other foreign land animals, are discovered pretty commonly through all Europe, and in Ireland very large horns of American moose-deer have been dug up. All these substances are commonly found near to, or in the same strata with, fossil shells, and other marine bodies; and all of them, whether original productions of sea or land, appear evidently to have been deposited in the places where we now find them, by one and the same cause. To account for these phenomena, Dr. W. thinks that Buffon must admit a universal deluge, such as is related in the holy scripture; and if a deluge of this kind is once admitted, why should we assign other causes for the transportation of marine and terrestrial bodies into climates foreign to those where they were produced? why, say Buffon and the author of Telliamed, because many thousands of years seem to have been requisite for the production of so immense a quantity of sea-shells, as those we find every where fossil; and besides, says the author of Tel-

lianied, there disposition is so regular, that it is plain the confusion of a deluge could never have placed them in such a manner. But as to the immense quantity of fossil shells, on which these gentlemen insist so much, that they have been misled by imagining that many parts of the surface of the earth contain marine bodies, which evidently do not; and these parts are, as before observed, the mountains properly so called, in the constituent strata of which no sea-shells nor marine bodies of any kind, no bones of land animals nor impressions of plants, are to be found. And as to the regular disposition of these bodies, this could not have happened in supposing a violent commotion of the waters to have continued the whole time they covered the earth. But is such a supposition natural or necessary?

The deluge must have produced very considerable changes on the surface of the earth. Many volcanos seem to have been formed at that time by the accumulation of animal, vegetable, and mineral substances, into huge masses, which have afterwards fermented and putrified, and in process of time burst out into flames. Earthquakes must have been frequent the first years after the deluge by the fermentation of these heterogeneous bodies, before the remains of so prodigious an inundation could be dissipated; for wherever there is any intestine commotion in the earths, its violence must be greatly increased, if it meets with water, and by its heat reduces it into vapour, which we know acts with an immense force.* That this must have been the case in the first years after the deluge, may be inferred from the abundance of moisture it must have left, and the fermentation of so great a quantity of heterogeneous substances buried in ruins by that memorable catastrophe. There are many observations which seem to prove that the earth, or at least many parts of its surface, have suffered by fire; not to mention the marks of it observed on many mineral substances. The artificial production of potter's earth or clay, is a very strong argument in support of this opinion. Potter's earth, as is well known, is found plentifully in most low grounds and vallies, between mountainous tracts, and where calcareous strata abound. By exposing common flint-stones to the confined vapour of boiling water in Papin's digester, a clay of the very same kind may be formed, and is no more than a decomposition of the flints. Hence it would appear, that wherever this clay is to be found, there the earth has undergone some violence from fire; and that this has been effected by earthquakes soon after the deluge seems extremely probable.

The deluge has given origin to many fossil substances, and produced many combinations, which otherwise would not have happened. Chalk is no more

* This seems to be the reason, why places situated on the sea-shore, or on large rivers, as was the unhappy city of Lisbon, suffer more from earthquakes than more inland situations, where such circumstances do not concur.—Orig.

than the ruins of sea-shells, and limestone consists of the same bodies cemented together by a stony juice. Amber appears evidently to be the resin of antediluvian trees (which are frequently found along with it at this day) united to the acid of sea-salt, which abounds in the earth. The reason of insects, straws, &c. being immersed in amber, absolutely inexplicable from the hypothesis of its being of mineral origin, is now no more a secret; for we know that nothing is more common, than to find such bodies immersed in the resin of trees. Fossil sea-salt, or salt-gem, seems to have been deposited in the quarries, whence it is dug, at the time of the deluge. All or most part of pit-coal appears to be of diluvian origin, for it gives a caput mortuum, the texture of which exactly resembles that of burnt wood. We may reasonably suppose large forests to have been buried at the time of the deluge, which have undergone a fermentation and putrefaction in the earth, so that the colour of the woody part has been changed, though the texture has remained entire enough to allow us to distinguish to what kingdom it belongs. All bitumens, pissasphaltum, pessilæum, &c. seem to be no more than productions of resinous substances united with mineral acids, which have caught fire in the earth by fermenting with heterogeneous matter, and have thus undergone a sort of natural distillation and exaltation. These are more than chimerical notions, and are even demonstrated by experiments; for amber can be produced artificially, as also bitumens by the distillation of resinous substances with mineral acids; and there is great probability that pit-coal might be imitated.

CVI. A Retractation, by Mr. Benjamin Wilson, F.R.S. of his former Opinion, concerning the Explication of the Leyden Experiment.* p. 682.

I think it necessary to retract an opinion concerning the explication of the Leyden experiment, which I troubled this Society with in the year 1746,† and afterwards published more at large in a Treatise on Electricity, in the year 1750; as I have lately made some further discoveries relative to that experiment, and the minus electricity of Mr. Franklin, which show I was then mistaken in my notions about it. What I mean by the minus electricity of Mr. Franklin regards the

* Mr. Wilson died June 6, 1788, at the age of 80 or upwards. It is said he had been formerly an eminent painter. For many years before his death he enjoyed the lucrative contract for the house, &c. painting under the Board of Ordnance. He wrote a Treatise on Electricity, published in 1750, and some papers on the same subject in several volumes of the Phil. Trans. But he has been chiefly distinguished as the ostensible person whose perverse conduct in the affair of the conductors of lightning produced such shameful discord and dissensions in the Royal Society, as continued for many years after, to the great detriment of science.

† That paper was not published in the Phil. Trans.

minus electricity of the Leyden experiment only, which that gentleman discovered.

CVII. On the Extraordinary Agitation of the Waters in Several Ponds in Hertfordshire. By the Rev. Thomas Rutherforth, D.D., F. R. S. p. 684.

On the 1st. of Nov. last (1755) there was an unusual motion of the water in a pond at Patmer-hall, a farm in the parish of Albury, and county of Hertford. Mr. Thomas Mott, the occupier of the farm, stated that there are two ponds in his yard, parted from each other by a causey, just wide enough to allow of a convenient passage for a waggon and driver: the causey runs from north to south; so that one of the ponds is to the west, and the other to the east of it. At the western end of the former are 2 drains, one higher than the other, to carry off the waste water; and on each side, at the other end, close to the causeway, is a mouth, or opening, where his cattle go to drink. The pond itself is about 8 roods over, and 12 roods long. The other pond is of the same size; except that there is a dove-house in the middle of it, which stands on a small island. On the first day of Nov. last, between 10 and 11 o'clock in the forenoon, his servants, who were then close to these ponds, heard a rumbling noise like the wind; and 3 ducks, which were then in the western pond, immediately flew out of it into the other, as if they were frightened. At the same instant the water in the western pond rose at the head of it, so as to run out of the lower drain, which was 10 or 12 inches above the level. It continued to move backward and forward for some time; but it did not swell any more at the head, but only rose and fell by turns at the two mouths; so that the motion was then from north to south. When it rose at either of the mouths, it flowed about 6 feet beyond what was then the water-mark. The other pond was quite still during the whole time.

Mr. Mott further stated, that at Wickham-hall, another farm about two miles and a half from him, in the parish of Bishop-Stortford, in the same county, a pond was moved at the same time in the same manner; and that the first motion of it was from east to west. And that a like motion was observed in a pond at Thaxted, in the county of Essex. At Royston also, in the county of Hertford, an unusual motion was observed in the pond, at 10 o'clock in the forenoon, Nov. 1, last past. The pond is a large one, and almost round. The bank of it, towards the north, is faced with a brick wall; and the bottom rises in a slope towards the south, so as to go $5\frac{1}{2}$ feet beyond the water-mark. In its return it rose against the brick wall, the top of which was about one foot above the level of the water, so as to run over it. The water afterwards moved from north to south, and back again, 5 times before it stopped.

CVIII. On a Drawing of two Large Pieces of Lead, with Roman Inscriptions on them, found several Years since in Yorkshire. By John Ward, LL.D. Rhet. Prof. Gresh. and V.P.R.S. 686.

Some time since a draught of two large pieces of lead, similar to each other, was communicated to this Society by a worthy member, Henry Stuart Stevens, Esq. The account then given of them, which accompanied the draught, was that "they were found in February 1734, a foot and a half under ground, on Hayshaw Moor, belonging to Sir John Ingilby, bart. in the manor of Dacre, near Pateley bridge, in the west riding of Yorkshire. The weight of each piece is 1 cwt. 1 qr. 16 lb." The larger inscription, placed on the top, may be thus read in words at length:

Imperatore Cæsare Domitiano Augusto, Consule VII.

And the lesser, on the side: *Brigantum.*

Dr. W. apprehends that those pieces of lead were part of the tax, which at that time was paid to the Romans out of the lead-mines in Britain,* and probably cast about the year 80, when Agricola was governor in Britain. And it is observable that the method now made use of in our lead-mines is not much different from this. For the metal, while liquid, is cast in an iron mould into large pieces, which from the shape of them are usually called pigs, of nearly the same shape, and on an average nearly the same weight, with that specified in the draught. And they are likewise commonly marked with the initial letters of the name of the smelter, or factor, and sometimes both, before they are sent from the mines.

CIX. Two Essays addressed to the Rev. James Bradley, D.D., and Astrom. Reg. By Mr. Charles Walmesley,† F.R.S. Dated Rome, Dec. 3, 1756. Translated from the Latin. p. 700.

I have, Rev. Sir, addressed to you 2 little essays, that relate to astronomy; for as no one is more master of that science than yourself, you can best judge of

* After this paper was written, Dr. W. found that another draught of those pieces of lead, with a brief account of them, had formerly been communicated to the Society, not long after they were discovered; and published in their Transactions, vol. xli. No. 459. That account differs very little from this, either as to the form, dimensions, and weight of the two pieces of lead; or the time and place of their discovery. But no attempt is there offered to explain the design for which they were made.—Orig.

† Dr. Charles Walmesley was an English Benedictine monk, and Roman Catholic bishop of Rome, also senior bishop and vicar apostolic of the western district, as well as a doctor of theology in the Sorbonne. He died at Bath in 1797, in the 76th year of his age, and the 41st of his episcopacy. Dr. W. was the last survivor of those eminent mathematicians who were concerned in regulating the chronological style in England, which produced the change of the style in this country in the year 1752. Besides this article, and some other ingenious works of a similar kind, in the Phil. Trans.,

the worth of any performance of that kind. The first essay is a Theory of the Precession of the Equinoxes, and the Nutation of the Earth's Axis; which, as is indebted to you for the discovery of the cause on which it is founded, as it also for settling of the effects with which its result is to be compared, ought to be laid before you as a homage, that of right is due. You expressed a desire of a theory on that subject; I have therefore examined, according to the principle of gravity, what motions may be produced in the globe of the earth by the actions of the sun and moon, and have endeavoured to determine their precise quantity and laws of variation. You observed yourself, that the supposition you made use of, of the earth's pole moving round the periphery of a circle, whose centre represented the mean place of the pole, was not exact: and in effect, as theory shows there are 2 equations arising from the sun's action, and as many from the action of the moon, to be used in settling the true place of the pole, the simple motion in the circle cannot answer accurately to the composition of these several motions; and has hence proceeded that surprizing difference you found between the polar distances calculated on that supposition, and those observed, in the star α Cassiopea, in the year 1738, and in η Ursæ Majoris in the year 1740 and 1741; which distances, if computed from the theory, as here laid down, agree with the observations as nearly as the others. You also insinuated that it would be proper to examine, whether the position of the moon's apogee had not a share of influence in these apparent motions of the stars. I therefore considered that point, but found, as you will see in the 5th prop. that the diminution of the moon's action in the higher part of its orbit, is so compensated by the increase of the same action in the lower part, that in the whole revolution of the moon no alteration arises, whatever be the situation of the nodes.

The second essay is a Theory of the Irregularities, that may be occasioned in the annual Motion of the Earth by the Actions of Jupiter and Saturn. I was led into this research by reflecting on that question, debated among the astronomers for so many ages past, whether the mean inclination of the two planes of the ecliptic and equator suffers any change, or remains invariable. Considering then what cause could produce a change in this inclination, I easily conceived, that if the action of Jupiter had sufficient power to alter the plane of the earth's

he published several separate works, both on mathematics and theology: as, 1. Analyse des Mesures des Rapports et des Angles, 4to, 1749; being an extension and explanation of Cotes's Harmonia Mensurarum. 2. Theorie du Mouvement des Apsides, 8vo, 1749. 3. De Inæqualitatibus Motuum Lunarium, 4to, 1758. An Explanation of the Apocalypse, Ezekiel's Vision, &c. By the fire at Bath, at the time of the riots, several valuable manuscripts, which he had been compiling during a well-spent life of labour and travelling through many countries, before his return to England, were irretrievably lost.

orbit, with respect to its own, by making their common intersection recede, in the same manner as the sun's action operates on the lunar orbit, an alteration in the obliquity of the ecliptic would necessarily follow, and on closer examination it appeared, that Jupiter really caused the earth to deviate in its course, and gave a retrograde motion to the line of intersection of their orbits; and further, that according to the present situation of that line, its regress was such, as to have occasioned a gradual diminution in the obliquity of the ecliptic for many ages past: by which means that question seems decided. The reason why the astronomers have not hitherto been able to settle that point is, because this variation proceeds at so slow a rate, that the observations of the ancients are not sufficiently exact to ascertain the small diminution, that has happened since their time. I have endeavoured to fix the laws, the quantity, and the period of this variation. From the same cause are also computed a progressive motion occasioned in the earth's aphelion, and a small regressive one in the equinoctial points: in all which is added the small share of influence that belongs to Saturn. In the last proposition are deduced some inequalities that occur in certain elements of the earth's theory, that have hitherto been supposed invariable. These as they are very small, I have only added in that view, that you, who know the best what degree of precision may be expected from astronomical observations, may judge whether they are worth notice or not.

I must observe, that some of the points of these two treatises have been considered by others; and if my conclusions any where differ from them, I leave it to other geometricians to decide which are right. All I shall say on that head is, that my result agrees with the computation of the great Sir Isaac Newton. As to the method, I have rather chosen to deduce the propositions by geometrical reasoning, after the manner of Sir Isaac Newton, which in researches of this kind always appeared to me much more simple, more rational, and more elegant, than the long calculus of an intricate analysis. Besides, if in the application there slips any error, it is more easily discovered in the former method.

On the Precession of the Equinoxes and Nutation of the Earth's Axis.

LEMMA 1. To find the Sun's Force on the Equatorial Parts of the Earth.

Let T (fig 1 pl. 2) be the earth's centre, B the pole, AT the right line joining the centres of the earth and sun, ASQ a circle described from the centre T, and perpendicular to the equator TS, and TQ the intersection of the circle TASQ, and of the plane perpendicular to the plane of the ecliptic: then from the point S of the equator draw SM parallel to AT, meeting TQ in M, and produce it to P, so that it may be $SP = 3SM$, and from P make PN perpendicular to the plane of the equator TS. Then by the similar triangles STM, SPN, it will be ST : TM :: SP or 3SM : $PN = \frac{3SM \times TM}{ST}$. Now, if the earth's radius ST denote the force

with which the sun urges the particle S towards the centre T, then 3SM will denote the force urging the same particle from the plane perpendicular to the plane of the ecliptic, therefore $\frac{3SM \times TM}{ST}$ will denote the force PN disturbing the situation of the equatorial plane, and its effect to turn the equator is as PN $\times$ ST, that is, as the force PN itself. But the force ST is to the force by which the earth is retained in its orbit about the sun, as the earth's radius ST is to the earth's distance from the sun; and the force by which the earth is retained in its orbit, is to the centrifugal force at the earth's equator, in the ratio compounded of the direct ratio of the earth's distance from the sun to the earth's radius, and of the inverse duplicate ratio of the earth's periodic time about the sun to the time about it sown axis: hence by composition of forces, writing s for the earth's annual periodic time, and t for its diurnal, it will produce the force PN to the centrifugal force at the earth's equator, as $\frac{3SM \times MT}{ST^2} \times \frac{t^2}{s^2}$ to 1. And it appears that this is the efficacious force PN to turn the equator about the axis perpendicular to the plane TASQB, that is, about the axis which lies in the common section of the equator, and the plane QT perpendicular to the ecliptic.

At equal distances from the point S, in the circumference of the equator, take the two points F; then because the force of each of these to turn the equator about the axis perpendicular respectively to the plane TFB, the effect of both forces compounded concurs with the said force PN, for turning the equator with the earth about the axis perpendicular to the plane TASQ. Draw the perpendiculars FG in the plane QT perpendicular to the ecliptic; then the sum of the forces by which these two particles fly from the plane of the equator, will be $\frac{6FG \times GT}{FT}$, as appears from above, of which that part which conspires with the said force PN, since it is to $\frac{6FG \times GT}{FT}$ as FT to ST, will be $\frac{6FG \times GT}{ST}$ (the remaining parts of those forces, because opposites, mutually destroying each other;) or, from the similitude of the triangles FGT and SMT, this sum will be to the force PN as $2FT^2$ to ST^2; and therefore, since the sum of all the FT^2 through the whole circumference, is half the sum of all the ST^2, the sum of all the actions through the circuit of the equator, is half on the particle S: therefore that force, by which the position of the equatorial circle is disturbed, collected from the forces of all the points in that circumference, is to the centrifugal force at the same equator, putting the earth's radius ST $= 1$, as $\frac{3SM \times MT}{2} \times \frac{t^2}{s^2}$ to 1. Q. E. I.

LEMMA 2. The force of all the particles without the interior globe of the earth, viz. except that whose diameter is less than the earth's axis, on all sides, to turn the earth about the side axis, is to the force of all the particles uniformly disposed in the equatorial circle after the manner of a ring, for turning the earth about the same axis, as 2 to 5. As is clearly proved by Newton.

LEMMA 3. To determine the Ratio of the Motion of the Whole Earth, to the Motion of the Material Stratum over the Interior Globe of the Earth.—Let C represent the centre of the earth (fig. 2,) CK a portion of any diameter of the equator, EDGK a section of the earth perpendicular to the diameter CK, and to the plane of the equator; this section and all the sections parallel to it are similar ellipses, as is well known. From the centre K of the ellipse EDG draw the radius KE in the plane of the equator, and it will be the greater semiaxis of the ellipse, and the radius KD perpendicular to it will be the less semiaxis; draw also the two radii KM, Km very near each other, and with the centre K and radius KD, describe the circle DHe cutting Km, KM, KE, in h, H, e; and with any radius Kr describe the arc rn cutting KM, Km, in r, n, and the arc st very near the arc rn, cutting KM and Km in s and t. Now because the areola $rstn$, while the earth revolves about the axis CK, has a velocity proportional to the distance Kr, its momentum will be proportional to $\text{K}r \times rs \times st$ or $\frac{\text{K}r^2 \times \text{H}h \times rs}{\text{KH}}$; hence the momentum of the whole areas KMm will be proportional to $\frac{\text{KM}^3 \times \text{H}h}{3\text{KH}}$. Make HV perpendicular to KD, and if the greater semiaxis KE be supposed but little to exceed the less semiaxis KD, it will be $\text{HM} = \frac{\text{HV}^2 \times \text{E}e}{\text{KH}^2}$ nearly, therefore $\frac{\text{KM}^3 \times \text{H}h}{3\text{KH}} = \frac{\text{KH}^2 \times \text{H}h}{3} + \frac{\text{HV}^2 \times \text{H}h \times \text{E}e}{\text{KH}}$, therefore the sum of the momenta of all the areas KMm, that is, the momentum of the whole section, will be proportional to the circumference drawn into $\frac{1}{3}\text{KH}^2 + \frac{1}{2}\text{KH} \times \text{E}e$. Now let CA be equal to the greater semidiameter of the earth, CB the less semidiameter, and AB their difference; let Kk be a very small particle of the axis CK, and let c denote the circumference of the equator; then, because KH : KE :: CB : CA, and Ee : AB :: KE : CA, the momentum of the speroidical portion whose thickness is Kk, terminated by two parallel sections, that is, the circumference DHD drawn into $\text{K}k \times (\frac{1}{3}\text{KH}^2 + \frac{1}{2}\text{KH} \times \text{E}e,)$ will be proportional to $\frac{c \times \text{CB}^3 \times \text{KE}^3 \times \text{K}k}{3\text{CA}^4} + \frac{c \times \text{CB}^2 \times \text{KE}^3 \times \text{AB} \times \text{K}k}{2\text{CA}^4}$; therefore the sum of all these momenta, or the momentum of the whole spheroid about the axis CK, will be denoted by $\frac{c^2 \times \text{CB}^3}{16\text{CA}} + \frac{3c^2 \times \text{CB}^2 \times \text{AB}}{32\text{CA}}$, or by $\frac{d^2 \times \text{CA} \times \text{CB}}{16} + \frac{3d^2 \times \text{CA} \times \text{AB}}{32}$, if d denote the circumference described with the radius CB. Hence the momentum of the interior globe, whose radius is CB, will be expressed by $\frac{1}{16}d^2 \times \text{CB}^2$: therefore the momentum of the interior globe is to the momentum of the whole earth revolving about the axis CK, as CB^2 to $\text{CA} \times \text{CB} + \frac{3}{2}\text{CA} \times \text{AB}$, or as $\text{CA} - 2\text{AB}$ to $\text{CA} + \frac{1}{2}\text{AB}$ nearly; and the momentum of the matter incumbent on the interior globe of the earth, to the momentum of the whole earth, as 5AB to 2AC nearly. Q. E. I.

COROL. By the same mode of reasoning, if the circumference of the circle described with the radius CB be revolved about its own diameter, since the motion of

any point of the circumference is as its distance from that diameter, the motion of the whole circumference will be expressed by 4CB^2: hence, if instead of the circumference there be substituted a very thin ring, the motion of the ring will be to the motion of the globe whose semidiameter is CB, as 4CB^2 to $\frac{1}{16}d^2 \times \text{CB}^2$; that is, in the ratio composed of the ratio of the matter in the ring to the matter in the globe, and of the ratio of double the square of the diameter to triple the square of the quadrantal arc of a circle; as Newton demonstrated. And thus, if the less semidiameter of the earth be to the greater as 229 to 230, and the whole matter diffused over the interior globe of the earth he conceived to coalesce, as Newton supposes, into a uniform ring surrounding the equator, the motion of the ring will be to that of the interior globe, as 4590 to 485223; and the notion of the ring to that of the whole earth, as 4590 to 489813.

It may be observed here that this proportion of the motions, viz. which is derived from the hypothesis, that the whole matter in the interior globe of the earth coalesces into a superior ring about the equator, is a very little erroneous: for it appears that the single particles of matter, in their own places, will not take each the same motion from the rotation of the earth, as when they are supposed to adhere together as in that hypothesis. But, in the investigation of the mean precession of the equinoxes, Newton omits that difference of the motions, as being very small, and of no consequence. But now, by the late discoveries in astronomy, when we must search out more accurately the proportion of the forces of the sun and moon, and of each of their effects, it seems necessary to have regard to that difference, and therefore to this lemma we must annex the following proposition.

PROP. 1. *Prob.* To Investigate the Mean Precession of the Equinoxes generated by the Force of the Sun. Let SPQ (fig. 3) represent the earth's equator, ARL the ecliptic, TL the line of the intersection of the planes of the equator and ecliptic, and PM a perpendicular from the point P of the equator on the plane QT supposed perpendicular to the ecliptic. Taking any very small arc P*p* of the equator, let PN be double the space which a body can run through perpendicular to the equator, impelled by the force defined in lemma 1st, in the time in which the point *p* revolving with the equator describes the arc P*p*; by which means, after that particle of time the plane of the equator being changed into the position *s*N*pn*, and now cutting the ecliptic in *n*, then the arc L*n* will be the recess of the intersection of the equator and ecliptic, or the precession of the equinoxes. On N*pn* demit the perpendicular L*r*, and on TL the perpendicular PI; then since the lines PN, L*r* are as the sines of the arcs P*p*, PL, it will be P*p* : PN :: PI : L*r*; and writing *b* for the sine, and *c* for the cosine of the inclination of the ecliptic and equator, to radius 1, in the rightangled triangle L*rn* it will be $b : 1 :: \text{L}r : \text{L}n$, therefore $\text{P}p \times b : \text{PN} :: \text{PI} : \text{L}n$, and $\text{L}n = \frac{\text{PN} \times \text{PI}}{b \times \text{P}p}$: therefore, the arc

Pp being given, Ln is as PN $\times$ PI. From the centre T describe the arc RP of a circle perpendicular to the equator LP; then, in the spherical triangle LRP, the tangent of the angle RLP, (viz. of the inclination of the ecliptic and equator,) to the tangent of the arc RP, i. e. $\frac{b}{c}$ to $\frac{\text{MT}}{\text{PM}}$, as radius 1 to PI the sine of the arc PL; hence is $\text{PI} = \frac{c \times \text{MT}}{b \times \text{PM}}$. Also in the same triangle it is $b : 1 :: \text{MT} : \text{RH}$ the sine of the elliptic arc RL, that is, $\text{MT} = b \times \text{RH}$. Further, PN is as PM $\times$ MT by lemma 1; therefore PN $\times$ PI and also Ln is as RH^2; that is, the horary precession of the equinoxes, generated by the sun's force, is in the duplicate ratio of the sine of the sun's distance from the equinox. And because the sum of all the RH^2, in which time the sun completes his period, is half the sum of all the TR^2, therefore the annual precession of the equinoxes is the half of that which the sun, always remaining in the quadratures of the equinoxes, that is in the solstices, could generate in the same time.

Let therefore the sun be in the solstitial colure, and let LP and LR (fig. 4) be quadrants of a circle, and Lr the measure of the angle Lpn or PpN; hence, in the triangle Lrn, Ln or the horary precession of the equinoxes, is in this case to Lr, or to the angle Ppn, as 1 to b: but the angle PpN (drawing ps perpendicular to the radius TP,) is to double the angle Pps, that is to the angle PTp, which is the earth's horary motion about its axis, as the force which acts according to PN, to the centrifugal force at the equator, that is, by lemma 1, as $\frac{3\text{SM} \times \text{MT}}{2} \times \frac{t^2}{s^2}$ (fig. 1) to 1; or, because in this case MT is $= b$, and $\text{SM} = c$, as $\frac{3bc}{2} \times \frac{t^2}{s^2}$ to 1; and the horary motion of the earth about its axis, is to the horary motion of the sun, as s to t: hence, compounding the ratios, the horary precession of the equinoxes, is to the horary motion of the sun, as $\frac{3c}{2} \times \frac{t}{s}$ to 1, and in the same ratio is the annual precession to the annual motion of the sun.

Therefore the annual precession of the equinoxes, on the hypothesis that the sun remains all that time immovable in the solstice, will be $\frac{3c}{2} \times \frac{t}{s} \times 360^\circ$, and the true annual precession will be the half of this. But as the sun acts not only on the equatorial circle, as is supposed in this proposition, but on the whole matter dispersed over the interior globe of the earth, and the globe itself must participate of the motion generated by this force, therefore the precession is diminished in the ratio composed of the ratio of 2 to 5 by lemma 2, and of the ratio of 5AB to 2CA by lemma 3; therefore the annual precession of the equinoxes arising from the solar force, at length produces

$$\frac{3c}{4} \times \frac{t}{s} \times \frac{2}{5} \times \frac{5\text{AB}}{2\text{AC}} \times 360^\circ = \frac{3c}{4} \times \frac{t}{s} \times \frac{\text{AB}}{\text{AC}} \times 360^\circ.$$

Let then the earth's greater diameter be to the less, as 230 to 229, and it

will be $\frac{AB}{AC} = \frac{1}{230}$; and, the inclination of the ecliptic being 23° 28′ 30″, the nnual precession of the equinoxes produced by the solar force will be 10″.583. But if the ratio 178 to 177 be that of the earth's diameters, as it has been derived from late observations, then will $\frac{AB}{AC} = \frac{1}{178}$, and the annual precession of the equinox 13″.675.

If the communication of motion between the interior globe of the earth and the exterior matter be according to the Newtonian hypothesis, as explained in the corol. to lemma 3, and the earth's greater diameter be to the less as 230 to 229, then the annual precession of the equinoxes by the sun's force will be $\frac{3c}{4} \times \frac{t}{s} \times \frac{2 \times 4590}{5 \times 489813} \times 360° = 9''.124 = 9''\ 7'''\ 26''''$. And if the inclination of the ecliptic be supposed $23\frac{1}{2}°$, that precession will become $9''\ 7'''\ 20''''$, as Newton found it. Q. E. I.

Corol. 1. Put now, with Bradley, the whole mean annual precession equal to 50″.3; then from it deduct 10″.583, and there will remain 39″.717 for the mean annual precession arising from the force of the moon, and then the lunar force will be to the solar as 3.753 to 1, on the hypothesis that the ratio of the earth's diameters is that of 230 to 229; but if the ratio be that of 178 to 177, the earth being uniformly dense, from 50″.3 taking 13″.675, the annual precession generated by the lunar force will be 36″.625, and the lunar force to the solar as 2.678 to 1.

Corol 2. Take now in the ecliptic the arc Rq, fig. 3, described by the sun in a very small time, as an hour, and draw qh parallel to RH; then because of what is said in the proposition, the horary precession of the equinoxes, the sun being in any place R, is to the mean horary precession, as RH^2 to $\frac{1}{2}TR^2$, or, since $RH : TR :: Hh : Rq$, as $RH \times Hh$ to $\frac{1}{2}\, TR \times Rq$, the true precession will be to the mean precession, while the sun describes the arc LR, as the space LRH to the sector LTR, and their difference to the mean precession, as the triangle TRH to the sector LTR: therefore, LR being $= 45°$, that is, in the octants of the equinoxes with the sun, this difference or equation, which then becomes the greatest, writing d for the circumference of the circle whose radius is 1, is $= \frac{10''.583}{2d}$ or $\frac{13''.675}{2d}$; hence arises this theorem: "The motion of the sun is to the motion of the equinoxes generated by the sun's force, as radius is to the sine of double the greatest equation of the equinoxes." Whence in the former case the greatest equation produces 51‴, in the latter 1″ 5‴. In other places, that equation is to the greatest equation, as the sine of double the sun's distance from the nearest equinox or solstice, to radius, as is evident: which is to be added to the mean motion while the sun passes from the solstices to the equinoxes, but subtracted while he passes from the equinoxes to the solstices.

Corol. 3. From the proposition in general it follows, that the mean horary regress of the line of intersection of the planes of the terrestrial equator, and of the orbit of any planet revolving about the earth, is as the force of that planet on the terraqueous globe, other things remaining, and the cosine of the inclination of its orbit to the earth's equator conjointly.

Prop. 2. *Prob.* To find the Inequality of the Precession of the Equinoxes, which depends on the Various Situation of the Moon's Nodes.—Let SLD (fig. 5) be the equator, EAFL the ecliptic cutting the equator in L, E the vernal equinox, L the autumnal, GFD the moon's orbit cutting the equator in D and the ecliptic in F, AGS a great circle perpendicular to the equator, and let SD, GD be quadrants of circles. While the node F describes the horary ecliptic arc Ff, the intersection D, by the lunar force, will be transferred through the arc Dd, and describe the circle Sd representing the position of the equator after an hour is elapsed, and cuts the ecliptic in n; and draw Dg, Lr perpendicular to the equator. At that time let B be the sine, and C the cosine, of the inclination of the lunar orbit to the terrestrial equator; b and c being, as before, the sine and cosine of the inclination of the ecliptic, or of the mean inclination of the lunar orbit to the equartor; then (by corol. 3 of the preceding prop.) will the mean horary regress of the intersection of the planes of the equator and ecliptic, produced by the moon's force, be to Dd, the mean horary regress of the intersection of the planes of the equator and lunar orbit, as c to C; but Ff is to the said regress of the intersection of the planes of the equator and ecliptic, as the mean motion of the moon's nodes, to the mean motion of the equinoxes generated by the moon's force; which ratio put as h to 1; therefore Ff : Dd :: $c \times h$: C; but Dd : Dg :: 1 : B, and Dg : Lr :: 1 : sine of the arc LS, which call K, and let Lr : Ln :: b : 1; hence, by composition of ratios, Ff : Ln :: $b \times c \times h : B \times C \times K$.

Through the node F describe the great circular arc FC perpendicular to SL; then, by spherical trigonometry, the cosine FL is to radius 1, as the cotangent FLC to tangent LFC; and sine LFC to sine DFC, as cosine FLC to cosine FDC: but as the angle DFC is the sum of the angles DFL and LFE, the sine DFC = sine DFL × cosine LFC + cosine DFL × sine LFC. Hence, writing p and q for the sine and cosine of the angle DFL, viz. of the mean inclination of the moon's orbit to the ecliptic, and v and u for the sine and cosine of the arc EF, viz. of the distance of the node from the vernal equinox, it is then cosine FDL = C = $cq + bpu$. Also in the triangle FDL, it is B : p :: v : sine DL, therefore is the cosine of the arc DL, or sine of the arc LS, that is, $K = \frac{1}{B}\sqrt{B^2 - p^2v^2} = \frac{1}{B} \times \overline{bq - cpv}$.

Hence therefore is obtained $BCK = (cq + bpu) \times (bq - cpu) = bcq^2 - (c^2 - b^2) \times pqu - bcp^2u^2$: but by writing 1 for q, and rejecting the term bcp^2u^2

on account of the smallness of p, being the sine of $5^0\ 8\frac{1}{2}'$; then is Ln to Ff as $bc - (c^2 - b^2) \times pu$ to $b \times c \times k$; and the sum of the motions Ln to the sum of the motions Ff, while the node F describes the arc EF, as the sum of the quantities $bc - (c^2 - b^2) \times pu$ to the sum of all the $b \times c \times k$, that is, as $b \times c \times$ EF $+ (c^2 - b^2) \times pv$ to $b \times c \times k \times$ EF: and therefore while the node is passing from the equinox to the solstice the precession of the equinoxes is $\frac{90^\circ}{k} + \frac{(c^2 - b^2) \times p \times 90^\circ}{b \times c \times k \times \text{EA}}$, and while the node passes from one equinox to the other the precession is $\frac{180^\circ}{k}$. From the former motion taking half the latter, there will remain $\frac{(c^2 - b^2) \times p \times 90^\circ}{b \times c \times k \times \text{EA}}$ for the difference between the true and the mean precession, that is, for the greatest equation of precession, viz. when the lunar nodes are in the solstitial points. In other places it appears that this equation is to the greatest equation, as the sine of the nodes distance from the equinox is to radius, adding it to the mean precession in the regress of the ascending node, from the vernal equinox to the autumnal, but subtracting it from the autumnal to the vernal equinox. It is to be noted also that $c^2 - b^2 = 2c^2 - 1 =$ cosine $2 \times 23^0\ 28\frac{1}{2}'$, and $b \times c = \frac{1}{2}$ sine $2 \times 23^0\ 28\frac{1}{2}'$, and hence $\frac{c^2 - b^2}{b \times c} = \frac{2 \text{ cos.}}{\text{sin.}}\ 2 \times 23^0\ 28\frac{1}{2}' = \frac{2}{\text{tang } 2 \times 23^\circ\ 28\frac{1}{2}'}$. Therefore $\frac{(b^2 - c^2) \times p \times 90^\circ}{b \times c \times k \times \text{EA}}$ becomes $\frac{90^\circ \times 2 \sin\ 5^\circ\ 8\frac{1}{2}'}{k \times \text{EA} \times \tan.\ 2 \times 23^\circ\ 28\frac{1}{2}'}$; and hence comes this following theorem: "The tangent of double the inclination of the equator to the ecliptic, is to the sine of double the inclination of the lunar orbit to the ecliptic, as radius to the sine of a certain angle; and then, the mean motion of the nodes, is to the mean motion of the equinoxes generated by the moon's force, as the sine before found, to the sine of the greatest equation of the equinoxes." Instead of the sine of double the inclination of the lunar orbit to the ecliptic, in the theorem, may be taken double the sine of the same inclination, since the error hence arising is of no consequence, as may be easily found on trial. But the annual motion of the moon's nodes is $19^0\ 20\frac{1}{2}'$; and the annual motion of the equinoxes generated by the lunar force is $39''.717$ by corol. 1 prop. 1, the ratio of the earth's diameters being $= \frac{230}{229}$, whence k is $= 1753$. And the motion of the equinoxes, when the ratio of the earth's diameters is $\frac{178}{177}$, is 36 .625, and then $k = 1901$. Hence in the former case the greatest equation of the equinoxes produces $19''\ 38'''$, in the latter $18''\ 16'''$. Q. E. I.

COROL. From this proposition, the precession of the equinoxes generated by the lunar force, in a given time, is proportional to the quantity $b \times c \times k$, or $bc - (c^2 - b^2)\,pu$: therefore it is greatest when the moon's ascending node is in the beginning of Aries, for then u is $= -1$, and it is least when that node is in the sine Libra, because $u = 1$ in that case. Hence, since the annual pre-

cession generated by the lunar force is $= \frac{39''.717}{bc} \times (bc - (c^2 - b^2)pu)$, or $\frac{36''.625}{bc} \times (bc - (c^2 - b^2)pu)$, having no regard to the change of situation of the nodes made in that time, the difference between the annual mean and greatest precession will be $\frac{39''.717\,(c^2 - b^2)\,p}{\tan.\,2 \times 23°\,28'\frac{1}{2}} = \frac{39''.717 \times 2\sin.\,5°\,8'\frac{1}{2}}{\tan.\,2 \times 23°\,28'\frac{1}{2}}$, or $\frac{36''.625 \times 2\sin.\,5°\,8\frac{1}{2}'}{\tan.\,2 \times 23°\,28\frac{1}{2}'}$.

Therefore, "The tangent of double the inclination of the ecliptic to the equator, is to the sine of double the inclination of the lunar orbit to the ecliptic, as the mean annual precession of the equinoxes generated by the lunar force, to the difference between the mean precession and the greatest or least." Hence in the former case that difference is $= 6''\ 37'''$, in the latter $6''\ 6'''$; and therefore if the total annual precession be stated at $50''\ 20'''$, in that year, in the middle of which the moon's ascending node is near the first degree of Aries, the precession of the equinoxes will be $56''\ 57'''$, or $56''\ 26'''$; but when the node is in the sign Libra, the precession of that year will be $43''\ 43'''$, or $44''\ 14'''$. And because the aforesaid difference, in other times, is as the sine of the node's distance from the solstitial points, it will be easily found for any year, the situation of the nodes being given.

PROP. III. *Prob.*—To find the Variation of the Inclination of the Ecliptic to the Equator generated by the Sun's Force.—Every thing remaining as in the 1st proposition, produce the arc LS (fig. 3) to V, till LV be a quadrant of a circle, and demit Vs perpendicular to the arc pN produced; then will Vs be the measure of the horary variation of the inclination of the ecliptic to the equator. But Vs : Lr :: TI : PI, and Lr : Ln :: b : 1, and by prop. 1 the horary precession of the equinoxes Ln, is to the horary precession when the sun is in the solstices, which call g, as RH^2 to TR^2: therefore, compounding the ratios,

$Vs : g :: \frac{b \times TI \times RH^2}{PI} : TR^2$, or, because $TR = 1$, $PI = \frac{c \times RH}{PM}$, $TI = \frac{TH}{PM}$, it is $Vs : g :: \frac{c}{b} \times RH \times TH : 1$; and the sum of all the horary variations Vs, while the sun describes the arc LR, is to the sum of as many angles g, as the sum of all the factors $RH \times TH$ drawn into $\frac{b}{c}$, to the sum of as many squares of 1, that is, as $\frac{1}{2}RH^2 \times \frac{b}{c}$ to the arc LR; and the total variation, by which the inclination of the equator to the ecliptic is diminished, in the sun's progress from the equinox to the solstice, is to the sum of the angles g, (which then becomes equal to half the annual precession generated by the solar force, that is, equal to half of $10''.583$ or half of $13''.675$) as $\frac{b}{2c}$ to the arc LV; and hence the total variation will be $\frac{b \times 10''.583}{c \times 4LV} = \frac{10''.583 \times \tan.\,23°\,28'\frac{1}{2}}{4LV}$, or $\frac{13''.675 \times \tan.\,23°\,28'\frac{1}{2}}{4LV}$. Hence arises this theorem: "The motion of the sun, is to the motion of the equinoxes ge-

nerated by the sun's force, as the tangent of the mean inclination of the ecliptic to the equator, is to the tangent of the total variation of the same inclination." And hence the total variation will come out in the former case $= 44''$, in the latter $57''$, viz. the sun being in the solstices: in other places the variation is in the duplicate ratio of the sine of the sun's distance from the equinox, to radius; and because the difference between half the total variation and the variation generated in the time the sun describes any arc LR, is to half the total variation, viz. to $22''$ or $28''\frac{1}{2}$, as $2RH^2 - 1$ is to 1, that is, as the co-sine of double the sun's distance from the equinox, to radius; and therefore, the sun's place being given, there will be given this difference or equation, to be added to the mean inclination of the ecliptic whenever the sun's distance from either equinox is less than 45°: and to be subtracted when that distance is greater. Therefore the inclination of the ecliptic to the equator is greatest when the sun is in the equinoxes, but least when in the solstices.

Prop. IV. *Prob.* To determine the Variation of the Inclination of the Ecliptic, which depends on the Various Situation of the Moon's Nodes.—The same things remaining as in the 2d prop. let now the moon be at K (fig. 5), and describe the arc of a great circle KZ perpendicular to the equator DZS, and through the point Z, the arc Zd showing the situation of the equator after the space of an hour; and let Zd cut the moon's orbit in d, and the lines Dg, Lr in e and q; also from the point V of the equator, LV being a quadrant of a circle, demit Vt perpendicular on the arc dZ produced. Let p denote the mean horary motion of the equinoxes generated by the moon's force: then by prop. 2 it is p : Dd :: c : C; and, DS being a quadrant of a circle, from what is demonstrated in prop. 1, it follows that 2Dd : Dd :: 1 : sin.^{2} DK; and hence Dd : De :: 1 : B; then De : Lq :: sin. DZ : sin. LZ, and Lq : Vt :: sin. LZ : cos. LZ; hence, by compounding all these ratios, it is $2p$: Vt :: c × sin. DZ : B × C × sin.^{2} DK × cos. LZ. But the cos. LZ is = sin. DL × sin. DZ + cos. DL × cos. DZ; hence $2p$: Vt :: c : B × C × sin.^{2} DK × sin. DL + cos. DL × $\frac{\text{cos. DZ}}{\text{sin. DZ}}$; but in the spherical triangle DKZ it is C : 1 :: cotan. DK or $\frac{\text{cos. DK}}{\text{sin. DK}}$: cotan. DZ or $\frac{\text{cos. DZ}}{\text{sin. DZ}}$; hence at length is produced $2p$: Vt :: c : B × C × sin. DL × sin.^{2} DK + B × C × cos. DL sin. DK × cos. DK. Therefore the sum of all the Vt, that is, the sum of all the horary variations of the inclination of the ecliptic, generated in the time of the moon's revolution, the situation of the nodes remaining, is to the sum of as many motions p, as the sum of all the quantities 2B × C × sin. DL × sin.^{2} DK + 2B × cos. DL × sin. DK × cos. DK in a circle, to the sum of as many cosines c, that is, as B × C × sin. DL to c. Putting therefore, as before, the mean motion of the nodes, to the mean motion of the equinoxes generated by the lunar force, as h to 1, the variation of the horary mean inclination of the ecliptic, in a given

month, will be to the mean horary motion of the nodes Ff, as $B \times C \times$ sin. DL to $C \times k$, that is, because sin. DL $= \frac{pv}{B}$, and $C = cq + bpu$, as $cpqv + bp^2vu$ to $C \times k$, or as pv to k very nearly; therefore the sum of all the variations of the inclination of the ecliptic, while the moon's node describes the arc EF, is to the motion of the node EF, as the sum of all the pv is to the sum of as many times k, that is, as $p \times (1 + u)$ to $k \times$ EF; and the whole variation that changes the inclination of the ecliptic, in the regress of the node from one equinox to another, is to the motion of the nodes 180°, as $2p$ to $k \times$ EL, which therefore is equal to $\frac{2p \times 180°}{k \times EL}$, and which therefore will be easily produced by the following theorem: "The motion of the nodes, is to the motion of the equinoxes produced by the lunar force, as the sine of the inclination of the lunar orbit to the ecliptic, is to the sine of half the whole variation of the inclination of the ecliptic to the equator."

If the ratio of the earth's diameters be $\frac{230}{229}$, the motion of the moon's nodes will be to the motion of the equinoxes, by the prop. as 1753 to 1, and as 1901 to 1 if the ratio of the diameter be $\frac{178}{177}$. In the former case the theorem will produce 21″ 5‴ for the whole variation of the inclination of the ecliptic, in the latter case 19″ 27‴; generated while the lunar nodes pass from one equinox to another. In places between the equinoxes, the variation will be to the whole variation, from the demonstration, as $1 + u$ to 2, that is, as the versed sine of the node's distance from the vernal equinox, to the diameter; or, the difference between half the whole variation and the variation for a given time, is to half the whole variation, viz. to 10″ 32½‴, or 9″ 43½‴ as the cosine of the node's distance from the vernal equinox, is to radius: and this difference or equation is to be added to the mean inclination of the ecliptic in the node's regress from the summer solstice to the winter, but to be subtracted in the other half of the node's revolution, to obtain the true inclination of the ecliptic. And the greatest obliquity of the ecliptic is when the moon's ascending node is in the vernal equinox or beginning of Aries; but the least, when the same node comes back to the autumnal equinox, or to the sign Libra. Q. E. I.

PROP. V. *Prob.* To investigate the Inequalities of the Precession of the Equinoxes, and of the Variation of the Obliquity of the Ecliptic, which depend on the Situation of the Moon's Apogee.—Let the moon describe in the plane of the ecliptic the ellipse APBL (fig. 6), of which C is the centre, T the focus occupied by the earth, AB the greater axis, CD the less semi-axis, TL the common section of the planes of the equator and ecliptic. Let the moon be at P, and draw TP, Tp, cutting off the sector TPp described by the moon's horary motion. With the centre T, and radius equal to the greater semi-axis CA, describe the circle HNO, cutting TP and Tp in N and n; and on TL demit the perpendiculars NI, nm,

and on TA the perpendicular NR. If the moon be supposed to revolve in the circle HNO, where it touches it at the place N, the horary precession of the equinoxes generated by the lunar force, by the demonstration in prop. 1, will be as NI^2; but that precession increases in the ratio of the force that produces it, and this force is in the inverse triplicate ratio of the moon's distance TP; therefore the horary precession is as $\frac{NI^2}{TP^3}$, or as the same quantity $\frac{NI^2}{TP^3}$ drawn into the constant sector TPp, that is, as $\frac{NI^2 \times Nn}{TP}$ or as $\frac{NI \times Im}{TP}$; but $\frac{1}{TP} = \frac{CA^2 + TC \times TR}{CA \times CD^2}$ by the nature of the ellipse; hence the total precession, generated while the moon revolves in her orbit, is as the sum of the quantities $NI \times Im \times \frac{CA^2 + TC \times TR}{CA \times CD^2}$ in the circle; or (because the ambiguous term $+\frac{TC \times TR}{CA + CD^2}$ may be rejected, as being positive through one half of the circular circumference, and negative in the other half), as the sum of all the factors $NI \times Im$ in the circle, that is, as the area of the circle HNOH itself: and therefore the precession of the equinoxes, in every revolution of the moon, remains the same in any situation of the apogee.

The horary variation of the inclination of the ecliptic, if the moon be in N revolving in the circle HNO, will be, from what is demonstrated in prop. 3, as $NI \times TI$: but if the moon be transferred to P, the same variation will be as $\frac{NI \times TI}{TP^3}$, or as $\frac{NI \times TI}{TP^3} \times TPp$, that is, as $\frac{NI \times TI \times Nn}{TP}$, or as $\frac{NI \times nq}{TP}$ by drawing nq parallel to TI; and therefore, because of the given parts in the ratio, the variation of the inclination of the ecliptic, generated in the time of the moon's revolution, is as the sum of all the factors $NI \times nq$ in the circle, that is, nothing.

Hence there can be nothing legally collected from the situation of the moon's apogee, to induce any variation, either in the motion of the equinoxes, or in the obliquity of the ecliptic.

SCHOLIUM. From the premises it appears, that the double motions of the earth's poles, by the forces from both the sun and moon separately, unite; the one in a plane parallel to the ecliptic, by which the equinoctial points are continually retracted in antecedentia, and therefore the stars seem to proceed in consequentia; but the other motion is perpendicular to the plane of the ecliptic, by which the poles of the earth incline and oscillate, by turns approaching to and receding from the pole of the ecliptic, and hence changing the declination of the stars. We deduce the quantity of these motions from the excess of the earth's altitude at the equator above its altitude at the poles, according to a double hypothesis, viz. by which that excess is estimated either the 230th or the 178th part of the whole altitude, hitherto used by the chief mathematicians. But if the known nutation of the earth's axis be assumed, which hitherto has been stated at 18″ as due to the action of the moon, and there be hence required the

3

other motions, by the foregoing propositions these will come out, viz. the annual mean precession of the equinoxes generated by the solar force 16″ 24‴; by the lunar 33″ 54‴, the greatest equation of precession by the solar force 1″ 23‴, by the lunar force 16″ 45‴: the nutation of the axis by the solar force 1″ 10‴, the earth considered as uniformly dense.

To this part Mr. Walmesley adds some tables for computing these effects in all different circumstances and positions; but are omitted, as they have been superseded by more recent and correct tables.

On the Inequalities of the Earth's Motions.

PROP. I. To find the Forces of Jupiter and Saturn for Disturbing the Earth's Motions.

Let S be the sun (fig. 7), I Jupiter, T the earth revolving in the orbit TOT: join SI, IT, ST; the latter cutting the moon's orbit HLH in L. Then, by reasoning after Newton's manner in determining the sun's action on the moon, if SI denote the gravitating force of the sun on Jupiter, ST will denote the force by which Jupiter depresses the earth towards the sun very nearly; but the gravity of the sun on Jupiter is to the gravity of Jupiter on the sun, at like distances, from Newton's demonstration, as 1 to 1067; and the gravity of Jupiter on the sun is to the gravity of the earth on the sun, as ST^2 to SI^2: then is the gravity of the earth on the sun to the force of the sun for depressing the moon towards the earth, as ST to TL. Uniting these ratios gives the force of Jupiter depressing the earth to the sun, in proportion to the force of the sun depressing the moon to the earth, as ST^4 to $SI^3 \times TL \times 1067$ very nearly, or, because by writing m and n, for the periodic times of the earth and Jupiter, it is $ST^3 : SI^3 :: m^2 : n^2$, as $m^2 \times ST : n^2 \times TL \times 1067$; and in this ratio is the force of Jupiter for disturbing the earth's motion, to the sun's force for disturbing the moon's motion. And the latter force being given, there will hence be given the former.

Also because the gravity of Saturn on the sun is to the gravity of the sun on Saturn, at equal distances, as 3021 is to 1, instead of the number 1067, in the preceding computation, be substituted 3021, and Saturn's revolution instead of Jupiter's, then it will show the ratio of Saturn's force on the earth, to the sun's force on the moon. Q. E. I.

Corol. Because the linear errors, arising from different forces, are as those forces and the squares of the times conjointly; and the angular errors as those linear ones applied to the radii of the orbits; it follows that the annual angular errors of the earth with respect to the sun, are to the menstrual angular errors of the moon in respect of the earth, in the ratio compounded of the direct ratio of Jupiter's force on the earth, and of the sun's force on the moon, and the duplicate ratio of the periodic times of the earth about the sun, and the moon about the earth conjointly, and of the inverse ratio of the radii ST, TL, that is, if h be

taken to denote the periodic time of the moon, from what is above demonstrated, as m^4 to $n^2 \times h^2 \times 1067$, or as 1 to $\frac{n^2h^2}{m^4} \times 1067$. Therefore if the errors in a given time for example, in a certain number of years, are to each other as 1 to $\frac{n^2h}{m^3} \times 1067$; that is, the inequalities of the earth's motion, are to the inequalities of the moon's motion, in a given time, in a ratio compounded of the duplicate ratio of the earth's periodic time to the periodic time of Jupiter, of the simple ratio of the earth's periodic time about the sun to the moon's periodic time about the earth, and of the ratio of the gravity on Jupiter to the gravity on the sun, conjointly. Therefore, the periodic times being, for Jupiter 4332.514 days, for the earth 365.2565, for the moon 27.3215, then the inequalities of the earth's motion by the force of Jupiter, will be to the inequalities of the moon's motion, in a given time, in the ratio of 1 to 11229.4.

For the revolution of Jupiter put now that of Saturn, viz. 10759.275 days; and for 1067 the number 3021; then the inequalities of the earth's motion from the force of Saturn, will be to the inequalities of the moon's motion, in a given time, as 1 to 196076.5 And hence it appears that the force of Saturn is to the force of Jupiter, for disturbing the earth's motion, as 1 to 17.46.

Prop. 2. To determine the motion of the Nodes and Apses of the Terrestrial Orbit. By the motion of the nodes of the terrestrial orbit, is meant the motion of the line of intersection of the orbits of the earth and Jupiter and Saturn, made in the plane of Jupiter or Saturn. The motion of the moon's nodes in a sidereal year, according to the astronomers, is 19° 20′ 32″; and this motion drawn into 100, and diminished in the ratio of 1 to 11229.4, by corol. to the preceding prop. is 10′ 20″ 5‴; which increased in the ratio of the cosine of the inclination of Jupiter's orbit and the ecliptic, to the cosine of the inclination of the lunar orbit, that is, in the ratio of the cosine of 1° 19′ 10″ to the cosine of 5° 8′$\frac{1}{2}$, becomes 10′ 22″ 26‴. This therefore is the regressive motion of the earth's nodes in the plane of Jupiter's orbit in 100 sidereal years, by the force of Jupiter. Then this motion 10′ 22″ 26‴ diminished in the ratio of 1 to 17.46, will produce the motion of the nodes, which in the same time is generated by the force of Saturn, in the plane of his orbit, or even in the plane of Jupiter's orbit nearly, viz. 35″ 39‴. Therefore the whole motion of the earth's nodes, from these forces united, in 100 years, in the plane of Jupiter's orbit, is about 10′ 53″ in antecedentia.

Also in the same manner may be collected the motion of the earth's aphelion: for this motion will be, as far as relates to the force of Jupiter, to the motion of the moon's apogee, in a given time, as 1 to 11229.4; therefore if the moon's apogee moves annually 40° 40′ 43″ in consequentia, the earth's aphelion will move annually 13″ 3‴ 28⁗, and in 100 years 21′ 44″ in consequentia. Further, this motion diminished in the ratio of 1 to 17.46, gives 1′ 14″$\frac{1}{2}$ produced by the

orce of Saturn. And the sum of these two, or the whole progressive motion of the earth's aphelion, in 100 years, becomes 22′ 58″½, and its annual motion 13″ 47‴. And this agrees with the more celebrated astronomical tables, which commonly show the annual progress of the earth's aphelion plus minus 1′ 3″, that is, deducting the 50″ regressive motion of the equinoxes, 13″. Q. E. I.

Corol. 1. The linear errors of the planets inferior to Jupiter are, in each of their revolutions, nearly as the forces of Jupiter exerted on these, and as the squares of the times of revolution conjointly: and because the planes of these orbits diverge but little from one another, and from the plane of Jupiter's orbit; the force of Jupiter for disturbing the motion of each, is as the distance of any planet from the sun; hence their angular errors in each of their revolutions, are as the squares of the periodic times, and therefore in a given time as the periodic time itself, or in the sesquiplicate ratio of their distances from the sun. Therefore, putting the motion of the earth's nodes, in 100 years, 10′ 22″½ in antecedentia, from Jupiter's force, and 35″½ from that of Saturn, as above stated; and the periodic times being, for Mars 686.9785 days; for Venus 224.701, and for Mercury 87.9692; hence results the following table:

Motion of the nodes in 100 years, of	From Jupiter's force.	From Saturn's force.	Whole regressive motion.
Mars	19′ 30″	1′ 7″	20′ 37″
Venus	6 23	0 22	6 45
Mercury	2 29½	0 8½	2 38

In like manner, if the earth's aphelion in 100 years, by Jupiter's force, move 21′ 44″ in consequentia, and by Saturn's 1′ 14″½; for the rest of the planets there will be

Motion of the aphel. in 100 years, of	From Jupiter's force.	From Saturn's force.	Whole progressive motion.
Mars	40′ 52″½	2′ 20″½	43′ 13″
Venus	13 22	0 46	14 8
Mercury	5 14	0 18	5 32

Corol. 2. Let ID*d* (fig. 8) represent the orbit of Jupiter, DE the ecliptic, which after 100 years has the situation *d*E, changing the node from D to *d*: draw the arc D*g* perpendicular to *d*E; then will D*d* be to D*g*, as radius to the sine of the inclination of Jupiter's orbit to the ecliptic, that is, as radius to sine of 1° 19′ 10″; therefore, D*d* being = 10′ 58″, as before mentioned, D*g* will be = 15″ 9‴; or rather a star placed in the common section of the ecliptic and orbit of Jupiter, will appear to recede gradually from the ecliptic, so as after 100 years its distance from it will be 15″ 9‴; and so through many ages the star's latitude will be almost equally increased; and in like manner will all the stars, having the same

longitude with Jupiter's node have their latitudes increased or diminished. Therefore these fixed stars from the time of Hipparchus, that is, in about 1900 years, will have changed their latitudes near 5′. In like manner since all the arcs comprehended between the circles DE, dE, and perpendicular to dE, are as the sines of their distances from the point E, or as the cosines of their distances from Jupiter's node, the increment or decrement of the latitude of any star will be to 15″ 9‴, as the cosine of the difference of longitudes of that star, and the nearest node of Jupiter, to radius; and therefore, having given at once the longitude both of a star and of Jupiter's nodes, there will be given the variation of the latitude of the star for any time.

PROP. 3. To determine the Variation of the Obliquity of the Ecliptic arising from the foregoing Forces.—Since, from the preceding proposition, the ecliptic sensibly changes its situation, hence in general it appears that it must also vary its inclination to the equator. In order then to investigate the quantity of this variation, let VED (fig. 9) be the ecliptic; ID the orbit of Jupiter cutting the ecliptic in D; QL the equator; and L the equinoctial point. Let DE and LV be circular quadrants; then if in any particle of time the node D be conceived to be transferred by its mean motion to d, the circle dEt described through the points d, E, will represent the situation of the ecliptic after that time. And if on the same there be demitted the perpendiculars Dg, Vt, the latter Vt will show the variation of the obliquity of the ecliptic generated in the same time. Writing therefore s for the sine of the inclination of Jupiter's orbit to the ecliptic, radius being 1; then, in the triangle Ddg, Dd : Dg :: 1 : s; but Dg : Vt :: 1 : sin. EV; hence Dd : Vt :: 1 : s × sin. EV; and because DE = LV, then DL = EV, and therefore Dd : Vt :: 1 : s × sin. DL; and hence it appears that the momentary variation of the obliquity of the ecliptic is as the sine of the distance of Jupiter's node from the equinox.

Now draw LC to C the centre of the sphere, and on LC the perpendicular DK; then because of the regressive motion both of the node D and of the equinox L, but the equinox with a quicker motion than the node, the points D, L, will mutually approach to or recede from each other, with the difference of the velocities. Suppose therefore either of them, as for instance the node D, to move with this difference of velocities, the equinox L continuing immoveable, and let De be a very small arc described by this difference of velocities, also on LC demit the perpendicular ek; then will De : Kk :: 1 : DK or sin. DL; hence Dd : Vt :: De : s × Kk; and the sum of all the variations Vt, while the point D with the aforesaid difference of the velocities describes any arc DH, will be to the sum of as many motions of the node D, that is, the variation of the obliquity of the ecliptic generated in any time, will be to the motion of the node, as the sum of all the Kk drawn into the sine s, to the sum of as many arcs De, that is, drawing HM

perpendicular to LC, as KM $\times$ s to the arc DH. If therefore n denote the motion of Jupiter's node, while describing the arc DH, the variation of the inclination of the ecliptic to the equator, generated in the same time, will be $\frac{KM \times s \times n}{DH}$. Hence, as $\frac{n}{DH}$ denotes the ratio of the motion of the node to the difference between the motions of the node and equinox, and as KM is the difference or sum of the cosines of the distances of the points D and H from the equinox, according as the points K and M lie on the same or contrary sides of the centre C, there results the following theorem: "Radius is to the sine of the inclination of Jupiter's orbit to the ecliptic, as the difference or sum of the cosines of the distances of the node from the equinox at the beginning and end of a given time, is to a certain sine; then the differences of the motions of the node and equinox, is to the motion of the node, as the sine above found, is to the sine of the variation of the obliquity of the ecliptic.

Also for the node and inclination of Jupiter's orbit, substitute those of Saturn, then the same theorem will give the variation of the obliquity of the ecliptic generated by Saturn. Q. E. I.

COROL. 1. The node D in the figure is the descending node of Jupiter, and the point L the vernal equinox; hence, and from the reasoning in the prop. it appears, that while the node D and the equinoctial L approach each other, the inclination of the ecliptic to the equator decreases; but increases when the node and equinox recede from each other; or, which comes to the same, while the ascending node of Jupiter's orbit passes from the vernal equinox to the autumnal, the obliquity of the ecliptic diminishes, and when the same node passes from the autumnal to the vernal equinox, that obliquity increases.

COROL. 2. If the points D and H be on different sides of the equinoctial points, i. e. if the node in the time proposed pass through the equinox, it appears from the preceding corollary, that the obliquity of the ecliptic will partly increase and partly decrease; in which case the difference of the increment and decrement will be given by the above theorem; and there will also be had the sum of these, or the whole variation of the obliquity generated in that time, if instead of the difference or sum of the cosines of the distances of the node from the equinox, be substituted in the said theorem the sum of the versed sines of the same distances, as is evident. The same mode of reasoning as in both corollaries will also serve for Saturn.

SCHOLIUM 1. As there has been much debate among astronomers, both ancient and modern, whether the obliquity of the ecliptic be constant or variable: but whoever may please to examine the phenomenon by the laws of gravity, he may fully try its investigation by this proposition. Further, as Jupiter's ascending node is now in the sign Cancer, it appears from corol. 1 of this prop. that

for many ages the obliquity of the ecliptic has always decreased. But to show this particularly: the secular motion of Jupiter's node, by prop. 2, is 10′ 22½″; and the annual motion of the equinox being 50″, its motion in the same time is 1° 23′ 20″; therefore the difference of the motions of the node and equinox, is to the motion of the node, as 7.0331 to 1; therefore the time of the node's transit from the vernal equinox to the autumnal, which gives the termination of the diminution of the ecliptic's obliquity, will be in 14803 years, independent of the small acceleration due to the force of Saturn. Jupiter's node then being at present in 8⅓° of Cancer, it appears that for 8000 years (if such be supposed the age of the world) the obliquity of the ecliptic has decreased, and that it will continue to decrease for more than 6000 years longer, and will not recover its first situation till after a period of 29606 years. But the total diminution, which in the aforesaid time can be generated in the obliquity of the ecliptic by the force of Jupiter, produces by the theorem in this prop. 22′ 30″; and therefore this is the maximum of the variation.

If there be required the decrease of the ecliptic's obliquity in the space of the last 1000 years, it will be thus easily computed. The motion of Jupiter's node, by prop. 2, in 1000 years is 1° 43′ 44″; and the precession of the equinoxes in the same time is 13° 53′ 20″, the difference of which motions is 12° 9′ 36″; hence, supposing the place of the node in the beginning of the year 1755 to be in 8° 20′ of Cancer, according to Dr. Halley's astronomical tables, the distances of the node from the equinox at the beginning and end of the given time would be 93° 49′ 36″ and 81° 40′; and hence, by the foregoing theorem is obtained 2′ 22″ 56‴ for the required decrement by the force of Jupiter. In like manner, the motion of Saturn's node, by prop. 2, in 1000 years, is 5′ 56½″; hence the difference between the motion of the node and that of the equinox, is to the motion of the node, as 139.265 to 1; but the distances of the node from the equinox, at the beginning and end of the given time, according to this ratio would be 68° 38′ 24″ and 82° 25′ 48″, supposing the node according to the same tables to be in 21° 21′ 36″ of Cancer, in the beginning of the year 1755; and hence, the inclination of Saturn's orbit to the ecliptic being 2° 30′ 10″, by the same theorem the decrement produced by the force of Saturn, comes out 15″ 2‴. Therefore the whole decrement of the ecliptic's obliquity in the last 1000 years, by the united forces of Jupiter and Saturn, becomes 2′ 38″. So that from the time of Hipparchus the diminution of the obliquity of the ecliptic is about 5′.

Thus also, if Jupiter's ascending node in the beginning of the year 1750, be in 8° 15′ 50″ of Cancer, and Saturn's node in 21° 20′ 6″ of Cancer, as in Halley's tables, the following little table will come out:

From the beginning of the year.	To the beginning of the year.	Decrement of the obliquity of the ecliptic by Jup.	Decrement of the obliquity of the ecliptic by Sat.	Total decrement of the ecliptic's obliquity.
1750	1800	7″ 5‴	0″ 44‴	7′ 50″
1800	1900	14 9	1 27	15 36
1900	2000	14 5	1 26	15 31

Collation of the Theory with Phenomena.

For the proper comparing of the theory with phenomena, the observations of the ancients should be consulted and compared with the moderns; but the former are more imperfect than what can serve for minute considerations of this kind. We can therefore only make use of those of the latter, though less fit for the purpose.

1. M. Le Monnier relates, in the Memoirs of the Paris Academy for 1738, that Picart observed there the altitude of the sun's centre at the summer solstice, and found it in the year 1669 to be 64° 39′, and in 1670 to be 64° 38′ 58″; we shall take the medium 64° 38′ 59″. Le Monnier himself, in the same Memoirs for 1743, found the altitude of the sun's centre there at the solstice to be 64° 38′ 45″. Also the mean place of the moon's ascending node, by the corresponding observations, was about 27° of ♈ at the former time, and 16° of ♉ in 1743; hence in the former case the nutation of the earth's axis was 8″, the whole being 18″, and in the latter 6″ 15‴; which quantities being respectively deducted, the altitude of the sun's centre at the former becomes 64° 38′ 51″, and at the latter 64° 38′ 38″ 45‴, the difference 12″ 15‴ is the decreasing in the mean obliquity of the ecliptic in the interval of 73½ years. By the proposition the decrease from the force of Jupiter in the same interval of time was 10″ 27‴, and by that of Saturn 1″ 5‴. Therefore the whole decrease in the obliquity according to theory was 11″ 32‴.

2. From the observations of Walther, compared among themselves, La Caille (in the Paris Memoirs for 1749) collected that the obliquity of the ecliptic about the year 1496 was 23° 29′ 32″, which at present is stated at 23° 28′ 30″; and therefore in the 260 years the obliquity has decreased about 1′. Now by our theorem that decrease by Jupiter's force would be 37″ 2‴, and by Saturn's 3″ 50‴; hence the whole decrease in the same time becomes 40″ 52‴, or about 41″. If instead of Cassini's table of refractions that of Newton be used, the obliquity of the ecliptic deduced from Walther's observations, will come out a few seconds less, and so come nearer to the determination by theory. But because of the uncertainty of the refractions and of the latitudes of places, it seems that the variation of the obliquity can be most safely determined by observations at the summer solstices made at the same place.

If the variation at length accurately derived from experiments, should, as in the example above, exceed the variation assigned by this theory, that excess will

be due to the actions of the other planets Mars and Venus, which, since the ascending nodes of both are within the first six signs, conspire also to diminish the obliquity of the ecliptic. For which reason, if at any time by observations there can be accurately known both this variation and the progress of the earth's aphelion, then we may also come to know the forces of the planets Mars and Venus, and to weigh their masses.

PROP. 4. To determine the Motion of the Equinoxes due to the preceding Causes.

Here the motion of the equinoctial point is not investigated, so far as to the earth's equator, because of the redundant matter there, may change its situation in respect of the ecliptic by the force of Jupiter and Saturn, like as it can be done by the forces of the sun and moon; for this kind of mutation arising from the actions of Jupiter and Saturn must be insensible: but we inquire after that motion of the equinox, arising from the variation, which we have showed above can be made in the situation of the plane of the ecliptic.

The same things remaining therefore as in the preceding proposition, from the point m, where the equator cuts the circle dE, demit mn perpendicular on DE; then because Dg : mn :: 1 : cos. DL or CK, and Dd: Dg :: 1 : s, it will be Dd : mn :: 1 : CK $\times$ s; or drawing the radius CS perpendicular to CL, and on CS the perpendiculars DR, er, HG, it will be Dd : mn :: De : Rr $\times$ s; therefore the sum of all the mn, while by the difference of the motions of the equinox and node the arc DH is described, will be to the sum of all the Dd, as the sum of all the Rr $\times$ s, is to the sum of as many arcs De, that is, as RG $\times$ s to the arc DH. Therefore the sum of all the mn, that is, the latitude of the equinoctial point, or its distance from the plane DCE considered as immovable, is $= \frac{\text{RG} \times s \times n}{\text{DH}}$, the letter n denoting the motion of the node while the arc DH is described. Now, as RG is equal to the difference or sum of the sines of the arcs DL, HL, according as the points R, G lie on the same or on contrary sides of the centre C, the circle ID representing the orbit of either Jupiter or Saturn, hence will result the following theorem: "Radius is to the sine of the inclination of the orbit of Jupiter or Saturn to the ecliptic, as the difference or sum of the sines of the distances of the node from the equinox at the beginning and end of a given time, to a certain sine: then, as the difference of the motions of the node and equinox, is to the motion of the node, so is the sine just found, to the sine of the variation in the latitude of the equinoctial point." Or again, since the variation in the obliquity of the ecliptic is, from the preceding prop. $= \frac{\text{KM} \times n \times s}{\text{DH}}$, and $\frac{\text{RG} \times n \times s}{\text{DH}}$ is the variation in the latitude of the equinoctial point, we thence have this other theorem: "The variation in the latitude of the equinox, is to

the variation in the obliquity of the ecliptic, as the sum or difference of the sines of the distances of the node from the equinox at the beginning and end of a given time, is to the sum or difference of the cosines of these distances."

And, because it is always, as Ln is to mn, so is the cosine of the obliquity of the ecliptic, to the sine of the same obliquity, or as radius to the tangent of the same obliquity, then will the sum of all the Ln in a given time, i. e. the variation of the equinoctial point according to longitude measured from a fixed point in the plane DCE, be to the variation of the same according to latitude, in the same ratio, and therefore is given. Q. E. I.

Corol. Hence it follows that the variation of the vernal equinox according to latitude, computed from an immovable plane, is always made towards the north, in the passage of the ascending node of Jupiter or Saturn from the summer solstice to the winter; and towards the south when the same node transits from the winter solstice to the summer. The contrary must be said of the autumnal equinox. But the variation of the equinox according to longitude, counted from a given place in that immovable plane, is in the former case made contrary to, but in the latter according to the series of the signs; that is, in the former case the equinox recedes backward, but in the latter precedes forward.

If the prints D and H should be situated on different sides of the solstice, i. e. if in the time proposed the node should transit through the sign cancer or capricorn, the theorems in the prop. will give the difference of the contrary variations of the equinoctial point; and h the sum of these may be had is easily seen.

Scholium. Since in the course of the last 1000 years Jupiter's ascending node has possessed the sign Cancer; and because the aforesaid variations have not been made sensible in the whole of that time, we may inquire what they have been in the 500 years counted backward, from the beginning of the year 1755: in which case the difference in the motions of Jupiter's node and the equinox, is by the scholium to the preceding prop. $6^{\circ}\ 4'\ 48''$; hence proceeding as there for the rest, either theorem in that prop. produces, for the variation of the vernal equinox in north latitude, $6''\ 37'''$, and hence the variation in longitude is $= 15''\ 14'''$, by Jupiter's force.

By adding, in the former case, $2''\ 26'''$ for the force of Saturn, and $5''\ 36'''$ in the latter, then the total variation of the equinoctial point, according to latitude, in the last elapsed 500 years, is $= 9''\ 3'''$, and the regression of the same point $20''\ 50'''$. So that the variations of this kind are not sensible unless in a long interval of time.

Prop. v. To Investigate the Equations of the Terrestrial Errors.

The greatest equations of the angular errors are directly as the forces and the square of the times, and inversely as the diameters of the orbits; and therefore

they are as those errors or motions, of which they are the equations, generated in those times: but the periodic times are very nearly as the equations. Hence, because of the given errors of the lunar and terrestrial motions, and the periods of the equations of the lunar errors, by analogy those of the terrestrial errors will be deduced.

Thus, the period of the equation of the lunar apogee, and of the variation of the equation of the moon's centre, since it is proportional to the sun's revolution at the moon's apogee; and because of the similitude of forces similarly applied, the period of the equation of the earth's aphelion, and of the variation of the equation of the centre, must be proportional to the revolution of Jupiter at the earth's aphelion, therefore those lunar equations will be to these like equations of the earth, as the motion of the lunar apogee, in the time of the sun's revolution at the moon's apogee, is to the motion of the earth's aphelion in the time of Jupiter's revolution at the earth's aphelion; that is, the mean annual motion of the moon's apogee being $40^\circ\ 40'\ 43''$, and the annual motion of the earth's aphelion above found $13''\ 2'''\ 28''''$, as $45^\circ\ 51'\ 40''$ to $2'\ 34''\ 42'''$. Therefore, putting the total variation of the greatest equation of the moon's centre $= 2^\circ\ 41'\frac{1}{3}$, as it is nearly in astronomical tables, the variation of the greatest equation of the centre of the earth or sun will be $9''\ 4'''$. Now, let e denote the greatest mean equation of the sun's centre, then will $e + 4''\ 32'''$ be the greatest equation, and $e - 4''\ 32'''$ the least equation; and by these equations there will also be given the corresponding excentricities.

Then, like as the variation of the greatest equation of the moon's centre increases in the duplicate ratio of the sine of the distance of the moon's apogee from its quadrature with the sun, so the variation of the greatest equation of the sun's centre, that is, the increment of the greatest equation, is augmented in the duplicate ratio of the sine of the distance of the earth's aphelion from its quadrature with Jupiter; or, the variation of the mean equation is to half the total variation, viz. to $4''\ 32'''$, as the cosine of double the distance of Jupiter from the earth's aphelion is to radius; then adding this to the mean equation when the line of the apses of the orbis magnus passes from its octant with Jupiter to the syzygies, or from the syzygies to the octants; in the other parts it is subducted.

In like manner, if the greatest equation of the moon's apogee be stated at $12^\circ\ 18'$, then $45^\circ\ 51'\ 40''$ will be to $2'\ 34''\ 42'''$, as $12^\circ\ 18'$ is to the greatest equation of the motion of the earth's aphelion, or of the sun's apogee, which therefore will be $41''\ 30'''$, viz. where the apses of the earth's orbit is in its octant with Jupiter. In other positions the equation of the aphelion will be to the greatest equation, as the sine of double the distance of Jupiter from the earth's aphelion, is to radius, adding it to the mean motion in the transit of the apses of the

orbis magnus from the syzygies with Jupiter to the quadratures, and subtracting it in passing from the quadratures to the syzygies; and then in any case there will be had the true place of the earth's aphelion or the sun's apogee.

In like manner will the variation of the moon be to the variation of the sun, as the mean motion of the moon's apogee in the time of the moon's revolution at the sun, is to the mean motion of the sun's apogee in the time of the sun's revolution at Jupiter: and therefore, since the motion of the moon's apogee in a synodic time is 3° 17′ 20″, and the motion of the sun's apogee is 14″ 15‴, in which time the sun revolves at Jupiter, putting the greatest variation of the moon at 35′ 10″, the greatest variation of the sun comes out 2″ 32‴, which takes place when the sun is in the octants with Jupiter; in other places the variation will be to the greatest variation, as the sine of double the sun's distance from his quadratures or syzygies with Jupiter, is to radius very nearly.

Also, if the excentricity of the earth's orbit and Jupiter's were the same, the equation of the mean motion of the earth or sun, which arises from the various contraction and dilatation of the orbis magnus by the force of Jupiter, would be to the like equation of the moon, as the motion of the sun's apogee in the time of Jupiter's revolution, is to the motion of the moon's apogee in the time of the sun's revolution, that is, as 2′ 34″ 41‴ to 40° 40′ 43″: but this equation of the sun should be augmented in the ratio of the excentricity of Jupiter's orbit to the excentricity of the earth's orbit, or in the ratio of the greatest equation of Jupiter's centre, to the greatest equation of the sun's centre very nearly, that is, in the ratio of 5° 31′ 36″ to 1° 6′ 20″: hence if the greatest equation of the moon's mean motion be 11′ 50″, the greatest equation of the sun's mean motion will be 2″ 8‴, viz. in Jupiter's mean distances from the sun. In other places it is proportional to the equation of Jupiter's centre. In all these circumstances the force of Saturn is neglected as insensible.

CX. A Journal of the Weather in Dublin for the Years 1753, 1754, 1755. *By James Simon, F. R. S., S. A.* p. 759.

This journal contains the register of the barometer, with the state of the weather, as to wind, rain, snow, &c. for every day in the three years 1753, 4, 5, and that for three times in each day, viz. morning, noon, and night.

CXI. An Account of what happened at Bergemoletto, by the Tumbling down of Vast Heaps of Snow from the Mountains there, on March 19, 1755: *As taken by the Intendant of the Town and Province of Cuneo. Received from Dr. Joseph Bruni, Professor of Philosophy at Turin, and F. R. S. Communicated by Mr. Henry Baker, F. R. S. Translated from the Italian.* p. 796.

In the neighbourhood of Demonte, as in the upper valley of Stura, on the

left hand, about an hour and half distant from the road leading to the castle of Demonte, towards the middle of the mountain, there were some houses in a place called Bergemoletto, which on the 19th of March, in the morning, (there being then a great deal of snow) were entirely overwhelmed and ruined by two vast bodies of snow, that tumbled down from the upper mountain. All the inhabitants were then in their houses, except one Joseph Rochia, a man of about 50, who with his son a lad of 15, were on the roof of his house, endeavouring to clear away the snow, which had fallen without any intermission for 3 preceding days. Whence perceiving a mass of snow tumbling down towards them from the mountain above, they had but just time to get down and flee, when, looking back, they perceived the houses were all buried under the snow. Thus 22 persons were buried under this vast mass, which was 60 English feet in height, insomuch that many men, who were ordered to give them all possible assistance, despaired of being able to do them the least service.

After 5 days, Joseph Rochia having recovered of his fright, and being able to work, got upon the snow, with his son, and two brothers of his wife's, to try if they could find the exact place under which his house and stable were buried; but though many openings were made in the snow, they could not find the desired place. However the month of April proving very hot, the snow beginning to soften, and indeed a great deal of it melted, this unfortunate man was again encouraged to use his best endeavours to recover the effects he had in the house, and to bury the remains of his family. He therefore made new openings in the snow, and threw earth into them, which helps to melt the snow and ice. On the 24th of April the snow was greatly diminished, and he conceived better hopes of finding out his house, by breaking the ice, which was 6 feet thick, with iron bars, and observing the snow to be softer underneath the ice, he thrust down a long pole, and thought it touched the ground; but the evening coming on he proceeded no further.

His wife's brothers, who lived at Demonte, went with Joseph and his neighbours, to work upon the snow, where they made another opening, which led them to the house they searched for; but finding no dead bodies in its ruins, they sought for the stable, which was about 240 feet distant, and having found it, they heard a cry of "Help, my dear brother". Being greatly surprized as well as encouraged by these words, they laboured with all diligence till they had made a large opening, through which the brothers and husband immediately went down, where they found still alive the wife about 45, the sister about 35, and a daughter about 13 years old. These women they raised on their shoulders to men above, who drew them up, as it were from the grave, and carried them to a neighbouring house: they were unable to walk, and so wasted that they appeared like mere shadows. They were immediately put to bed,

and nourishments administered. Some days after the intendant came to see them, and found the wife still unable to rise from her bed, or use her feet, from the intense cold she had endured, and the uneasiness of the posture she had been in. The sister, whose legs had been bathed with hot wine, could walk with some difficulty; and the daughter needed no further remedies, being quite recovered.

On the intendant's interrogating the women, they told him, that their appetite was not yet returned; that the little food they had eaten (excepting broths and gruels) lay heavy on their stomachs, and that the moderate use of wine had done them great good: they also gave him the account that follows: that on the morning of the 19th of March they were in the stable, with a boy 6 years old and the girl about 13: in the same stable were 6 goats, one of which having brought forth 2 dead kids the evening before, they went to carry her a small vessel full of gruel; there were also an ass and 5 or 6 fowls. They were sheltering themselves in a warm corner of the stable, till the church bell should ring, intending to attend the service.

That the wife wanting to go out of the stable to kindle a fire in the house for her husband, who was then clearing away the snow from the top, she perceived a mass of snow breaking down towards the east, on which she went back into the stable, and shut the door. In less than 3 minutes they heard the roof break over their heads, and part of the ceiling of the stable. The sister advised her to get into the rack and manger, which she did. The ass was tied to the manger, but got loose by kicking and struggling, and though it did not break the manger, it threw down the little vessel, which the sister took up, and used afterwards to hold the melted snow which served them for drink.

Very fortunately the manger was under the main prop of the stable, and resisted the weight of the snow. Their first care was to know what they had to eat: the sister said, she had in her pocket 15 white chestnuts: the children said they had breakfasted, and should want no more that day. They remembered there were 30 or 40 loaves in a place near the stable, and endeavoured to get at them, but were not able, by reason of the vast quantity of snow. On this they called out for help as loudly as they possibly could, but were heard by nobody. The sister came again to the manger, after she had tried in vain to come at the loaves, and gave 2 chestnuts to the wife, also eating 2 herself, and they drank some snow water. All this while the ass was very restless and continued kicking, and the goats bleated very much, but soon after they heard no more of them. Two of the goats however were left alive, and were near the manger; they felt them very carefully, and knew by so doing that one of them was big, and would kid about the middle of April; the other gave milk, with which they preserved their lives. The women affirmed, that during all the time they were

thus buried, they saw not one ray of light; yet for about 20 days they had some notion of night and day; for when the fowls crowed, they imagined it was break of day: but at last the fowls died.

The 2d day, being very hungry, they eat all the remaining chestnuts, and drank what milk the goat yielded, which for the first days was near 2lb. a day, but the quantity decreased gradually. The 3d day, being very hungry, they again endeavoured to get to the place where the loaves were, near the stable, but they could not penetrate to it through the snow. They then resolved to take all possible care to feed the goats, as very fortunately over the ceiling of the stable, and just above the manger, there was a hayloft, with a hole through which the hay was put down into the rack. This opening was near the sister, who pulled down the hay and gave it to the goats as long as she could reach it, which when she could no longer do, the goats climbed upon her shoulders, and reached it themselves.

On the 6th day the boy sickened, complaining of most violent pains in the stomach, and his illness continued 6 days, on the last of which he desired his mother, who all this time had held him in her lap, to lay him at his length in the manger, where he soon after died. In the mean time the quantity of milk given by the goat diminished daily, and the fowls being dead they could no more distinguish night and day; but according to their calculation the time was near when the other goat should kid, which as they computed would happen about the middle of April; which at length happened accordingly. They killed the kid, to save the milk for their own subsistence. Whenever they called this goat, it would come and lick their faces and hands, and gave them every day 2lb. of milk.

They say, during all this time, hunger gave them but little uneasiness, except on the first 5 or 6 days: that their greatest pain was from the extreme coldness of the melted snow water, which fell on them, and from the stench of the dead ass, dead goats, fowls, from lice, &c. but more than all from the very uneasy posture they were obliged to continue in: for though the place in which they were buried was 12 English feet long, 8 wide, and 5 high, the manger in which they sat squatting against the wall, was no more than 3 feet 4 inches broad. For 36 days they had no evacuation by stool after the first days: the melted snow water, which after some time they drank without doing them harm, was discharged by urine. The mother said she had never slept, but the sister and daughter declare they slept as usual. The mother and sister say, that on the day they were buried their monthly evacuations were upon them, but they had not the least sign of them afterwards.

CXII. On some of the more Rare English Plants observed in Leicestershire By W. Watson, M. D., &c. p. 803.

This account of some of the more rare plants, growing spontaneously in Leicestershire, was transmitted to Dr. Watson, by Mr. Richard Pulteney, an apothecary at Leicester,* whom Dr. W. describes as a person of real merit, well skilled not only in whatever related to his profession, but also in various parts of natural history. That his botanical knowledge was very extensive, and that he was very zealous in promoting it. That he had already laid before the public, though

* Dr. Richard Pulteney was born at Loughborough in the county of Leicester, on February 17th, 1730. His parents had 13 children, of whom he alone arrived at the age of maturity; and he himself was affected at an early period of life with a pulmonary complaint, which indicated considerable delicacy of constitution. Though the circumstances of the family were easy, yet they did not admit of an expensive education, and the subject of these memoirs was placed in an ordinary elementary school, and was afterwards apprenticed to an Apothecary. His taste for Natural History commenced in very early youth, and in his hours of relaxation he used to wander about the fields, examining the plants that he happened to see in his way. At the termination of his apprenticeship he settled in business at Leicester, where he remained some years. Having commenced an acquaintance and correspondence with Sir William Watson, he was by him introduced to the Earl of Macclesfield, (then President of the Royal Society,) Mr. Hudson, author of the Flora Anglica, and other scientific people; and it was suggested to him by the Earl of Bath, one of his patrons, and to whom he was in some degree related, that his situation in the profession was not adequate to his character. He therefore obtained, with much reputation, a diploma from the University of Edinburgh, in the year 1764. Not long after this period he removed to Blandford in Dorsetshire, where he continued to practise during the rest of his life. He was the author of many useful and ingenious papers, relative to botanical and other subjects, in the Gentleman's Magazine, and of many valuable papers in the Philosophical Transactions; and, conceiving that the science of Natural History in general would be benefited by a *general view of the writings of Linnæus,* he published a most elegant and agreeable work under that title, which appears to have met with its deserved reception and encouragement in the literary world. He afterwards published *historical and biographical sketches of the progress of Botany in England, from its origin to the introduction of the Linnæan system.* This publication is very highly esteemed, and affords a concise and satisfactory view of the gradual improvements of botanical knowledge, and of the lives of its principal cultivators in this kingdom. Dr. Pulteney during his hours of leisure employed himself in the study of Natural History, and was possessed of a very valuable herbarium, a good collection of shells, &c. On the 7th of October, 1801, he was attacked with symptoms of pulmonary inflammation, and of this disorder he died on the 13th of the same month, in the 71st year of his age. He bequeathed his valuable Museum to the Linnæan Society of London.

During 40 years of successful practice, Dr. Pulteney acquired a very considerable fortune, his professional character stood very high, and his whole conduct in private life was exemplary and amiable. In his general intercourse with the world he was somewhat reserved, but to those of his more particular acquaintances, and to the young and inquisitive student, he was always open and communicative. In his person he was rather below the ordinary stature, and somewhat slender, with regular features and countenance, indicating unusual intelligence and superiority of mind. His view of the writings of Linnæus has been republished, with large and curious additions, under the care of Dr. Maton, from whose memoirs the preceding account is chiefly extracted.

his modesty would not permit him to subscribe his name to it, a series of very curiou sand useful observations on the vegetable poisons growing in England; the knowledge of which could not be too much or too generally inculcated.

The plants described in this account were disposed according to the sexual system of Linnæus: but our author had not contented himself with a simple arrangement of the plants, the subject of his work; he had gone further, and had given not only the synonyms of some of the best authors, but, as far as his reading and observations had enabled him, their medical and economical uses, and their places of growth.

Nothing could more tend to the advancement of the natural history of this kingdom, than that persons conversant in the various parts of it, should collect the productions of their own neighbourhood, and transmit accounts of them to the R. S. How much correspondence of this kind had already done, nothing could give a stronger testimony than the Synopsis Stirpium Britannicarum of the late Mr. Ray; as this, joined to his own industry, enabled Mr. Ray to communicate to the public a more perfect account of the plants of this country than any other nation had then seen.

[Then follows the catalogue of Leicestershire plants, which it was deemed unnecessary to reprint, after the complete arrangement and descriptions of English plants by Hudson, Withering, and Smith.]

CXII. An Attempt to Ascertain the Tree that yields the Common Varnish used in China and Japan; also to Promote its Propagation in our American Colonies; and to Rectify some Mistakes Botanists appear to have entertained concerning it. By Mr. John Ellis, F.R.S. p. 866.

As Mr. E. had a favourable opportunity, from his situation opposite to Mr. Christopher Gray's nursery garden at Fulham, to examine his curious collection of exotic plants, he began with the rhus, or toxicodendron, in order to clear up some points disputed in two letters, lately published in the last volume of the Transactions, Art. 27. One from the Abbé Mazeas, on the discovery of the juice of certain species of toxicodendron staining linen of a fine black colour, and the other, in answer to it, from Mr. Philip Miller, of Chelsea, insisting that it was not a new discovery.

To be satisfied of the fact; Mr. E. made several experiments on the 3 species of toxicodendrons, mentioned by the Abbé Mazeas; and found that their juices do stain black, and, if fixed by alum, are not to be washed out by soap, or boiling in a lye of pot-ashes: but the pinnated one, called by the gardeners the poison ash, did not strike so deep a black as the other two trifoliate ones, being more of a rusty colour.

He went next on the inquiry, to compare, and see, whether in reality this

pinnated toxicodendron of our North American settlements, is the true varnish tree of Japan, as asserted by Mr. Miller; and first he found it necessary to know where this poison tree was described. This he was led to by Mr. Miller's letter, where he says the poisonous quality is described in the Philosophical Transactions, N° 367, and a very exact figure of a leaf of it there referred to in Plukenet's Phytographia, tab. 145, fig. 1. See fig. 1, pl. 3.

In order to know what Dr. Kæmpfer has said of this matter, whose words Mr. Miller seems to depend on, Mr. E. carefully translated his description both of the true varnish tree, (fig. 2,) and the spurious one (fig 4;) and found that his description of the true varnish tree, or sitz, does not agree with this toxicodendron, which Mr. Miller supposes to be the same; for the leaf-stalk or midrib of this, that supports the pinnæ or lobe leaves, as well as the under part of the leaves are quite smooth; which is one specific character, that every botanist and gardener knows is necessary to be observed in the proper classing the various species of this genus of plants; many of them being smooth, and many of them downy; whereas Dr. Kæmpfer, speaking of the midrib of his true varnishtree, calls it, "leviter lanuginoso," which may be translated, somewhat downy: and when he describes the under part of the leaves, he says, "dorso incano et molliter lanuginoso," that is, the under part hoary and covered with a soft down.

How far the bottom or lower part of each lobe, or small leaf, answers to the drawing he has given of it, Mr. E. leaves to the curious botanist; for he says it is, "basi inequaliter rotundâ," that is, having some inequality in the roundness of its base: whereas the lobe leaves of the American pinnated toxicodendron come to a point at their footstalks, nearly equal to that at top, as may be seen in Plukenet's figure, (fig. 1,) here copied exactly. He likewise copied minutely, for inspection, Dr. Kæmpfer's figure of his true varnish-tree, as in fig. 2.

Dr. Dillenius, late professor of botany at Oxford, has omitted these necessary characters in his description of the true japan varnish-tree from Dr. Kæmpfer in his Hortus Elthamensis, where he gives it as a synonym for this American pinnated toxicodendron: whereas had he been exact in the description given it by his author, he must evidently have made it another species. This has misled the accurate Linnæus, who quotes Dillenius's synonyms for Kæmpfer's arbor vernicifera, or sitz-dsju.

As another synonym, and in proof of our poison ash, or winged-leaf toxicodendron, being the true japan varnish-tree, Mr. Miller says in his letter, that Mr. Catesby has given a very good figure of it, in his Natural History of Carolina, vol. 1, page 40, where he calls it toxicodendron foliis alatis, fructu purpureo pyriformi sparso (fig. 3;) but as the bare inspection into Catesby's figure of this tree will convince the curious inquirer, whether botanist or not, that it

cannot be the poison ash, known to the gardeners, he observes, besides its having a pear-shaped fruit, that he is persuaded, as are many other persons skilled in these things, that Mr. Catesby never saw the blossom of this tree, so as to determine absolutely its genus, or he would certainly have given it to us; and that he does not once say, that the inhabitants of Carolina call it the poison tree, or even that it grows among them. Mr. E. has given (fig. 3,) a sketch of a leaf, and some of the fruit, which he copied out of Catesby's Natural History, that it may be compared with the other figures, to save the trouble of turning to the original.

How near the jesuit, Father D'Incarville's, Pekin varnish-tree, which he says grows in the province of Nankin, will agree with the figure Kæmpfer has given of his fasi-no-ki, (fig. 4,) or spurious varnish-tree, which Mr. Miller says in his letter are the same, Mr. E. leaves to those gentlemen who may have seen it growing in the curious exotic garden at Busbridge, or at the physic garden at Chelsea; at both which places it has been raised from seed received from the R. S. sent by Father D'Incarville; but lest it may not be in the power of every curious person to take that trouble, Mr. E. sent the figure of one of the leaves, which he drew from a specimen he got in the former garden. As it has not been yet described, he calls it (fig 5.) . "Rhus Sinense foliis alatis, foliolis oblongis acuminatis, ad basin subrotundis et dentatis." The lobes or small leaves are of an oblong figure, pointed at top and roundish at the bottom, where they are remarkably jagged with about 4 teeth. He joined to the figure of this on the same paper an exact copy of a leaf of Kæmpfer's fasi-no-ki (fig. 4,) or spurious varnish-tree. Kæmpfer, takes notice in his, that the middle nerve often divides the small leaves into 2 unequal parts, which is a character Mr. E. has not observed in this China one; nor has he observed that it is of a remarkable fine red in the autumn, as indeed many of the sumachs are; whereas Kæmpfer gives a very poetical description of the striking red of this wild varnish at that season. Dr. Kæmpfer, in the account he gives of his sitz-dsju, or true varnish-tree, takes notice of the effect of its poisonous exhalations; which reminds Mr. E. that this China rhus, when first it began to extend its leaves in the small stove, had so remarkably a disagreeable smell, that he frequently complained of getting the head-ach and a sickness at his stomach by remaining too long near it; and after it was removed into the great stove, where, notwithstanding that building was very spacious, and near 20 feet high, yet, as it grew most luxuriantly, one could not without pain continue long near it. He measured one of the whole great leaves of this tree in the summer 1755, and it was above 3 feet in length. He supposed, as it is a native of Nankin, where the winters are cold, it thrives well in the open air, as it does in the physic garden at Chelsea; where it throws out a great number of suckers.

After Dr. Kæmpfer has described the true japan varnish-tree, he then tells us, that the varnish is collected from it near the city of Jassino, and that it is the best varnish in the world, but that it is in so small quantities, that there would not be sufficient for their own manufactories, were it not for a baser kind of varnish, brought to them from Siam, and called nam-rak. This Siam varnish he says, is got in the province of Corsima and kingdom of Cambodia, from the tree anacardium, called by the inhabitants ton-rak, that is tree-rak. The fruit of this tree he says expressly is called in our shops anacardium; his words are, "cujus fructus officinis nostris anacardium dictus." See fig. 6.

In Mr. Miller's answer to the Abbé Mazeas he says, this varnish is produced from the anacardium, or cashew nut-tree; and recommends it to the inhabitants of our southern colonies in America to draw this varnish from it, as a national advantage. In order to know what kind of tree bears this officinal anacardium, Mr. E. consulted Linneus's Materia Medica, and Species Plantarum; and there he found it a quite different genus of plants from the acajou or cashew nut-tree of Tournefort. He calls this oriental anacardium, avicennia; and has given its characters at large in his Genera Plantarum, and ranks it among the tetrandria monogynia; whereas the occidental anacardium, or cashew nut-tree of the American islands, he calls anacardium, and ranks it among the decandria monogynia.

As the printers or stainers of callicoes in the East Indies make use of some black dye, that holds its colour, and does not impair their cloths, Mr. E. tried some fresh nuts of this oriental anacardium, and found, that not only from his own experience, but lately from the confirmation of many gentlemen in the East India trade, that a fine black colour, which will not wash out, is struck on cotton and linen with the juice of the shell of this nut. They are known all over India by the name of marking-nuts, and are sold for that purpose in their bazars or markets, the figure of which is given in fig. 6. At the same time he tried the acrid oily substance of the shell of some fresh cashew-nuts (fig. 7,) and observed that it gave no colour to linen, but remained like oil of olives on it. He had heard indeed, that the juice of the fleshy fruit that supports the cashew-nut will stain the lips black, and perhaps it may linen; but the gum or liquor which proceeds from the tree, it is agreed by later authors, is of the same nature and mechanical use with gum arabic; and consequently will dissolve in water; which would render it improper for varnish. The figure of the cashew-nut and its fruit are exhibited in fig. 7.

Dr. Kæmpfer further observes, that the quantity of varnish obtained from this officinal anacardium tree is so great, as not only to serve to varnish all the utensils of China, Tonquin, and Japan, but that it is exported in wooden vessels to Batavia, and several other parts of India. It is not improbable therefore that

this is the varnish mentioned by Father D'Incarville in the Philosophical Transactions, vol. 48, p. 254, called tocng-ycou; so universally used in China for preserving and ornamenting their furniture.

Mr. E. confesses that he cannot find, after carefully considering and examining Mr. Miller's letter, that he has brought any proof to lessen the merit of the Abbé Mazcas and the Abbé Sauvage's discoveries: and the use Mr. E. would propose from the remarks he made, is, that as the Premium Society for the encouragement of Arts and Sciences have a scheme on foot to promote the growth of many really useful vegetable productions, which are at present brought to us, at a great expence, from Spain, France, Italy, the Levant, Africa, and the East Indies; he thinks this anacardium orientale, or avicennia of Linneus, claims a place among the rest; especially when we consider of what use and importance it is in the two great empires of China and Japan, besides all the other parts of India. The chief difficulty will be the preserving its vegetative quality during 2 so long voyages; but by many contrivances he was persuaded it will at last be effected; however, the very attempt is laudable.

After writing the above, he received a specimen of the gum of the cashew nut-tree, and found it dissolves in the mouth like gum arabic. It is of the colour of myrrh; but very brittle, shining, and clear. He also procured a specimen of the varnish of China from Mr. Margas, a great dealer in China commodities, just as it was imported from thence; this seems to answer the description of the Siam varnish. He made some experiments on it, and found it did not dissolve by being put either into water or spirits of wine. And further, Dr. Sibthorp, professor of botany at Oxford, informed him, that they have no specimen of the sitz, or true varnish-tree of Japan, in the Sherardian collection, as mentioned by Dr. Dillenius; but that they have one of the fasi-no-ki, or spurious varnish-tree of Kæmpfer, with the synonym, "toxicodendron foliis alatis fructu rhomboide, Hort. Eltham:" inscribed under, "from Japan:" and that it resembles much our American one. So that Mr. Miller's observations on his toxicodendron, or poison ash, may be proper in the 6th edition of his Dictionary, but not in his letter above-mentioned, where he makes the spurious varnish-tree of Japan, or Fasi-no-ki, the same with the Nankin varnish-tree, of which the jesuits of China sent the seed over to the R. S. a few years ago: whereas they are utterly unlike each other. Dr. Dillenius was perhaps led into this error by depending on the report made to Dr. Kæmpfer on the common people of Japan, which was, that the true varnish-tree degenerated into the spurious one for want of culture. But Mr. E. believes our knowledge in this science is so much improved, that such doctrines are not easily admitted among our gardeners (whatever varieties may possibly arise from seed;) and in this he was persuaded Mr. Miller would agree with him, that the 2 sorts of varnish-trees, mentioned by Dr.

Kæmpfer, are 2 distinct species of rhus, or toxicodendron, and will ever remain so, let the soil be either good or bad that they are planted in.

P. S. After writing the above, Mr. E. received a parcel of the officinal anacardiums, which had been lately brought from the East Indies. These have their fleshy fruit with their stalks still adhering to them. The better to illustrate this matter, he gave a figure of one of them in fig. 8. The manner of the growth of this fruit evidently shows that it cannot be the œpata of the Hort. Malab. vol. 4, p. 95, tab. 45, as quoted by Linneus; the whole nut of which is inclosed in a fleshy coat, like an almond. It seems to come nearest to the cassubium sylvestre of Rumphius, Hort Amboin. vol. 1, p. 179, tab. 70; where, besides the figure and manner of growth of the fruit, he mentions that they varnish their warlike and other kinds of wooden instruments, of a black colour, with the milky juice which they draw from this tree; and that they mark themselves on their arms and other parts with the corroding juice of the nut, which continues a long time before it disappears.

Rumphius further particularly describes this plant to be of the pentandria monogynia of Linneus's method; so that it must differ entirely from the anacardium occidentale, which belongs to the decandria monogynia of that author. He likewise makes this remark, that the cashew-tree, or occidental anacardium, is not a native of the East Indies; but has been brought thither by the Portuguese, from the Brasils: and that they are no where to be found in those parts, except where they have had their settlements.

CXIII. On the Present Increase of the People in Britain and Ireland. By William Brakenridge, D. D., Rector of St. Michael Bassishaw, London, and F. R. S. p. 877.

Dr. B. intends the present communication as a supplement to his two former papers on the number of persons in London, lately printed in these Transactions. Having re-considered the subject, he thinks it may be proved, that there is no increase at all from both our British isles, after the deduction of our losses; and that in England, taken by itself, the natives would be in a decreasing state, if it were not for the supplies from Scotland and Ireland. As this seems to be of some importance to discover, because of its consequence with regard to policy, and the influence it may sometimes have, he endeavours to show it as plainly as the present circumstances of things will allow.

Dr. Halley has shown, from his table of the probabilities of life at Breslau, that the number of men able to carry arms in any country, between 18 and 56 years of age, or, as they are called, the fencible men, may be estimated as a 4th part of the whole people, children included. From which it demonstrably follows, that the 4th part of the annual increase will likewise be the increase of

the fencible men; and that their increase or decrease will always be in that proportion. And therefore, if in England the annual increase of the people does not exceed 18000, as he had before proved from the proportion of births and burials, and the whole number being 6 millions, the annual increase of the fencible men will not be above 4500. But in Scotland and Ireland this increase may be reasonably supposed to be more, in proportion as there are more marriages than in England. And therefore, to avoid any uncertainty in calculation, we will suppose the annual increase in those countries, to be double in proportion. That is, as we have from observation, assumed the births to be to the burials at 112 to 100 at an average through England, we will now allow them in Scotland and Ireland to be as 124 to 100; where the difference, which is the increase, is double to the other, and by which the whole people would be doubled in about 114 years; which is surely as much as can be supposed. And then, by the method that has been shown in his last letter, if the people in both countries do not exceed 2,500,000, the annual increase will be found to be 15,000, and that of the fencib e men will be 3750.

From the account given in the Philosophical Transactions, N° 261, the number of people in Ireland, in the year 1696, did not appear to be more than 1,034,000; since which time he thinks there has been little increase; and in Scotland they are supposed to be less than 1,500,000; and so both together they cannot be reckoned at more than 2,500,000: and therefore the annual increase of the fencible men cannot possibly be more than 3750, in both countries; which with those in England will be 8250, for the annual increase in Britain and Ireland, or a little above 8000 men. And no reasonable computation can make them more. The number then 8250 may be considered, at the utmost, as the yearly increase of the fencible men; from which all our public losses in our ordinary commerce at sea, and in our wars by land and sea, and by our colonies, are to be deducted. And it is plain, if in all these ways our losses are annually equal to about 8000 men, there can be no increase at all of our fencible men; and consequently no increase of our people, which must always be in proportion to them; but if our losses are more, we must be in a decreasing state.

To make a just and moderate estimate of our losses, it will be proper that we take 50 or 60 years at an average, to avoid any uncertainty. And if we begin at the year 1690, which is 66 years ago, we shall find, that during that time, in our commerce at sea, and in our wars by land and sea, we cannot have lost less than 450,000 men. To show this, it may be observed that in all bodies or armies of fencible men, which consist generally of those between 18 and 56 years of age, there dies annually about one in 54, by the natural decrease of life, as appears from Dr. Halley's table. And therefore, if there are 80000 sea-

men or more, as is said in Britain and Ireland, the natural decrease, which is not here to be considered, will be about 1480 or 1500 annually. But the number must be much greater that is lost, by the various contingencies of the sea, by wreck, scurvy, and the inclemency of different climates, &c. for fewer cannot be supposed to be destroyed by such incidents, than the double of those that may be by natural mortality. He thinks there must be more; for if a ship goes a voyage for a year with 100 men on board, and returns only with the loss of half a dozen, she is reckoned to have made a healthy voyage, though the loss is above 3 times what might be expected from natural decrease; that is, though the loss by the sea only may be considered as double the other. And it often happens, that by sickness there will be much more than this, besides all the other hazards of the sea. Our ships of war in long cruising have generally a greater consumption of their people: so that our losses by sea are rather undervalued, when they are estimated to be the double of what is from the natural decrements of life. And, if this be allowed, the loss by the various contingencies of the sea will be more than 3000 annually, over and above the number that might die by natural casualties if they were at home; and in 66 years it must be 198,000.

And as to our losses by war at land and sea, of our own people, they are commonly reckoned to be 300,000, in all the three French wars, since 1690: but if we abate 50,000 from that number, that we may reason with more safety, they cannot be less than 250,000; for in all those wars, that taken together were about 20 years, there must be more than 10,000 lost yearly by land and sea. And therefore, by our commerce and wars, from that time mentioned, we have at least lost about 448,000, or 6800 annually. In which are included those who died by fatigue, and other hardships, as well as those in actual engagements.

And if we add to this, the number that is constantly and secretly drawn from Ireland, for foreign military service and on the account of religion; and likewise those taken from Scotland, for our regiments in the Dutch service; all which cannot be less than 500 yearly, though some have thought it to be double this, we shall then appear to have lost 7300 annually, since the year 1690. To which if we put the loss of those who go from hence to our colonies, and other settlements, particularly to Jamaica and the East Indies; and, last of all, the number we have lost by the use of spirituous liquors; it will be plain, that our whole loss cannot be less, but more than 8250 annually; which is at most the yearly increase of our fencible men: and therefore that there has been no increase at all of our people these last 66 years; but rather perhaps a decrease, though it cannot be ascertained with any precision.

Now if this can be proved, as he imagines it has, that there is no increase of our people in Britain and Ireland, because of our losses, we may make this un-

pleasant reflection, that our country can never be fully peopled, while our losses continue so great as they have been these last 60 years. And as the greatest part of those losses above-mentioned belong to England, because of its much greater trade, and the greater number of its people, it may be considered as in a decreasing state with regard to its natives; and, if it were not supplied from Scotland and Ireland, the decrease would be plainly discovered. For, as the people in England are double to those in both the two other countries, its losses must be in that proportion at least, or about 5300 annually, two-thirds of the whole; which is more than the increase of its fencible men.

From the above calculation we may likewise see, how small the annual increase of fencible men may be in Britain, or perhaps in any other country in Europe. For as that increase in both our islands does not appear to be more than 8250, but rather less, or about 7900, and the number of our whole people in them is not found to exceed 8,000,000, the annual increase in each million must be less than 1000, or about 987; that is, less than one in a thousand; though we have allowed the increase in Scotland and Ireland to be double in proportion to what it is in England. And from this we may form a good rule, by which we may judge of the increase, or decrease of other nations. For though they may be supposed to increase perhaps faster than we do, by more frequent marriages, the annual increase of their fencible men will not generally exceed 1000, for every million of people. And therefore, according as their losses by war, or other devastations are fewer, or exceed 1000 fencible men annually, for every million of their people, they are either in an increasing or decreasing state; and for every 1000 men that are lost, there is the increase of a million for one year destroyed; which it were to be wished, that princes would attend to, in their ambitious schemes, by which they make such havock of mankind.

Now, to account for the cause of the want of increase in our British isles, it seems to be chiefly owing to three things, that operate together. The fashionable humour that greatly prevails, by which above one-third of our people in England above 21 are single, occasioned by a variety of circumstances; and to our wars and commerce at sea, which are rather beyond our natural strength, by destroying more of our people than can well be spared, and which, if preserved, might improve our country, and augment our power; and lastly, to the use of spirituous liquors, by which numbers have been and daily are lost. But there may be easy remedies for two of those evils, by a little attention of the legislature; which would greatly conduce to the public happiness.

P. S. Since the above was written, Dr. B. had been certainly informed, that from the survey lately made of the window lights, after the year 1750, there are about 690,000 houses charged to that tax in England and Wales, besides

cottages that pay nothing. And though the number of cottages is not accurately known, it appears from the accounts given in, that they cannot amount to above 200,000. And therefore there are not in England and Wales more than 890,000 houses, or 5,340,000 people, allowing 6 to a house; which well agrees with what he has said in this and his former letter. For if the survey made before the year 1710 was near the truth, from which it appeared, that there was not above 729,048 houses, besides cottages, or 929,048 houses in the whole; which will make about 5,570 000 people; then there must have been no increment since that time, but rather a decrease, notwithstanding the continual supplies from Scotland and Ireland, and from foreigners.

The argument from which he inferred, that there is a decrease of the inhabitants within the bills, is this; that, before the year 1743, for 20 years, the burials in them were at an average above 27,000, and the baptisms between 15,000 and 17,000; but since that time they are both gradually decreased; so that now the burials are about 22,000, and the baptisms between 14,000 and 15,000, taken at an average for 10 years: and therefore these different numbers, continued so long, cannot come from the same number of people; but that as the burials and baptisms are both decreased, the whole people must be also diminished. In his first letter he reasoned, and made his calculation, on the same principles with Sir William Petty, Mr. Graunt, and other approved authors. From a continued increase in the bills they inferred, that there must be a proportional increase of inhabitants; and Dr. B. from the continued decrease in them, in the same circumstances, endeavoured to prove a similar decrease of people. If their reasoning is just, his cannot be false; and if the bills never again appear so high, as formerly for a continuance, in healthy times, it will be he thinks a demonstration.

CXIV. A Letter to the Rev. William Brakenridge, D. D., F. R. S. with a Table of the Value of Annuities on Lives; by Mr. James Dodson, F. R. S. Dated Dec. 8, 1756. p. 891.

Rev. Sir, as I have made a great many calculations, relative to annuities on lives, and have otherwise contributed, as much as was in my power, to facilitate the performance of such, I thought it almost a duty incumbent on me, to compute the values of them according to your curious table of the decrements of life, inserted in the Philosophical Transactions; accordingly I have inclosed a table of them, as follows, &c.

A Table of the Value of an Annuity of one Pound, payable yearly, during a Life of any Age, allowing compound Interest at 4l. per Cent. per Ann. computed from the Table of the Decrements of Life, constructed by the Reverend William Brakenridge, D. D., F. R. S. Inserted Page 601, *vol.* x.

1	12,510	16	17,000	31	14,433	46	11,580	61	8,071	76	3,895
2	15,001	17	16,855	32	14,280	47	11,360	62	7,839	77	3,708
3	16,001	18	16,716	33	14,090	48	11,190	63	7,541	78	3,603
4	16,781	19	16,525	34	13,890	49	10,960	64	7,234	79	3,468
5	17,470	20	16,379	35	13,740	50	10,780	65	6,920	80	3,261
6	17,712	21	16,221	36	13,580	51	10,540	66	6,598	81	3,146
7	17,800	22	16,061	37	13,420	52	10,350	67	6,338	82	2,923
8	17,821	23	15,865	38	13,210	53	10,170	68	6,007	83	2,508
9	17,800	24	15,688	39	12,960	54	9,938	69	5,670	84	2,084
10	17,781	25	15,504	40	12,780	55	9,694	70	5,399	85	1,651
11	17,671	26	15,313	41	12,590	56	9,444	71	5,139	86	1,210
12	17,560	27	15,112	42	12,400	57	9,186	72	4,895	87	0,762
13	17,453	28	14,951	43	12,190	58	8,918	73	4,677	88	0,320
14	17,291	29	14,784	44	11,980	59	8,643	74	4,395		
15	17,171	30	14,612	45	11,800	60	8,361	75	4,130		

CXV. Of an Earthquake felt at Cologn, Leige, Maestricht, &c. on the 19*th of November,* 1756. *By Mr. Abraham Trembley, F. R. S. Translated from the French.* p. 893.

There was felt, at 3 in the morning, a shock of an earthquake, at Cologn, Leige, Maestricht, in the country of Limburg, and, as appears, in all that between the Meuse and the Rhine. One of almost the same kind was felt in the same places on the 3d of June. You saw by the account, sent on the 11th of May, that earthquakes were very frequent in this country in the beginning of this year. The shock, which has been lately felt in these parts, as well as in Portugal, shows that the cause of earthquakes is still active. Persons very attentive observed, that in the neighbourhood of Lisbon for several days after the 1st of November, 1755, those who lay on the gronnd, perceived a motion under them, which they compared to the beating of the pulse. They mention likewise, that after this motion ceased to be felt, there was perceived another, which they compare to that felt in a boat on a river, the current of which is very slow. Those only who lay on the ground were sensible of this motion; for such as were sitting on chairs, or standing, perceived nothing of it.

CXVI. An Account of a Treatise, in Latin, presented and dedicated to the R. S., intitled, " Gottlob Caroli Springsfeld, M. D., &c. Commentatio de Prærogativa Thermarum Carolinarum in Dissolvendo Calculo Vesicæ præ Aqua Calcis Vivæ. By William Watson, M. D. at Madrid, and F. R. S. p. 895.

Dr. Springsfeld's Treatise, which he lately communicated to the R. S. contains a series of experiments and observations on the Carlsbad waters in Bohemia, as a

solvent for the stone in the bladder; from which it appears that these waters have that property in a much higher degree than even lime-water. The Carlsbad waters have been long celebrated for their excellent effects in removing, or at least relieving, many of the disorders to which mankind is subject. How high they stood in the opinion of the great Hoffinan, almost every part of his writings bears testimony; and if to their other before-known properties they should prove a safe, easy, and effectual solvent for the stone in the kidneys and bladder, it certainly would greatly enhance their value.

Our author has very attentively considered the writings of Doctors Jurin, Hales, Hartley, Whytt, and others, concerning solvents for the stone. He has administered to several patients, with little or no success, the late Mrs. Stephens's medicine, with the strictest observance of all the cautions, said to be necessary in courses of that medicine. And though he allows every thing to be true that has been laid down by Dr. Whytt and others, in regard to oyster-shell lime-water, he does not scruple to assert, that the Carlsbad waters, which have great analogy to calcareous waters, are a far more excellent solvent for the stone in the kidneys and bladder, than any lime-water. Of this truth he is satisfied by various experiments, several of which were made by himself alone, and others in conjunction with our learned and ingenious brother Dr. Lieberkuhn, whose exactness, as well as fidelity in making experiments of this kind, no one will question.

Dr. Springsfeld, in a treatise on the Carlsbad waters, published by him in the year 1749, has shown by undoubted experiments, that these waters partake always of an alcaline principle; for every pint of them, besides the neutral purging salt, contains 3 grains of alcaline salt, and 10 grains of calcareous earth;* for which reason they ferment with every species of acids. Dr. S. before mentioned that these waters have great analogy with lime-water; and if they continue in the baths for any considerable time, they not only turn milky, like lime-water, but have a pellicle on them as that water is observed to have. They have likewise a gently constringing taste; that were it not for their saline taste they could not easily be distinguished from lime-water.

It must here be premised, that all hard bodies, viz. pieces of wood, bone, stones, earthen vessels, bits of straw, and such like, are incrusted over by lying in the Carlsbad waters, and that in a very little time. These bodies in the space of a night will be covered with a tophaceous crust, which continually increases. But human calculi, though hard in themselves, are not incrusted by them; but

* The solid ingredients in the Carlsbad waters are carbonate of lime (mild calcareous earth) sulphate of soda (Glauber's salt) muriate of soda (sea salt) carbonate of soda (mild mineral alkali) and a minute portion of iron. See the analysis of these waters by Bergman (1778) by Becher (1789) and by Klaproth (1790).

are rather dissolved; which is the more remarkable. The same effects are observed on pieces of the hardest cheese, which swell in these waters, and are changed into a kind of poultice.

In the treatise before us, the author has given the detail of many experiments, which prove the solvent power of these waters. Dr. W. lays a few of them only before the Society, from which an opinion both of the author's exactness in making them, as well as how far he is justified in his conclusions, may be formed. And here Dr. W. observes, which should be a very comfortable consideration for the inhabitants in these parts, that the author has been obliged frequently to suspend his researches for want of human calculi, which is a disease exceedingly rare in Bohemia.

June 20, 1749, a stone of a brown colour, which weighed near 2½ oz. was placed in a China basin near that source, which is called Prudel, in such a manner, as to be continually covered with the warm water. On the next day the external crust began to grow soft; on the 3d you might make an impression on it with your nail, as upon cheese; on the 4th and 5th, it was dissolved to the nucleus; on the 6th, the nucleus itself was dissolved, and in the bottom of the basin there was left a white viscid mass, like poultice, or newly steeped cheese: this was impalpable between the fingers. In this time the basin was incrusted with a very hard tophaceous mass, of the thickness of a quill. Certain calculi, not larger than peas, were dissolved thoroughly, some in one day, and the rest in two.

1750, June 12, a stone weighing more than ½ oz. was placed in the same manner as the former, and not a grain of it remained on the 4th day. At this time a clergyman, who was in a course of these waters for gouty complaints, voided 6 stones, all which were dissolved in the same manner.

A nobleman, who was afflicted with bloody urine, from calculi in the kidneys, came to Carlsbad for the relief of his complaints; and brought with him some small calculi, which he had voided a few years before. By Dr. Lieberkuhn's advice Dr. Springsfeld divided these calculi into 4 equal parts, each of which weighed 6 grs. One part of these was infused in the water of the source called Prudel; the 2nd, in the new spring; the 3d, in that near the mill. In 12 hours the first part had lost 5 grs. the 2nd, 4 grs. and the 3d, only 1 gr. The 4th portion was put upon a linen rag, which was stretched over the bottom of a funnel. Into this funnel the nobleman was directed to make water every day before dinner, after his having drank his quantity of Carlsbad water. On this, these calculi, after 8 days, had lost 2-3ds of their weight; viz. 4 grs. It must be here remarked, that this nobleman, during the regimen, voided several small calculi, which he had not done for some years. A larger quantity of bloody urine than usual attended the parting with these stones; but this continued only 2 or 3 days, and afterwards went quite off; and this nobleman from that time

was relieved from his former complaints, has enjoyed and does yet enjoy the most perfect health.

In the year 1754, the author became possessed of a calculus, which was of a flinty hardness, and bore a bright polish. It weighed a quarter of an oz. He conjectured, that a much longer time would be necessary to dissolve this stone; but, what was very remarkable, it dissolved sooner than the rest: for after having been immersed 24 hours, 2 grs. of it only remained undissolved. This stone was not placed in the China basin as the others were, but suspended in a little loose-woven net, that it might more freely be washed by the water. Dr. Lieberkuhn was at this time at Carlsbad; he was present at this experiment, and was witness of its truth. The net used in this experiment was covered with a tophaceous crust, from being steeped in the water.

The next year, when Dr. Lieberkuhn returned to Carlsbad, he brought with him, for experiment sake, several calculi, some of which were large ones. He made there many experiments, in which our author assisted. A large stone was sawed into 4 pieces, nearly equal. One of these weighing 99 grs. was put into a little linen bag, and immersed in the source called Prudel: the 2nd in like manner, which weighed 96 grs. into that called the new spring: the 3d, weighing 93 grs. into that near the mill: the 4th was set apart for other trials. After 4 days immersion they were severally examined. The first had lost 85 grs. the 2d, 33 grs. the 3d, only 16 grs. That it might be estimated in what degree the solvent power of the Carlsbad water exceeded that of lime-water, the following experiment was tried. Three pieces of calculi, each exactly 30 grs. in weight, were put into separate phials. On one was poured some fresh egg-shell lime-water: on the 2d some Carlsbad water: on the 3d, some of the urine of a person daily drinking these waters for the recovery of his health. These phials were all placed in one of the canals, which carries off the waste water from the baths: the degree of heat in this place was by Fahrenheit's thermometer 96, much the same as the heat of human blood. The lime-water, the Carlsbad water, and the urine, were changed every day, and the process continued for 14 days. On the 15th, the remaining fragments of stone were taken out of the phials, and weighed when dried. The piece macerated in lime-water had lost 1 gr. that in the Carlsbad water, 6 grs. that in the urine, 5 grs. According therefore to this experiment, the solvent power of the Carlsbad water was 6 times, that of the urine 5 times greater than that of the lime-water.

The solvent power of medicated urine is of very great importance, and requires more particular attention; as our greatest expectations in dissolving the stone in the bladder must arise from that. It was therefore very fit that our author should investigate, as far as was in his power, the solvent property of the urine of those who drank these waters. He therefore suspended to the end of a

funnel a sufficiently hard and compact calculus, weighing about 1 oz. This was contained in a linen rag, so that the urine might readily pass over it; and a person, who used the Carlsbad waters every morning, after having taken them, constantly made water into that funnel; whence it came to pass, that on the 16th day the stone was half dissolved, and the remaining part was become so porous and friable, that it almost fell to pieces. No one can suppose that the urine of a man perfectly in health would have the same solvent property; lest however, that should happen, our author suspended a piece of a calculus, weighing 2 drs. in the same manner with the preceding, and made water upon it himself many times a day; but this piece of calculus, after 12 days, was so far from being lessened, that it had increased 2 grs. in weight.

Our author, lest he should be thought to have depended too much on one set of experiments, made others. Among several calculi, which Dr. Lieberkuhn had communicated to him, there was one exceedingly hard. This he cut into 4 parts, each weighing exactly 80 grs. Each of these was put into a separate phial. On the first was poured fresh oyster-shell lime-water: on the 2nd, Carlsbad water: on the 3d, the urine of one who drank these waters: on the 4th, the urine of one perfectly in health, and who only drank for his breakfast some cups of tea. These phials were placed in the same manner with those before-mentioned, and their heat kept constantly the same. Every day these calculi had fresh liquid poured on them after the old was separated. At the end of 20 days these stones were dried and weighed. The fragment infused in oyster-shell lime-water was found to have lost almost 3 grs. that in Carlsbad water 22 grs. that in medicated urine 14 grs. but that infused in the urine of the man in health had increased 3 grs. These experiments therefore leave no room to doubt of either the solvent power of the Carlsbad water itself, or that of the urine of those who drink these waters.

Our author has a very curious remark in regard to a person who laboured under the stone, and who drank these waters for 2 months. He daily voided with his urine a large quantity of white viscid mucus; which, after filtration of the aqueous parts from it, was found to be a white earthy powder, rubbed off as it were from a stone. The quantity of this powder saved during the space of a month, amounted to more than 3 oz. If some of this powder was put into the urine of one who drank Carlsbad water, it was immediately converted into a poultaceous substance; but if into that of one who did not drink this water, it fell quite undissolved to the bottom of the vessel.

Dr. Springsfeld observes, that the Carlsbad water has great power in dissolving the tophaceous crust which frequently covers the teeth. During the course of these waters, this crust most generally separates from the teeth, and falls off. However great the power of these waters are in dissolving the stone in the

bladder, they have a quite contrary effect on gall-stones. So far from dissolving these last, our author has frequently found that these waters envelope them with their tophaceous crust. Dr. Whytt has observed, that lime-water has no solvent power on gall-stones. Hence we draw another proof of the analogy of lime-water with Carlsbad water.

If it should be wondered at, how it comes to pass that the urine of those who drink these waters should have the power of dissolving the stone, it is necessary to inform our readers, that this urine contains nearly the same properties which the water originally had. It has before been observed, that these waters are impregnated with an alkaline principle, and consequently ferment with acids. The urine of those who drink them, if made before dinner, has the very same quality as our author has frequently experienced; especially if the accustomed quantity of water is taken, and nothing else is drank upon them. The customary dose at Carlsbad is not less than 6, 7 or 8 pints of water taken every morning: for which reason we are not to wonder that the urine has the property of dissolving the stone in the kidneys and bladder, if it be long retained. And our author makes no doubt but that the injection of these waters into the bladder, would be very powerful in relieving calculous complaints; though this he had never tried; neither was he much induced to it, as the urine is possessed of all the powers which he was in search of.

It remains that we just take notice, by what means these waters are possessed of their solvent power. It is well known that acids, more especially mineral ones, dissolve animal calculi, by acting on their terrestrial parts, dividing their masses, and thus becoming neutral. These effects do not arise from alcalies, as they leave terrestrial substances untouched. If sometimes we carefully attend to the operations of nature, we now and then make discoveries which must otherwise have escaped us. If we pour nitrous or vitriolic acid on that stony substance called crabs-eyes, and let them remain in the glass for a considerable time perfectly still, we shall find at the bottom of the vessel, after the terrestrial parts are thoroughly dissolved, a membranous substance or jelly, exactly in size and figure resembling the crabs-eyes, and which the acid had left untouched. Exactly such a gelatinous mass our author has observed in stones of the bladder, more particularly in small ones, after dissolving them in acids. When crabs-eyes are infused in an alkaline lixivium for a considerable time, we see no change in them, which can be properly called a solution: about them we observe a certain viscid appearance like a cloud; when that is taken away, and the crabs-eyes are dried, and afterwards weighed, they have not only lost part of their weight, but are become much more friable; which is a great argument that they have lost something. When afterwards these crabs-eyes are washed with warm water, to carry off the alkaline matter adhering to them, and afterwards set to dissolve in acids,

these crabs-eyes, after the solution of their terrestrial parts, leave nothing gelatinous behind them, as they did in the other experiment; whence it is plain that the gelatinous substance had been extracted, and dissolved, by the alkaline lixivium. The very same thing happens to the human calculus.

It appears therefore more than probable to our author, that lime-water and Carlsbad waters, on account of their alike partaking of the alkaline and calcareous principle, do dissolve the before-mentioned animal gluten only, by which the terrestrial parts are united together; and on the solution of which these parts must separate and fall asunder. Hence may be accounted for also the origin of that white viscid matter, which adheres to the bottom of the vessel like poultice, after the dissolution of calculi in the Carlsbad waters; and which is nothing more than the terrestrial parts of the stone deprived of the animal gluten, which makes them adhere together. Hence we see the reason why our predecessors adopted two sorts of lithontriptic remedies, and those of quite opposite properties. Basil Valentine, Paracelsus, Helmont, and others, administered alkalies; Sylvius, Laurembergius, and Dippelius, acids. By these last they attempted to dissolve the terrestrial parts; by the former, the connecting gluten. But the case in gall-stones is different; their connecting gluten, which unites the bilious parts, is not an animal jelly, as in the calculus vesicæ, but a fat inflammable oil, which is neither dissoluble by the Carlsbad waters, nor by lime-water.

Our author conjectures that he has proved demonstratively, that the solvent power of the Carlsbad waters does exceed that of lime-water; besides which it has this advantage, that it is not in the least nauseous, and may be continued, if necessary, for 6 or 8 months, without any other inconvenience than that of drinking them on the spot; which may indeed oblige persons whose dwellings are remote from Carlsbad to take a journey thither; whereas lime-water may be drank at home.

END OF THE FORTY-NINTH VOLUME OF THE ORIGINAL.

Art. I. Of the Earthquake felt in New England, and the Neighbouring Parts of America, Nov. 18, 1755. *By Mr. Professor Winthrop of Cambridge in New England. Vol. L. Anno* 1757. p. 1.

This earthquake happened about a quarter after 4 in the morning of Nov. 18, 1755. The night had been perfectly calm and serene. The earthquake began with a roaring noise in the N.W. like thunder at a distance; and this became fiercer as the earthquake drew nearer; which was almost a minute in coming to this place. (Cambridge, in New England). The first sensation was like a pulse,

or an undulation, resembling that of a long rolling, swelling sea; and the swell was so great, that a person was obliged to run and catch hold of something, to prevent being thrown down. The tops of 2 trees close by him, one of which is 25, the other 30 feet high, he thinks waved at least 10 feet; and there were 2 of these great wavings, succeeded by a smaller one. This sort of motion, after having thus continued about a minute, abated a little; then presently the shock came on again with redoubled noise and violence; though the species of it was altered to a tremor, or quick horizontal vibratory motion, with sudden jerks and wrenches. The bed, on which Mr. W. lay, was now tossed from side to side; the whole house was prodigiously agitated; the beams cracked, as if all would presently be shaken to pieces. When this had continued about 2 minutes, it began to abate, and gradually kept decreasing, as if it would be soon over; however, before it had quite ceased, there was a little revival of the trembling and noise, though not comparable to what had been before: but this presently decreased, till all by degrees became still and quiet. Thus ended this great shock. It was followed by another about an hour and a quarter after, viz. at 5^h 29^m. This, though comparatively small, was very generally perceived, both as to its noise and trembling, by those who were awake. On the Saturday evening following, viz. the 22d of November, at 27^m after 8, there was a 3d, more considerable than the 2d, but not to be compared with the first. And on Friday the 19th of Dec. in the evening, exactly at 10 o'clock, there was a 4th shock, much smaller than either of the former, though like them preceded by the peculiar noise of an earthquake. The whole lasted but a few seconds; but the jarring was great enough to cause the window-shutters and door of the room to clatter. And the course of it was nearly from N.W. to S.E.

An account received from the West-Indies, agrees very well with the supposition that the earthquake proceeded south-eastward. The account is, that "on the 18th of November, about 2 o'clock in the afternoon, the sea withdrew from the harbour of St. Martin's, leaving the vessels dry, and fish on the banks, where there used to be 3 or 4 fathom water; and continued out a considerable time; so that the people retired to the high land, fearing the consequence of its return; and when it came in it rose 6 feet higher than usual, so as to overflow the low lands. There was no shock felt at the above time." As this extraordinary motion of the sea happened about 9^h after our great shock, it seems very likely to have been occasioned by the same convulsion of the earth. Now if this earthquake went off south-eastward into the Atlantic, it must have passed considerably to the eastward of St. Martin's; and in fact it did not reach that island, there being no shock felt there. The motion of the sea then was owing to a great agitation raised at a considerable distance in some part of the ocean, where the earthquake passed, and thence was propagated to that island. Nor is the

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length of time greater than what seems necessary for this effect. The earthquake itself, at the rate it moved with us, would be some hours in going from hence to the distance of St. Martin's: for sound would be about $2\frac{1}{4}^h$ in moving to such a distance; and the progress of the earthquake was slower than that of sound, as appears from hence, that the roar of this earthquake arrived here near a minute before the shake. The rest of the 9^h might well be spent in conveying the motion excited in the water, from the place where it was excited to St. Martin's; for the waves raised by it could not move with near the velocity of sound.

It is worthy of remark, that of the 5 great earthquakes, which this country has felt since its settlement by the English, 2 have gone nearly in the same track as this last did. The first, which was on June 2, 1638, "came from the northward, and passed southward." By the description given of it, it was very much like our late earthquake, only perhaps not quite so violent. "The noise and shakes of the earthquake, October 29, 1727, seemed," it is said, "to come from the north-westward, and to go off south-easterly; and so the houses seemed to reel." As to the great earthquakes of 1658 and 1662, we have no account of the courses which they went in. But from the other three, it may be reasonably conjectured that the source of our earthquakes, or the place in which they originate, is in some part of Canada, or perhaps beyond it.

The extent of this earthquake seems to have been greater than that of any in this country. The province of the Massachusetts-bay, or rather the province of New Hampshire, about the latitude of 43° north on the sea-coast, seems to have been the centre of it, or the place of its greatest violence; and the shake to have been less considerable each way from hence towards the s.w. and N.E. By the accounts we have from the s.w. the shock was less at New York than it was with us; and still less at Philadelphia, which is farther towards the s.w. By the best information, the limit toward the s.w. was Chesapeak-bay in Maryland, the shock having been felt on the eastern side of that bay, and not on the western. For the other limit toward the N. E. we are informed, that the earthquake was felt at Annapolis Royal in Nova Scotia, though in a much less degree than with us. It shook off a few bricks from the tops of some chimneys, but was not perceived by vessels on the water. And a letter from Halifax says, "The earthquake, which happened in the w. extended itself to this place, though scarcely perceivable here." But it was not at all felt by our army, which lay encamped at Seganecto, about 100 miles N. from Halifax. Thus Halifax seems to have been very near the N.E. limit. I am not able to ascertain its eastern and western limits; but it extended to all our back inland settlements; and was perceived, though in a very small degree, by our army at Lake George, distant from hence about 130 miles N.W. by W. But it was not felt at all at the British fort of

Oswego, situate on the south-eastern shore of Lake Ontario, and distant from hence about 250 miles w. by n. So great was the shock in the Atlantic, 70 leagues to the e. of Cape Anne, that the people on board a vessel there were suddenly surprised, just at the time of our earthquake, supposing they had run a-ground; till, on throwing over the lead, they found they had more than 50 fathom water. The extent of the earthquake e. and w. from Halifax to Lake George, was about 550 miles; and its extent along the sea-coast, from n.e. to s.w. at least 800 miles. But if the agitation of the water at St. Martin's was occasioned by our earthquake continued into the Atlantic, as was conjectured above, its extent, in a direction toward the s.s.e. must have been at last 1900 miles.

As to the effects of this earthquake, besides the throwing down of glass, pewter, and other moveables in the houses, many chimneys were levelled with the roofs of the houses, and many more shattered, and thrown down in part. Some were broken off several feet below the top, and by the suddeness and violence of the jerks canted horizontally an inch or two over, so as to stand very dangerously. Some others were twisted, or turned round in part. The roofs of some houses were quite broken in by the fall of chimneys; and the gable ends of some brick buildings thrown down, and many more cracked. Throughout the whole country, the stone fences were more or less thrown down. The vane upon the public market-house in Boston was thrown down; the wooden spindle, which supported it, about 5 inches in diameter, and which had stood the most violent gusts of wind, being snapped off. A new vane, on one of the churches in Boston, was bent at its spindle 2 or 3 points of the compass; and another at Springfield, distant about 80 miles westerly from Boston, was bent to a right angle. A distiller's cistern, made of plank, almost new, and very strong put together, was burst to pieces by the agitation of the liquor in it; which was thrown out with such force, as to break down one whole side of the shed, that defended the cistern from the weather; as also to stave off a board or two from a fence at the distance of 8 or 10 feet from it. In some parts of the country, particularly at Pembroke and Scituate, about 25 miles s.e. from hence, several chasms or openings were made in the earth, from some of which water issued, and many cart-loads of a fine whitish sort of sand. These are the principal effects of this earthquake on the land, some of which argue a very quick and violent motion of the earth.

But the agitation occasioned by this earthquake was not confined to the land; it was very sensible on the water, and even at considerable distances in the ocean. The vessels in our harbours were so shaken, that it seemed to those who were in them, as if they were beating on the bottom. Some that were in the bay, coming in from sea, thought they had run upon rocks or sands. One very uncommon

effect of this concussion is related by several of our seafaring men, that almost immediately after the earthquake, large numbers of fish of different sorts, both great and small, came up to the surface of the water, some dead, and others dying.

II. The Strange Effects of some Effervescent Mixtures. By Dr. James Mounsey, Physician of the Russian Army, and F.R.S. Dated Moscow, Sept. 20th, 1756. p. 19.

Mr. Butler, a paper-stainer, trying to make some discoveries for the better fixing of colours, was put in danger of his life by the following experiments: having put into one gallipot $\frac{1}{4}$ oz. of verdigris, and into another pot 2 leaves of false gold leaf, to each he poured about a spoonful of aquafortis. They began immediately to ferment, especially the gold-leaf. He was very assiduous in stirring them, to make the solution perfect. Having nothing else at hand, he did this with a pair of small scissars, at arm's length, carefully turning away his face, to prevent the fumes from entering his lungs. He was called away, about other business, before he had quite ended his process; and soon after washed and shifted himself: but had scarcely finished before he felt a burning pain in the ring-finger of his right hand, which he imputed to his having inadvertently touched the aquafortis. This increased every moment, and affected the whole hand with burning pain and swelling, which very soon subsided: but then it flew into the left hand, and a few minutes afterwards, into the insides of his legs, as if scalding water had been thrown on them. His stockings being immediately pulled off, there appeared a great many red spots, as large as sixpences, something raised above the skin, and all covered with very small blisters.

In about 2 hours after the accident, Dr. M. first saw him: he was very uneasy, complaining of pain, and great anxiety, at the pit of the stomach, as if a burning hot iron was laid on it, as he expressed himself. His pulse was regular, but slower and weaker than natural: he had a nausea, and complained of a very coppery smell and taste. Dr. M. ordered some alkaline volatile medicines, and to drink small sack-whey. He vomited once, and had 4 or 5 stools, and then his stomach became easy. But the scene soon began again with lancing pain in the left eye. He continued the same medicines, drank plentifully of the whey, and was kept in a breathing sweat, by which he found some ease at night: but whenever the sweating lessened, the burning pains returned in broad flakes, changing from one part of the body to the other; sometimes with shootings in his eye, and sometimes along the penis, but he had no heat of urine. His pulse continued regular, but weak; and in several places of his body such kind of spots struck out as those on his legs.

Monday, the 3d day, in the morning, after sleeping well, his pulse was some-

what raised, and he continued easy till about 11 o'clock, when the burning pains returned, shooting from place to place; but always so superficial, that he could not distinguish whether it was in or under the skin. Rubbing the part affected with the hand gave ease; but when the sweating went off, and the burnings and shootings became insufferable, Dr. M. always put him into a bath of hot water, with some wood ashes, kept ready in the room; which gave him great relief. That afternoon he felt violent burning pain in his great toes, and sometimes in his left hand, with shootings up to the shoulder. Once he cried out, in great pain, that his shoulder was burst; for he felt something fly out with a sort of explosion; but examining the part Dr. M. found nothing particular. He observed, when the flaky burnings began, they were as if they kindled from a point, and flashed like lightning, as he termed it. He was very often tormented with such pains on the pit of the stomach; and that evening had shootings through the back, with a pain in the belly. He complained of a strong sulphureous smell, which he said was like to suffocate him; though his breathing seemed easy, and his lungs no way affected. In the night he was seized with great pain about the heart, and cried out violently that his heart was on fire: but after taking a dose of nervous medicines, and being put into the bath, he was soon freed from this, and passed the rest of the night tolerably well. At the time of such violent attacks the pulse continued regular, but still slower and softer than usual.

Tuesday, he complained most of his toes, and now and then burning pains in the forehead. Wednesday, the whole day it continued most in the toes of the left foot; but in the evening the pain on the stomach returned, which lanced to the left side, with dartings inwardly. He became so uneasy and restless, that Dr. M. was obliged to add some opium to the other medicines; which answered very well. Thursday, the pains kept most in the toes of the left foot. Friday, nothing particular, except his feeling, with sharp pain, a spark (as he called it) fly out of his right cheek, in the same way, he said, as that which burst on his shoulder, but much less. He perceived no pain in that part before; nor any thing after, besides a soreness, which lasted for some days. Hitherto he had been kept in a continual sweat: his appetite was greater than his allowance; his digestion good, and his rest indifferent. From this time he was not attacked by any violent symptoms; and could be quiet, though he did not sweat. On Sunday he began to get out of bed; but was often seized with glowing pains, suddenly affecting different parts of the body; which seldom continued an hour in one part, but shifted from place to place: these he was troubled with, in a less degree, even long after he went abroad.

By care and watchfulness the violence of the symptoms were kept under; and by the use of antidotes for poisons of the nature of what he received this

from, the disease was overcome, and the patient recovered his perfect health and strength.

III. On the Good Effects of Malvern Waters in Worcestershire. By J. Wall, M.D. p. 23.

Among other remarkable instances of the medicinal effects of the Malvern water, Dr. W. relates the following: A poor woman, formerly a patient in the Worcester Infirmary for a fistulous ulcer in the hip, and another in the groin, which penetrated the abdomen, received her cure at Malvern, though she was reduced to such a degree, as to be thought incurable, and sent into the country on a milk-diet, &c. as the last resource. The discharge from the sores was prodigiously great, and so offensive, that she could hardly be borne in a room. The water took off the ill smell almost instantly; the discharge soon lessened, and grew thick and well-conditioned; her hectic symptoms went off in proportion; and by continuing the use of the water for 5 or 6 months, she was cured.

A woman with a phagedenic ulcer in the cheek, throat, and nose, from an ozæna in the hollow of the cheek-bone, received great relief in 5 or 6 weeks time; the external ulcer, which had almost destroyed the whole cheek, being healed in that time, and the other parts much amended. Her affairs would not permit her a longer continuance at the well; but she continued the use of the water at home, and found great relief from it there. Dr. W. hoped another season would complete the cure.

Mr. Parry, of Clent, had his skin cleared, and perfectly healed in 5 weeks; though when he came to the well, he was covered with an elephantiasis; for which he had tried most of the purging waters, and sea-water, under the direction of Dr. Russell, without effect. So bad was he, that he could not move a limb but the skin cracked, and oozed out a filthy sanies; and he left the mark of his body every night in his bed.

Dr. W. knew a lady who he had great reason to fear had an internal cancer, who had received great advantage from the use of these waters, after other things had been tried unsuccessfully.

IV. Of the Carlsbad Mineral Waters in Bohemia. By the Rev. Jeremiah Miles, D. D., F.R.S. p. 25.

Carlsbad is a small town on the confines of Bohemia, at the distance of 14 German, or 28 French leagues west of Prague. It is remarkable for its warm mineral springs, which are said to have been accidentally discovered, in the year 1370, by the Emperor Charles the 4th, as he was hunting: from whom they

received their present name of Carlsbad, or Charles's bath.* These waters, soon coming into repute, occasioned the building of a small neat town, consisting chiefly of houses calculated for the accommodation of the company, who frequent this place in the summer-time. There are 2 warm springs, which rise in the middle of the town, very near each other; and though they are supposed to be of the same quality, yet as one is much warmer, it is thought likewise to be more efficacious than the other. The former of these, called the Prudel, rises very near the bed of the small river Tepel, which runs through the middle of the town, and is sometimes overflowed by it. The water issues with great force from the bottom of this spring, rising in a considerable body to the height of 6 feet perpendicular; and would force itself much higher, if it were confined within a narrower compass. This spring is so impetuous, that they are obliged to pave and ram the bed of the river, lest it should force itself up in the channel: and Dr. M. observed one place on the river side, where it had burst through the rock; and they had been obliged to confine it, by fastening down a large stone on the orifice. The water is so hot, that you cannot bear your hand in it; and the inhabitants make use of it for scalding their pigs and their poultry.

The water, when put into a glass, has a bluish cast, not unlike that of an opal; and though Dr. M. could not discover, that in 24 hours it had deposited the least sediment, yet there was a thin whitish scum collected on the surface; and he observed the same in the baths, where it was much thicker; and was of the colour, and almost of the consistence of a wafer. It has a salt taste when first taken from the water, and is used by the inhabitants for cleaning of teeth and scouring silver: it is called Baden Flaum.

Though this water does not deposit any sediment, yet it is remarkable for the speedy and strong incrustation of all bodies which are put into it. Little plaster figures are sold here, on purpose to verify the experiment; which, though perfectly white when put into the spring, are in 48 hours entirely covered with a yellowish incrustation. The same effect is observed on the pipes and channels, through which the water is conveyed. If care were not taken to clean them 4 or 5 times a year, they would be entirely choaked up; and in some parts, where it has not been necessary to clean them so often, he had seen them covered with an incrustation 2 inches thick. In surrounding and covering these wooden pipes, they do not change the nature of the wood; but it is observable, that they add great hardness and solidity to it: so that it is affirmed a piece of deal will last 100 years in this water. The head spring is cleared out once in 30 or 40 years, at a very great expence; at which time they are obliged to break off all the stony

* Some account of these mineral springs, being an abstract from Dr. Sprengsfeld's treatise, has been inserted in a former vol. of these Trans. See p. 57, of this volume of these Abridgments.

incrustation, which had been made by the water since the last cleaning; and if neglected would (as it has sometimes actually done) choak the passages, and oblige the spring to find vent in some other place. The incrustations formed by these waters are of different kinds: that which is made in the troughs and pipes, through which the water is conveyed after it comes above ground, is of a light sandy nature, of a loose contexture, and a bright yellow. It is used by the inhabitants as a gentle corrosive for eating off proud flesh. There is another of a darker colour, and a much harder nature, found at the very mouth of the spring, where it bursts out of the rock. There are other sorts taken out of the subterraneous cavities of the spring at the time it was cleaned. They seem to be an alabastrine spar, and are beautifully marked with straight veins of different colours, which may be supposed to have received their tinge from the different colour of the spring water at the time whan this sediment, or rather scum, was formed on it. They find pieces of this kind most beautifully variegated; and some of them large enough, by fineering, to make tables: these polish very well, and are not much inferior to jasper in appearance. It is a part of the manufacture of the place, to work this part of stone into snuff-boxes, cane-heads, and sleeve-buttons.

There is another sort of incrustation, different from all these, which was found some years before in digging for the foundations of the new parish church, which is about 300 yards distant from the Prudel spring. They found there the same kind of water; but it did not rise with so great force as in the other spring: and they discovered in the cavities large masses of a stony concretion, which were a sort of pisolithi, most of them in a globular, but some in an oval form, from the smallest size to that of a nutmeg; the former sort lying in masses, the latter generally single and detached: they are perfectly white, hard, and smooth, and appear to consist of a great number of lamellæ formed round a small nucleus. This sort of incrustation has been found in no other place; but there are some of a browner sort, and more irregular shapes, which are taken out of the Prudel.

The medicinal virtues of these waters have been treated of by German authors. They are esteemed to be particularly efficacious in removing obstructions, and in cases of the stone and gravel; of which the treatise lately produced to the Society contains many remarkable proofs. They are much frequented in these and other cases; so that they have generally 200 persons in a season drinking the watrrs. The season begins in May, and ends in August. They drink them in the following method. They begin with a purge; and assist its operation with 10 or 12 chocolate-cups of the water, taken within 5 minutes of each other. The day following they take the waters in the same quantity, and at the same intervals, keeping themselves all the time in a warm room; which, with

the warmth of the waters, occasions a most plentiful perspiration. This is repeated for 7 or 8 days, increasing daily 2 or 3 cups of the water, till they come to drink 25 or 30 cups a day. The operation continues from 8 o'clock in the morning till noon. Some bleed once in the middle of the course, others not at all. After they have finished this course of drinking, they bathe 2 days successively, continuing in the bath $\frac{1}{2}$ an hour, or longer, as their strength permits them, or their case requires. This is the whole course; which is repeated 2 or 3 times, or oftener, as they find necessary. The whole is concluded with a gentle purge, though the waters themselves are of a laxative nature.

There is another spring in the town of the same nature, but not so warm, as the Prudel: it is called the mill-spring, and is only tepid. Those of a warm or weak constitution make use of this instead of the other, both for drinking and bathing.

There are likewise several chalybeate springs in the neighbourhood of Carlsbad; one at $\frac{1}{2}$ a mile, and the other at 2 leagues distance from the town. Both of them seem to resemble the water of the Pohun spring at Spa; but are not near so strong. They are not used medicinally on the spot; but are brought to Carlsbad, and sold, in order to be drank with their wine.

V. An Essay towards Ascertaining the Specific Gravity of Living Men. By Mr. John Robertson, F.R.S. p. 30.

To make some experiments on this subject, Mr. R. got a cistern made, of 78 inches in length, 30 inches wide, and 30 inches deep; for men of different sizes to be immersed in. He then endeavoured to find 10 persons, such as he proposed to make the experiments with; namely, 2 of 6 feet high, 2 of 5 feet 10 inches, 2 of 5 feet 8 inches, 2 of 5 feet 6 inches, and 2 of 5 feet 4 inches. One of each height he proposed should be a fat man, and the other a lean one; but he could not succeed in procuring such men; and, after waiting till near the middle of October, was obliged to put up with such as would submit themselves to the experiment at that season of the year. They were all labouring men, belonging to the ordinary of Portsmouth yard, and, except one or two of them, who were middling sized men, were for the most part very thin and slim made persons. A ruler, graduated to inches, and decimal parts of an inch, was fixed to one end of the cistern, and the height of the water noted before a man went in, and to what height it rose when he ducked himself under its surface; and of these several observations is the following table composed.

No.	Heights Ft.	Heights In.	Wt. Pds.	Ht. Water before immersed Inches.	Ht. Water when immersed Inches.	Water raised. Inches.	Solidity.	Weight Water. Pounds.
1	6	2	161	19.30	21.20	1.90	2.573	160.8
2	5	$10\frac{3}{8}$	147	19.25	21.16	1.91	2.586	161.6
3	5	$9\frac{1}{2}$	156	19.21	21.06	1.85	2.505	156.6
4	5	$6\frac{3}{4}$	140	19.17	21.21	2.04	2.763	172.6
5	5	$5\frac{7}{8}$	158	19.13	21.21	2.08	2.817	176.0
6	5	$5\frac{1}{2}$	158	19.09	21.26	2.17	2.939	183.7
7	5	$4\frac{3}{8}$	140	19.05	21.06	2.01	2.722	170.1
8	5	$3\frac{1}{8}$	132	19.01	20.86	1.85	2.505	156.6
9	5	$4\frac{1}{8}$	121	18.97	20.76	1.79	2.424	151.5
10	5	$3\frac{1}{4}$	146	18.93	20.66	1.73	2.343	146.4

One of the reasons, that induced Mr. R. to make these experiments, was a desire of knowing what quantity of fir or oak timber would be sufficient to keep a man afloat in river or sea water, thinking that most men were specifically heavier than river or common fresh water; but the contrary appears from these trials: for, excepting the first and last, every man was lighter than his equal bulk of fresh water, and much more so than his equal bulk of sea water: consequently, could persons, who fall into the water, have presence of mind enough to avoid the fright usual on such accidents, many might be preserved from drowning; and a piece of wood, not larger than an oar, would buoy a man partly above water so long as he had spirits to keep his hold. Some things herein advanced will perhaps more readily appear from the following relation.

The Lords of the Admiralty have appointed, for the exercise of the scholars belonging to the royal academy at Portsmouth, a small yacht; wherein, during the summer months, those young gentlemen are taught the practice of working a vessel at sea, under the directions of one of the master-attendants, assisted by 8 or 10 seamen. The last time this yacht was out, which was about the beginning of last October, one of the scholars was ordered to heave the lead. The youth was about 13 years old, small of his age, and far from being fat; as he was stepping on the gunnel, he fell over-board: the sea was rough, and the yacht had great way; so that he was presently at a considerable distance from the vessel. The skiff was immediately let down; but the painter not being fast, the rope ran an end, and the skiff went adrift. One of the seamen jumpt overboard, got into the boat, brought her alongside the vessel, took in another man, and then went after the youth, whom they recovered, after he had been in the water more than half an hour. The young gentleman, relating the affair, said, that as he could swim very little, and judging he should sink if he strove against the waves, he turned on his back, and committed himself to their mercy. He kept himself perfectly calm; and observed, when a wave was likely to break over him, to hold his breath, and to spurt out the water forced into his mouth. His hat, which happened to be tied by a piece of string to one of

his coat button-holes, he often held up with his hand, as a signal where he was. Just before the boat came up to him he began to be faint, his eyes became dim, and he thought himself on the verge of sinking. This youth, who by his prudence saved himself from drowning, must at that time have been specifically lighter than water.

VI. An Instance of the Gut Ileum, Cut through by a Knife, Successfully Treated by Mr. Peter Travers, Surgeon, at Lisbon. Communicated by John Huxham, M. D., F. R. S. p. 35.

Aug. 3d, Antonia Josée da Costa, one of the king's messengers, was attacked by 2 men, and, after receiving 2 blows on the head, was stabbed with a knife in the right hypogastric region, about 3 fingers breadth above the os pubis; the external wound being larger, as the knife was drawn obliquely towards the navel, and might be an inch and half in length, the perforation through the peritonæum about $\frac{3}{4}$ of an inch; the intestine ileum hanging out about 10 or 12 inches, and quite pierced through, the wound in the gut being large enough to admit a finger. After clearing the grumous blood with warm water and Hungary water, the uninterrupted suture was made on both perforations; then dilating the common integuments of the belly, the intestine was reduced, leaving the ends of the 2 threads at the superficies of the wound; and the external incision was sewed up by the interrupted suture, and common dressings of lint and bandage applied. A clyster was given him immediately after the above operation, of oil of olives, the yolk of an egg, and warm water.

4th. This day Mr. T. found he had passed in the most excruciating pains, attended with continual vomitings: his fever very high, pulse full and irregular: he was bled 10 oz. this morning, and the like quantity this evening. The clysters were continued thrice a day, with a decoction of wormwood and camomile, instead of the warm water, and an anodyne mixture of mint-water, liquid laudanum, and sugar to be taken occasionally; also 3 oz. of syrup of rhubarb, with an ounce of the fresh drawn oil of sweet almonds, to be taken, a common spoonfull every 2 hours.

5th. The bleedings were continued twice this day, 3 oz each time, and the clysters were administered as yesterday. His pulse and fever very high; he vomited some excrements; and towards night complained of a singultus.

6th. His bleedings and clysters were continued as before. Finding his singultus and vomiting so very troublesome, he ordered him Dr. Huxham's tincture of the bark; which was taken, a tea-spoonful, 6 times a day, in a little mint-water; which greatly relieved him: his singultus and vomiting became less frequent.

7th. Mr. T. found his skin moist, and pulse softened. He remained with

him about an hour, and found a plentiful perspiration throughout the body; on which he omitted his bleedings: the clysters were continued; and towards night he had a proper discharge by stool, very fœtid, and inspissated.

8th. Mr. T. found, for the first time, he had slept last night, and seemed much in spirits: the symptomatic fever something lessened; and he had purged last night, and this day, 8 times.—On the 9th he had 5 stools; his neausca much abated; and a gentle diaphoresis continued.

10th. The singultus ceased; his vomiting very little; his pulse low, accelerated, and thread-like in its stroke; his purging violent; and he greatly complained of a most acute pain of the wounded parts. A paper of the following absorbent powders was given him every 3 hours in rice-water. Crabs-eyes and red coral prepared, of each 1 dr. crude opium 2 grs: these were made for 3 doses, and given as above.—11th. He slept well; less pain; pulse more equal; his diarrhœa much the same.

12th. The threads, with which Mr. T. had made the suture of the intestine, came out of themselves: the wound well-conditioned; fever very little; his diarrhœa rather increased. He sent for Mr. T. in the evening, being much alarmed, as he thought some liquids he had taken had passed through the wounded parts.—13. Yesterday he complained of great pains in his belly: the discharge from his wound was laudable matter, and in good quantity.

14th. He rested well, and was seemingly well beyond expectation. His diarrhœa still continuing troublesome, he took the hartshorn decoction, with an addition of diascordium.—15th. Mr. T. cut off the threads of the external wound, and continued dressings of digestive in the common method.—16th. He grew visibly better each day after; and on Sept. 7th Mr. T. discharged him from any further attendance, his wound being entirely healed over, and he was in all respects very well, free from pain, or any inconvenience from the wound. He was kept 27 days on chicken-broth, and never admitted to use any solids during that time: afterwards he was indulged with young chickens, &c.

VII. An Account of a Visitation of the Leprous Persons in the Isle of Guadaloupe. By John Andrew Peyssonel, M. D., F. R. S. Translated from the French. Dated Aug. 10, 1748. p. 38.

About 25 or 30 years before the year 1748, a very particular disease appeared in many persons in the island Grande Terre. Its beginning is imperceptible: there appear but a few livid red spots on the skins of the white people, and of a yellowish red on the blacks. These spots in the beginning are not accompanied with pain, or any other symptom; but nothing can take them away. The disease increases insensibly, and continues several years in showing itself more and more. These spots increase, and extend indifferently over the skin of the whole

body. Sometimes they are a little prominent, but flat. When the disease makes a progress, the upper part of the nose swells, the nostrils are enlarged, the nose becomes softened; tuberosities appear on the cheek-bones; the eye-brows are inflated; the ears grow thick; the ends of the fingers, and even the feet and toes, swell; the nails become scaly; the joints of the feet and hands separate and mortify; ulcers of a deep and of a dry nature are found in the palms of the hands and soles of the feet, which grow well, and return again. In short when the disease is in its last stage, the patient becomes frightful, and falls to pieces. All these symptoms come on by very slow degrees, one after another, and sometimes require many years to show themselves: the patient is sensible of no sharp pain; but feels a kind of numbness in his hands and feet. These people perform their natural functions all the while, eating and drinking as usual: and when even the mortification has taken off the fingers and toes, the only ill consequence that attends, is the loss of those parts that drop off by the mortification; for the wound heals of itself; without any application: but when it comes to its last period, the poor sick persons are horribly deformed, and truly worthy of compassion.

This shocking disease is observed to have several other unhappy characters; as, 1st, that it is hereditary, and that some families are more apt to be seized with it than others: 2dly, that it is infectious; being communicated per coitum, and also caught by keeping company with those so diseased: 3dly, that it is incurable, or at least that no remedy has yet been found to cure it. They have in vain tried mercurials, sudorifics, and every other regimen used in the venereal complaints, under a notion, that this leprosy was the consequence of some venereal taint: but, instead of being of service, these methods rather served to destroy the patients; for, far from lessening the disease, the antivenereal medicines unlocked the distemper, the most dreadful symptoms appeared, and all those so treated perished some years sooner than the others, who did not take these medicines.

A very just fear of being infected with this cruel distemper; the difficulty of examining infected persons before the disease came to its state; the length of time of its lying concealed, by the care of the patients to keep it secret; the uncertainty of the symptoms, which distinguish it in the beginning; produced an extraordinary dread in all the inhabitants of this island. They suspected one another, since virtue and merit had no shelter from this cruel scourge. They called this distemper the leprosy; and consequently presented several memoirs to the generals and intendants, laying before them all these facts above-mentioned; their just apprehensions; the public good; the trouble that this distrust caused in this colony; the complaints and hatred that these accusations occasioned among them; the laws made formerly against such leprous persons, and their

expulsion from civil society. They required a general visitation of all persons suspected of this distemper, that such as were found infected might be removed into particular hospitals, or into some separate places. These memorials were sent to court, which, giving due attention to these just representations, issued orders for the required visitations in the most convenient manner, for the good of the public and of the state.

In the mean time the post of physician-botanist became vacant in the island of Cayenne. The minister was pleased to name Dr. P. for it; and though this island was much more fertile in philosophical discoveries than all the others, he thought proper to change Dr. P.'s destination, and sent him to Guadaloupe; and did not forget the article of the leprosy in his instructions. When Dr. P. arrived at Martinico in 1727, Mons. Blondel de Juvencourt, then intendant of the French isles, communicated to him both the orders of the court, and all the memoirs that related to this affair. A tax was then laid on the Negroes of the inhabitants of the Grande Terre, to raise a necessary fund for this visitation, thus made at the expence of the colony; and Mons. le Mercier Beausoliel was chosen treasurer of this fund.

Being arrived at Guadaloupe, the Count de Moyencourt, and Mons. Mesnier, ordinator and subdelegate to this intendance, communicated to him the orders of the general and intendant. Dr. P. began then to inform himself of the necessary instructions for acquitting himself of this dangerous commission, the disagreeable consequences of which he easily foresaw. He had so often heard of these leprous spots, that he judged it necessary to know, whether what was said was true: for he could not comprehend that a disease, which has so dreadful an end, and the symptoms then so terrible, should continue 10 or 15 years without any other appearance than these simple spots, which in themselves had nothing very bad. He demanded an inquest to be made, in order to satisfy himself of this fact: several surgeons, as practitioners, and several honest inhabitants, as observers, were accordingly called together, who all proved the same fact in this inquest; which may be seen in the register of the subdelegation of this island.

Result of the Visitation.

1. None of the patients, whom Dr. P. visited, had any fever; and they all declared, that they found no inconvenience nor pain; but, on the contrary, eat, drank, and slept well, performing every natural function; which was proved by their plumpness, which appeared even when the disease was most confirmed. 2. The disease began to show itself in the Negroes by reddish spots, a little raised, on the skin, being a dry kind of tetter, neither branny nor scabbed, and without any running, but of a livid red, and very ill-conditioned. The Negroes sometimes bring these spots with them from their own country. The spots are constantly found on every person troubled with this disease; and are in greater

numbers, in proportion as the disease becomes more inveterate. 3. Among the whites the disease shows itself at the beginning by spots of a livid violet colour, without pain; which are followed by little watery bladders, particularly on the legs, which burst, and leave small ulcers with pale edges, and different in their natures from the common ulcers. 4. In proportion as the disease increased, the hands and feet grew larger, without any signs of inflammation; since neither redness, nor pain, nor any œdematous appearance accompanied it; but it was the very flesh, that increased in bulk. And this growth of the hands and feet was not attended with any sharp pain, but only a kind of numbness. 5. This bloated state of the hands and feet was succeeded by white deep ulcers under the skin, which became callous and insensible; and which emitted only a clear serous matter like water, and were but little painful. Afterwards the ends of the fingers became dry, the nails became scaly, and as it were eaten away; the ends of the fingers dropped off; then the joints separated without pain, and the wounds cicatrised of themselves, without the least need of medicines. In the increase of the distemper, hardnesses and lumps were formed in the flesh, the colour became tarnished, the nose swelled, and the nostrils grew wide, at last the nose softened like paste, the voice became hoarse, the eyes round and brilliant, the forehead covered with tetters and lumps, as well as the face; the eyebrows became very large, the countenance was horrible, the breath fœtid, the lips swelled, large tubercles were formed under the tongue; the ears grew thick and red, and hung down; and such was the insensibility of all the parts, that pins were run through the hands of several, without their feeling any thing of it. In short, he was assured, that these people perished by degrees, falling into a mortification; and the limbs dropped off of themselves, without much pain, such persons continuing still to perform well their natural functions. 6. These leprous people lived thus easy, as it were, for several years, even 15 or 20; for the disease begins insensibly, and shows itself but very slowly. 7. Antivenereal remedies, which were ordered for almost every patient, were of no service; if they sometimes palliated some symptoms, they very often hastened the progress of the disease: besides they never found the parts of generation at all infected, nor any thing that looked like the pox about them. 8. Some of these people had indeed particular symptoms. In some the hair fell off; which was replaced by a finer kind: in others, worms were found in their ulcers: want of sleep, or frightful dreams, afflicted some, while others quite lost their voice, or it became effeminate like that of eunuchs; and others stunk extremely. 9. Almost all of them, being desirous of concealing their disorders, endeavoured to deceive the commissioners, by alleging false excuses for the causes of their sores and ulcers: the greater part of them pretended that the rats had eaten off their toes, and that burns had caused their ulcers. 10. The commissioners were confirmed in their opinions

by experience, supported by verbal process, that this was the state of the diseased; that the distemper could neither be the pox, nor the effect of an inveterate one: that it had no symptom of that disease; but that it had every character of what the ancients called leprosy, elephantiasis, or such other names, as they were pleased to give it. So that they did not hesitate to pronounce, that those infected with this disease, as above described, ought to be treated as leprous persons, and subject to the ordinances, which his majesty was pleased to issue against such persons. 11. Again, they were well assured, from their observations, that the distemper was contagious, and hereditary; and yet the contagion is not so active, nor poisonous, as that of the plague, small-pox, nor even as the ring-worm, itch, scald, and other cutaneous disorders: for, if that were the case, the American colonies would be utterly destroyed; and these persons so infected, mixed as they are in every habitation, would have already infected all the Negroes whom they come near. 12. They believed that this contagion did not take place but by long frequenting the company of the infected, or by carnal knowledge. Besides, they had observed that even such long frequenting, or cohabiting with them, were not always sufficient to communicate the disease; because they had seen women cohabit with their husbands, and husbands with their wives, in the distemper, while one was sound, and the other infected. They saw families communicate and live with leprous persons, yet never be infected; and thus, though experience, and the information of the sick, prove the contagion, they were of opinion, that there must be a particular disposition in people to receive the poison of the leprosy. 13. As to what regards the distemper's being hereditary, it is assuredly so. They had seen entire families infected; and almost every child of a leprous father or mother fall insensibly into the leprosy; and yet, in several other families, they had seen some children sound, and others tainted; the father had died of the disease, and the children grew old without any infection: so that, though it was certainly hereditary, yet they believed it was of the same nature with those in families troubled with the consumption, gravel, and other hereditary distempers; which are transmitted from father to son, without being so very regular, as to effect every one of the family. 14. They could never find out any certain rule of judging, at what age the disease showed itself first in those who were begotten by infected parents; but they had, as far as they could, observed, with regard to women or girls, that the symptoms began with the menses, and continued slightly till they had lain-in of a child or two; but that then more visible, and indeed more cruel, symptoms appeared. As to men, or infants, there was no rule to know it in them. 15. For the explanation of the causes, symptoms, and what they thought the most likely means of cure, they referred to a particular dissertation. Let it suffice here to observe, that they did not imagine that the air, water, or manner of living, could produce it;

for they had found as many sick in the low marshy places, as in more airy saline places: and if many Negroes were infected in the Grande Terre, where they drink the foul waters of ponds and lakes, they saw an equal number ill in places where they had fresh rivers and running waters; but they might prove proper causes for unlocking, and disposing persons to receive the disease. 16. They believed, and were persuaded, that the origin of this disease among the Negroes came from Guinea, for almost all the Negroes from that country told them, they came from thence with these reddish spots, the first and certain signs of the distemper begun. 17. As to the infected whites and mulattoes of Guadaloupe, they were informed, that the disease was not known among the whites, till about 25 or 30 years before, when, out of charity, they received a miserable object from the island of St. Christopher's, whose name was Clement; who, about the year 1694, fled to this island. It was the family of the Josselins, called the Chaloupers, that protected him; which family, as also that of the Poulins, they found infected by communication with this sick man, as old Poulin declared to them. It is thought that others were infected by communication with the Negro women, especially in the beginning, when the disease is much concealed, and at a time when they did not mistrust one another; which is very probable, since they saw many mulatto children, born of female Negroes, infected and leprous. 18. However this be, this distemper had had its progress; and in this visitation, which they made, they examined 256 suspected persons: that is, 89 whites, 47 free mulattoes, and 120 Negroes; among whom they found 22 whites, 6 mulattoes, and 97 Negroes, infected with the leprosy, amounting to 125. There were 6 whites and 5 Negroes more, whom they could not visit, for reasons set forth in the verbal process. The remaining persons, which were 131, appeared to them very sound: not that they could answer for the consequences, especially with respect to the children, who were the offspring of leprous persons, whether declared such by them, or dead before the visitation, suspected of infection.

This was the opinion, declaration, and result of the visitation made by them, the physicians and surgeon appointed for that purpose. At Basseterre, the day above-mentioned.

PEYSSONEL. LEMOINE. MOULON.

A second visitation was made in October 1748.

VIII. On the late Discoveries of Antiquities at Herculaneum. By Camillo Paderni, Keeper of the Herculaneum Museum, and F. R. S. Dated Naples, Dec. 16, 1756. p. 49.

Two volumes of the ancient papyrus have been unrolled. One treats of rhetoric, the other on music; and both are written by the same author, Philodemus. Il Signor Canonico Mazzocchi, a very learned gentleman of this city, is now

translating them from the Greek. There are two persons constantly employed in unrolling other volumes.

In the month of April were found 2 fine busts of women, the subjects unknown. Also a young stag, of excellent workmanship, on a base. The height of it, from the feet to the top of the head, is 3½ palms. Likewise its companion, but broken in many pieces. In May, a small young hog. In October, a female statue, of middling workmanship. Also a Silenus, a palm and 3 inches high, standing on a square base, raised on 3 rows of steps, supported at the angles by lions' claws. He has a bald head, a long curled beard, a hairy body, and naked feet. The drapery about him is loose and flowing: the fore finger of each hand is extended, and all the rest are closed. From his back arises a branch above the head, where it divides into 2, which, twisting their foliage round it, fall and spread themselves below the shoulders, on each of which a stand is placed to fix a lamp. In the middle, between the extremities of these 2 small branches, is a bird resembling a parroquet. The whole of this figure is in a very good taste. All these things above-mentioned are of bronze. In November was discovered a beautiful marble Terminus, of Greek workmanship, as large as the life. It is dressed in a chlamys; has a young countenance; and the head is covered with a Grecian helmet. Many other things have also been found; as lamps, vases, and such-like, in bronze. And we have often met with paintings.

IX. Of some Trees discovered Under-ground on the Shore at Mount's Bay in Cornwall. By the Rev. Mr. William Borlase, F. R. S. p. 51.

In the said place one day Mr. B. found the roots of a tree, branching off from the trunk in all directions. And, on further search, about 30 feet to the west found the roots of another tree, but without any trunk, though displayed in the same horizontal manner as the first. Fifty feet farther to the north was the body of an oak, 3 feet in diameter, reclining to the east. On digging about it, it was traced 6 feet deep under the surface; but its roots were still deeper than they could pursue them. Within a few feet distance was the body of a willow, 1½ foot in diameter, with the bark on; and one piece of a large hazel branch, with its bark on. What the first 2 trees were, was not easy to distinguish, there being not a sufficiency remaining of the first, and nothing but roots of the second, both pierced with the teredo, or augur-worm. Round these trees was sand, about 10 inches deep, and then the natural earth, in which these trees had formerly flourished. It was a black marsh earth, in which the leaves of the juncus were entirely preserved from putrefaction. These trees were 300 yards within full sea-mark; and, when the tide is in, have at least 12 feet of water above them; and doubtless there are the remains of other trees farther towards the south,

which the sea perpetually covers, and have more than 30 feet water above them. But these are sufficient to confirm the ancient tradition of these parts, that St. Michael's mount, now half a mile inclosed with the sea, when the tide is in, stood formerly in a wood. That the wood consisted of oak, very large, hazel and willow trees, is beyond dispute. That there has been a subsidence of the sea-shores hereabouts, is hinted in a former letter; and the different levels and tendencies, observed in the positions of the trees found, afford some material inferences as to the degree and inequalities of such subsidences in general; as the age in which this subsidence happened (near 1000 years since at least) may convince us that when earthquakes happen, it is well for the country that they are attended with subsidences; for then the ground settles, and the inflammable matter, which occasioned the earthquake, has no longer room to spread, unite, and recruit its forces, so as to create frequent and subsequent earthquakes; whereas, where there are earthquakes without proportionable subsidences, there the caverns and ducts under-ground remaining open and unchoaked, the same cause, which occasioned the first, has room to revive and renew its struggles, and to repeat its desolations or terrors, which is most probably the case of Lisbon.

X. Experiments on applying Dr. Hales's Method of Distilling Salt-water to the Steam Engine. By Keane Fitzgerald, Esq. F. R. S. p. 53.

On reading Dr. Hales's account of purifying salt water, by blowing showers of air through, it occurred to Mr. F. that something of the kind might be applied with advantage to the steam or fire-engine, by increasing the quantity of steam, and consequently diminishing the quantity of fuel otherwise necessary. As the strength of steam raised from boiling water is always in a fluctuating state, and has never been found above $\frac{1}{10}$ stronger or weaker than air; he was in doubt whether steam, produced by this method, would be sufficiently strong for the purpose of the steam-engine.

Mr. F. made an experiment first on a small boiler, about 12 inches diameter, made in the shape of those commonly used in steam-engines, with a funnel at the top, of about 1 inch diameter, for the steam to pass through, the aperture of which was covered with a thin plate, fixed at one end with a hinge, and a small leaden weight to slide on the other, in the nature of a steel-yard, to mark the strength or quantity of the steam. A tin pipe made for this purpose, with several small holes towards the end, passed from a small pair of bellows, through the upper part of the boiler, to within about an inch of the bottom. The boiler was half filled with water, which covered the holes in the pipe about 6 inches. From the best observation he was capable of making with this machine, by blowing air through the boiling water, it produced about $\frac{1}{6}$ more steam than was

produced by the same fire without blowing air through. He then applied a machine of this kind to the engine at the York-buildings water-works, the boiler of which is 15 feet diameter. It has a double concave, with a kind of doorway or passage from one to the other, to let the flame pass, as it were, through and round the water; by which means there is no-where above 9 inches of water to be heated through, though the boiler is so large; and which, by 3 years experience has been found to require $\frac{1}{4}$ less fuel than any other fire engine of equal size.

Mr. F. fixed a pipe, of an inch and a half diameter, to a pair of double bellows 3 feet diameter; which pipe reached about 1 foot under the surface of the water in the boiler, to the end of which are fixed horizontally 2 branches, each about 8 feet long, tapering from 1 inch diameter to about $\frac{1}{4}$ of an inch. These branches are bent in a circular manner, as in the plan, to answer the form of the concave, and are perforated with small holes about 4 inches distant at the thickest part, and decreasing gradually in distance, to within $\frac{1}{4}$ of an inch towards the small end. The reason of these branches being made taper, and the distance between the holes decreasing to the small end, was in order to give the greater power to the air forced by the bellows to discharge the water lodged in such a length of pipe; and he observed by this method, that the water was gradually forced through the holes to the end of each branch, and seemed to throw an equal quantity of air through the water. The length of the pipe, to which the branches are fixed horizontally, is about 18 feet to the nosle of the bellows: and yet the steam, that passed through the pipe into the bellows, was so hot before the water boiled, as to force through the leather: but this he easily remedied, by fixing a brass cock of $1\frac{1}{2}$ inch diameter to the pipe, which hindered the steam from ascending, till the engine was ready to work: and being opened, the air continually keeps it cold till the engine has done working; then the cock must be shut again.

The bellows is worked by means of a small lever, and pulleys applied to the great lever of the fire-engine, which keeps a continual blast while the engine works, the strength of which is increased or diminished, by adding or taking off the weights on the bellows.

The effect produced was, first, a very visible alteration for the better in the working of the engine. When the fire was stirred, as it must be every time fuel is added, the steam generally became too fierce, which occasioned great irregularity, and sometimes, if not watched, great damage to the engine; and when the fire abated, the stroke became immediately much shorter, or stopped entirely, if fuel was not soon added, whereas, by blowing air thus through the water, it keeps, with any moderate care, an equal stroke to its full length, from the beginning to the end; and by that means discharges a considerably greater quantity of water.

As to the quantity of fuel, that may be saved by this method, it is not easy to determine from any experiment on this engine, the boiler and fire-place of which is made very different from all others, and the quantity of fuel thus greatly lessened. The fire-place, which may be said to be within the boiler, and is but barely large enough to contain a quantity of the roundest and strongest burning coals sufficient to work the engine, cannot in this be made less; and consequently will not admit such a saving from this model, as from one properly constructed for the purpose.

XI. Extract of a Letter, by Mr. Abraham Trembley, F. R. S. on Earthquakes, Polypes, Fossils, &c. Translated from the French. p. 58.

Mr. T. mentions an earthquake felt in 1756, between the Rhine and the Meuse. He was also informed by Professor Donati of Turin, that a slight shock had been perceived there on the 13th of August 1756, at a quarter after 9 in the morning. It was likewise felt in other parts of Piedmont.

He further states, that Mons. Donati took last summer, according to his custom, a journey, to prosecute his researches into natural history. He was accompanied by Dr. Ascanius, F.R.S.; who was still in doubt about coral's being a composition of animals. Mons. Donati carried him to the sea of Provence. He ordered coral to be fished up in his presence. He placed it in a large vessel full of water; and carried this vessel on shore; where he soon convinced Dr. Ascanius, by his own eyes, that coral is a mass of animals of the polype kind.

Mons. Donati also wrote that he had thoroughly satisfied himself, by his last observations, that the polypes are fixed to their cells; of which he had before doubted. What he says afterwards of coral appears to express with more truth and precision what we ought to think of this kind of animals, than any of the descriptions, which have been given since the new discoveries have changed our sentiments on that subject. Polype-beds, and the cells which they contain, are commonly spoken of as being the work of polypes. They are compared to the honeycomb made by bees. It is more exact to say that coral, and other coralline bodies, have the same relation to the polypes united to them, that there is between the shell of a snail and the snail itself, or between the bones of an animal, and the animal itself. Mons. Donati's words are as follow: "I am now of opinion, that coral is nothing else than a real animal, which has a very great number of heads. I consider the polypes of coral only as the heads of the animal. This animal has a bone ramified in the shape of a shrub. This bone is covered with a kind of flesh, which is the flesh of the animal. My observations have discovered to me several analogies between the animals of kinds approaching to this. There are, for instance, keratophyta, which do not differ from coral,

except in the bone, or part that forms the prop of the animal. In the coral it is testaceous, and in the keratophyta it is horny."

The observations which Mr. T. made on some kinds of polype-beds, led him to think that what are called polypes, in those bodies which are observed to come out of and return into the cells, are more than the heads of the animal. He had seen some which had a bag, into which passed their food, which he saw them swallow; and another bag, into which passed the grossest part of that food, after it was digested. This is the case, for instance, of the plumed polypes, which he described at the end of the 3d memoir, in the work published by him on one kind of fresh-water polypes.

Mons. Donati had observed several very curious facts in the journey which he made into the mountains. He had, in particular, traced out an immense bed of marine bodies. This bed crosses the highest mountains which separate Provence from Piedmont, and loses itself in the plains of Piedmont. He had likewise observed a mass of rock, which forms the extremity of a pretty high mountain, the foot of which is washed by the sea. This rock is, at a considerable height, entirely pierced by pholades, that species of marine shell-fish so well known, which digs cells in the stones. It hence appears, that this rock was some time covered by the sea. According to Mons. Donati, the sea has insensibly retired from the parts which were washed by it; and he thinks that there must have been a very considerable space of time between that and the time when this mountain, pierced by pholades, was covered by the waters of the sea. He deduces his opinion from the following fact. There is in this rock, pretty near the surface of the sea, a natural cavern filled with water. In this earth have been found ancient Roman sarcophagi and lamps. Hence it follows that even in the time of the Romans this part of the rock, in which this cavern is situated, was not under water. As there is but a small distance between the cavern and the surface of the water, it follows that the water has sunk but very little since the time of the Romans. If it has sunk in the same proportion since the time when it covered the top of the rock, there is no doubt but that the time when it was entirely covered by the sea, must have been very distant. If the same manner of reasoning be used with respect to the bed of marine bodies mentioned above, which crosses the mountains that separate Provence from Piedmont, we shall be obliged to presume, that the time when those mountains were under the waters of the sea, was at a very great distance from the present. Mons. Donati concludes from these facts, and the consequences deduced from them, that the Mediterranean sea is a very ancient, and not a modern one, as Mons. de Buffon imagines.

Those who explain all the phenomena of marine bodies found out of the sea, by a universal deluge, do not admit the consequences drawn by Mons. Donati from those marine bodies now under consideration. It is plain that most of the

naturalists, who have observed a great number of these marine bodies, are not of opinion that all those phenomena can be explained by a universal deluge. On these subjects, before we undertake to judge, it is proper to be well informed of the nature of marine fossil bodies, which are found in divers parts, and of their situation and arrangement. It is necessary also to be acquainted with the state of those which are found actually under the sea, and the revolutions to which they are subject, while they are covered by it. It is still further requisite to attend to the revolutions which have been, and are constantly observed, with respect to the sea-shores, which change their situation in several parts, some advancing on the land, and others retiring. If all these different facts be compared together, it will not be doubted, but there are actually under the earth marine bodies, which are found there only in consequence of these slow revolutions, and not of a universal deluge. Perhaps this notion might be extended to the greatest part of the marine fossil bodies which are known to us.

XII. A Botanical and Medical History of the Solanum Lethale, Bella-donna, or Deadly Nightshade, by Mr. Richard Pulteney. Communicated by Mr. Wm. Watson, F.R.S. p. 62.

As accurate descriptions of the deadly nightshade, or atropa belladonna, Linn. together with an account of its poisonous quality and uses in medicine, are to be found in various modern systems of botany, and treatises on the Materia Medica (the plant being a native of this country), it is deemed unnecessary to reprint this paper.

XIII. On some of the Antiquities discovered at Herculaneum, &c. By John Nixon, A.M., F.R.S. p. 88.

Mr. N. first treats of the several tali lusorii, that were played with a set of dice. Mr. N. refers to several passages in the writings of the ancients on the use of these tali. He says the tali are supposed to have been known to the Greeks by the name of Ἀστράγαλοι as early as the Trojan war. We can with certainty determine the number of the tali used in this game to have been four; and likewise, that among the various chances resulting from them, the most fortunate one was that wherein each of the sides exhibited a different aspect.

He concludes with noting, that in order to prevent any fraud or slight of hand in managing the tali, it was usual to put them into a box, and after shaking them together, to throw them out upon a table. However, this caution does not seem to have been so universally observed, but that sometimes, viz. when the party consisted of ladies, it was (he presumes, for a reason greatly to their honour) superseded. Thus, in one of the first paintings found at Herculaneum, and now in the royal apartments at Portici, a young female figure is exhibited,

playing at this game, with one or more of the tali lying upon the back part of her hand, while the rest appear as having fallen off from it towards the floor. The 2d article, is a rule with 4 joints, each of which contained about 5 inches 9 tenths of our measure. He thinks there was another in 2 parts, which answered to the same proportion. 3. A weight, inscribed on one side EME, and on the other HABEBIS. 4. A small bolla d'oro, which (after that in the late Dr. Middleton's collection, and another preserved at Rome) is the 3d known to be extant in Europe. 5. A little figure like a Faunus, excepting that about the head it had something of the character of the Minotaur, viz. large curls on the forehead, and several muscular protuberances, or tori, under the throat. 6. A figure in relievo of a man sitting with a bowl in his hand, which has been thought a Socrates. 7. An antique painting of a muse, with a capsula near her, containing some volumes, from which hang labels showing the titles of the works. The same representation appears in another painting kept in a different part of the palace. 8. Some pieces of fine paper, coloured red on one side, and black on the other, found upon the breast of a skeleton; thought, by Signor Paderni, not to be of the charta papyracea, but of that of silk, cotton, or linen. 9. A flat piece of white glass, taken off from near the extremity of the sheet, as appears from the curvature and protuberant thickness of one of its sides above the other parts.

Mr. N. then mentions a few of the chief paintings, the same as have been noticed in former accounts printed in these Transactions.

XIV. On the Effects of a Storm of Thunder and Lightning, in the Parishes of Looe and Lanreath, in Cornwall, on June 27, 1756; in two Letters. From the Rev. Mr. Dyer of Looe, and the Rev. Mr. Milles of Duloe, in Cornwall p. 104.

On this occasion, the lightning is described as more like darting flames of fire, than flashes of enkindled vapour. At Bucklawren, a village on the top of a hill, about 2 miles from Looe, a farm house was shattered in a most surprizing and destructive manner. Some elm trees too, near the house, were struck and furrowed from top to bottom, and the ground at their roots torn up as if done with the plough.

Another was struck and damaged in a similar manner, in the parish of Lanreath, about 6 miles off, and 2 men in it much burnt and hurt.

XV. Of the Peat-pit near Newbury in Berkshire; in an Extract of a Letter from John Collet, M. D. p. 109.

This peat moss, which is about 9 miles in length, and near half a mile broad,

has nothing singular in it, except it be, that a great many horns, heads, and bones of several kinds of deer, the horns of the antelope, the heads and tusks of boars, the heads of beavers, &c. are found in it: and it is said that some human bones have been found also, all near the bottom.

XVI. *On the Alterations making in the Pantheon at Rome: In an Extract of a Letter from Rome to Thomas Hollis, Esq.* p. 115.

A project was lately laid before the government by Paolo Posi, an architect, for modernizing the inside of the pantheon, which unfortunately was approved. In consequence of which, the dome has been already cleaned, and rough cast; and the remainder of the lead taken away, which served as a lining to the silver work, that originally covered it. The vestiges of the cornices, and other ornaments of the silver work, were still discernible in the lead, which was fastened by very large iron nails. All this was effected by a moveable scaffold, fixed to the bronze cornice of the open circle above, by which the temple is illuminated, and descended to the cornice of the attic order, being as curious in the contrivance, as detestable for the purposes intended by it. It is true, we could not before see the dome in its pristine glory; but we had the satisfaction of viewing the traces and remains of what it had been. Nor could the adepts in architecture sufficiently admire the skill and sagacity of the builder, who, composing it of a number of small arches, which together formed a kind of net-work, and filling up the intervals between with pumice-stones and mortar, gave it that strength and lightness, by which it has probably stood so many ages. The evil would be comparatively small, had the project extended no further than what has been related; but they are now busy in removing the attic order, to make room for a new invention, suitable to the trifling taste which at this day prevails. And not content with that, they think of taking away the ancient pavement; and, what is still worse, the open circle at the top, to place a lantern instead of it, as is usual in modern cupolas.

You had the good fortune, Sir, to view this remarkable temple, in that state in which it was left by the ancient barbarians: but those who see it hereafter will find it in a much more deplorable condition; stripped of its precious marbles and ornaments; and so disguised by modern alterations, that the noble form given it by Agrippa will be no longer distinguishable.

XVII. *Of a New Medicinal Well, lately discovered near Moffat, in Annandale, in Dumfrieshire. By Mr. John Walker, Kirkudbright, Scotland.* p. 117.

This mineral spring was found out by one Mr. Williamson, a few years ago, when he was overseeing a mine, at that time carrying on in its neighbourhood. It is situated about 4 miles distant from Moffat, in the bottom of a deep scar, on

the west side of a large mountain called Hartfell, from which it has acquired the name of Hartfell-spa.* This scar is a part of the mountain, through which a small stream of water has worn its way to a considerable depth; by which it has laid open, and exposed to view, the strata of the earth on each side: and in the bottom of this scar, and near the brink of this small brook, the mineral water springs up, in 2 springs, the one a little lower than the other.

According to what may be inferred from the following experiments, it may be premised, that this water appears to contain in it a large proportion of iron, but in 2 different forms; and an aluminous salt united with a terrestrial principle. The water equals the clearest spring-water in transparency; and is as free from colour or odour: yet its taste is very strong, and may be discerned to be compounded of a sweet, subacid, and astringent taste. Its sweetness and acidity appear sensibly to arise from alum; and its high styptic and astringent taste as evidently proceeds from that mineral salt, joined with some earthy or ferruginеous parts.

Experiment 1. Some pieces of galls being added to equal quantities of the water of the 2 springs, a very deep and bright blue colour was immediately produced in the water of the upper spring, which in a little time turned to a perfect black. The water of the lower spring, though it was turned of the same colour, yet was not of so deep a shade, but was somewhat lighter than the former. The tincture of galls caused the same appearances. 2. A tincture of balaustine-flowers produced the above blue colours in both waters. 3. A quantity of the water being thoroughly tinged with galls, was allowed to stand 24 hours: being then filtrated through brown paper, the water, though almost quite colourless, would not again receive any tincture with galls. 4. After elixation, the water became of a turbid yellow colour with ochre, and afforded very little tincture with galls. 5. A solution of sal martis, chemically prepared, being mixed with galls, immediately turned of a bright dark blue colour, exactly similar to that produced in the water. 6. A solution of common and rock alum was noways changed in its colour with galls. 7. A solution of sal martis and alum being mixed in equal quantities, the mixture was turned of a bright blue colour with galls; yet not of so deep a hue, but of a more diluted colour than the solution of sal martis, without alum.

* Of this mineral water (the Hartfell Spa) an accurate analysis was published by Dr. Garnett, in the year 1800, from which it appears to belong to the class of vitriolated chalybeate waters. From a wine gallon he obtained 84 grs. of vitriol of iron, (sulphate of iron,) 12 grs. of alum, (sulphate of alumine,) and 15 grs. of calx of iron, (oxyd of iron.) During the evaporation, a small quantity of gas was given out. This mineral spring (the Hartfell) which lies near Moffat, must not be confounded with the Moffat mineral water, which is a saline sulphureous water, without any impregnation of iron.

From these experiments Mr. W. observes, that the colour which these waters afford with galls and pomegranate-flowers, is very uncommon. The more iron any mineral water contains, it will afford the deeper colour with such astringents: but though he had tried this experiment on many of the ferruginous waters in Scotland, and also on the water of some of the foreign spas, he never observed one that afforded so deep a colour as this. Some of the weakest of them gives only a red or faint purple tincture, and the strongest only a deep purple: but he never saw or heard of any chalybeate water, but this, either in Scotland or elsewhere, that afforded an intense black and inky colour with galls. From which, he thought he might venture to conclude, that the water of this spa contains a far larger proportion of iron than most, or perhaps than any other chalybeate water hitherto discovered: and for this reason, he supposed it would likewise be so much the more preferable to most others in medicinal virtues; which had indeed appeared by many surprising cures it had performed, and which, he was persuaded, would more fully appear, when its medicinal effects should be better known. There must he thinks, be a great quantity of iron in this water, when it yields as deep a colour with galls as a strong solution of sal martis. He was indeed at first apprehensive, that this perhaps might not be owing so much to a large and uncommon proportion of chalybeate parts, as to the commixture of alum, which he judged to be in the water. But we see the contrary appears by these trials: for alum of itself affords no tincture with astringents, and, instead of rendering a solution of sal martis with galls of a more intense colour, it rather makes it lighter and more diluted.

As there is an ochrous earth separated from all steel waters, when exposed to the air, which subsides to the bottom, and a metalline scum or cremor, which swim on their surface; he next considers the appearances which they make in this water.

Exp. 8. A solution of saccharum saturni being dropt into common spring-water, left the upper parts of the water clear and colourless, but formed a lactescency towards the bottom. The same solution being added to the mineral water, soon turned it of a turbid yellow colour, which afterwards subsided, and formed a deep yellow cloud in the bottom of the glass; and below this yellow sediment there adhered to the bottom of the glass a whitish substance, which he took to be the metalline parts of the saccharum saturni separated from the purer parts of the salt, which were still suspended in the water, and which made it of a muddy whitish colour. 9. Forty drops of oleum tartari per deliquium being added to 1 oz. of the water, made it of a uniform light yellow colour; but in an hour afterwards there were many small yellow terrene nubeculæ formed in it. These the next day were more conspicuous, being thoroughly separated from the water, and precipitated to the bottom, leaving the water quite clear, as it was

before the mixture. A small quantity of this limpid water being taken, it would afford no tincture with galls. It was then all poured off, except so much in the bottom of the glass as contained the above-mentioned clouds: to this some galls were added, which in half an hour turned these clouds from a light yellow to a deep red colour, but did not change the colour of the water, in which they swam. 10. Immediately after the affusion of ol. tart. p. d. to the water, galls were added to the mixture, which tinged it of a deep and bright red colour. After standing for some time, red clouds precipitated to the bottom, and the water continued of a dusky opaque red colour. 11. There is a small brook, formerly mentioned, which runs near by these springs; into which the water, that flows from them, is discharged. He observed the stones and channel of this brook all tinged with ochre of a deep yellow colour, so far up as the water of these springs flowed into it; but the channel, which the mineral water ran over before it was mixed with the water of the brook, was very little or nothing discoloured with ochre. As he conjectured what this was owing to, he afterwards took 2 equal quantities of the mineral water, into one of which he put an equal quantity of common water. In 2 hours the mixture became less transparent, and appeared yellowish, while the simple mineral water retained its clearness. Next day there was much ochre separated from the mixture, which subsided to the bottom of the glass: but the unmixed mineral water remained still clear and colourless, as at first.

All chalybeate waters separate their ochrous parts, when exposed some time to the air; but this separation is made sooner by the commixture of several kinds of salts. Thus we see the ochre in this water is immediately separated and precipitated by the solution of saccharum saturni. The oil of tartar causes a precipitation of these ferrugineous parts in the same manner. Which parts must be the sole cause that the water receives a tincture from galls; since, after they are precipitated, it loses that quality, which they notwithstanding retain even after they are separated from the water. This precipitation of the ochrous parts of the water were the only visible effects that Mr. W. could perceive follow from the affusion of the ol. tart. p. d. He remembered indeed, when he was at Moffat, to have seen the MS. of Dr. Horsburgh's experiments on this mineral water, which appeared to be very accurate; and which he understood were afterwards printed, in a vol. published by the Phil. Society at Edinburgh. Among these he observed one, which he thought so very remarkable, that he particularly adverted to it. It was the effects of the affusion of ol. tart. p. d. to the water, producing in it clouds, or a coagulation of a green or grass-green colour. He thought these were the words; and he owned he was something surprised at them. A solution of vitriolum martis, mixed with this alkaline oil, does indeed produce a green coagulum: but he could scarcely think that this,

or any other chalybeate water, contained so large a proportion of that vitriol as to be sufficient to produce these effects, when he considered that so many writers, which he had seen upon this subject, had all failed in their attempts of extracting conspicuous martial vitriol from such mineral waters. He had tried this experiment on 4 or 5 chalybeate springs in Scotland, and likewise on the Spa and Pyrmont waters, which had been well preserved; but there never resulted any such effects from the mixture of these with oil of tartar as are related in the above experiment. All the alteration it produced in these waters was the precipitation of an ochrous earth, but without the least appearance of any green colour. As he looked upon this as a leading experiment in the history of vitriolic waters; as he had often tried it, and as often seen the green coagulum produced with the solution of the factitious vitriol, and never could observe it produced in any of the above water; he began to suspect that these waters were either not possessed of a vitriolic salt at all, or else that it was in some respects very different from the factitious vitriol. For these reasons Dr. Horsburgh's experiment appeared very extraordinary; though at the same time he was greatly pleased that he should have the opportunity of repeating it, and of observing those phenomena in this ferrugineous water, which he had sought for in vain in several others. But when he came to make the trial, he was yet more surprised when he found it fail, and that the ol. tart. p. d. produced no green colour or coagulum in this mineral water, nor caused any other alteration in it than the separation of a large quantity of ochrous earth of a yellow colour, exactly the same with what he had observed in the other steel waters. This failure made him immediately conclude that he had committed some error in the experiment: and though he was pretty sure that the mineral water, which he had used in it, was quite fresh, yet he could not be so positive as to the oil of tartar which he suspected to have been long kept. Yet that this could have been the cause of his being so unsuccessful he could scarcely believe, though indeed he could assign no other.

When galls are added to the water at the same time with oil of tartar, instead of its deep blue colour it affords only a red tincture.

It appears from the 11th experiment, that an addition of common water causes the mineral water to precipitate its ochre; and the reason of this is obvious: for if these ochrous parts be altogether terrene, as they appear to be, and exist in the water unconnected with any other principle, then it must happen that as these parts are uniformly diffused through the water, in which they are suspended as in a menstruum; by the addition of common water, this menstruum being diluted, the cohesion of these terrene parts must be thereby weakened, and their contact destroyed, so that their menstrual equilibrium being thus taken

off, they can be no longer supported in the fluid, but must be precipitated by the force of their own gravity.

Exper. 12. When the water was exposed for some days to the air, there was a cremor separated from it of a shining chalybeate colour. This, like other kinds of cremor, takes a considerable time to complete its entire separation from the fluid, out of which it is expelled: for when it was despumated, a new cremor always succeeded, till the whole quantity, which the water contained, was exhausted. 13. When this cremor first appeared on the water, it was of a faint bluish colour: but as it increased, it changed into a deeper and more bright shining blue: and after longer standing it became blotched with various colours, as red, orange, yellow, green, blue, purple, and violet. 14. A quantity of the water being put in a gentle heat, this cremor was quickly separated from it, and appeared on the surface of the water. A like quantity of the water, with its cremor already on its surface, was put over a gentle heat, which by degrees broke the cremor into very small parts; but whether they evaporated or precipitated in the water, he could not be certain. But, by other trials, this cremor was found to have a great degree of fixity, bearing a considerable heat without avolation; yet not without the appearance of some of its parts flying off, though most of them were fixed; because what remained lost its fine colours, and was changed into a shining chalybeate colour. 15. The water of the lower spring afforded a much less quantity of the cremor than the water of the upper spring. It took also a longer time to separate, was of a bluish colour, and had not the vivid colours which the water of the upper spring showed. 16. When ol. tart. p. d. and spirit of sal ammoniac were added to the water, it did not separate its cremor.

This cremor, which is separated from the water, is the same as appears on the surface of a solution of vitriolum martis, when exposed for some time to the air: and an infusion of iron in common water also emits a cremor of the same kind. As he was once carefully observing a large glass full of a chalybeate-water, which contained much of this cremor; soon after it was exposed to the air, he observed a tenuious bluish vapour rising in the parts of the water next the surface, which very much diminished its transparency; and by degrees this vapour was emitted by the lowest parts of the water: but as the cremor increased on its surface, the water became gradually deprived of the bluish tincture, which it received from this halituous body; which was apparently nothing else but the parts of the cremor separating from the water, and ascending upwards. Whence we may conclude, that this cremor consists of the very finest parts of iron attenuated to the highest degree.

The next trials were in quest of alum.

Exper. 17. A quantity of the water being kept for some time in a boiling

heat, and after it was cool being filtered quite clear from its ochrous matter, it still retained a subacid and aluminous taste in a very strong degree. 18. To an oz. of common spring-water were added 2 drops of fresh sweet milk. This mixture being shaken, the milk mixed intimately with the water, without any kind of coagulation. 19. The same experiment being made with the mineral water, the milk, on its affusion, was so curdled, or separated into clouds, that the greatest shaking could not mix or incorporate it with the water. 20. This experiment being also made with a weak solution of alum in spring-water, its effects on the milk were not in the least different from those of the mineral water. 21. And the same trial being again repeated with the water, when boiled and filtred from its ochrous parts, the milk was in the same manner coagulated as before elixation. 22. One part of sweet milk being added to 4 parts of the mineral water, the milk subsided, and formed a cloud in the bottom of the glass, leaving the upper parts of the water clear. This mixture being well shaken, the milk mixed so well with the water, that it appeared to be but a very little curdled. 23. When a larger quantity of milk was added to a smaller quantity of water, and even when equal parts of the milk and mineral water were mixed and shaken together, no curdling or coagulation was observed. 24. An equal quantity of the water and milk being boiled together, the greatest part of the milk was coagulated into a thick white curd; and the remainder, with the mineral water, turned of a pure white milky colour, which drank like whey, and was very agreeable. 25. Eight drops of sweet milk being added to 4 oz. of the water, and the mixture boiled, part of the milk was curdled, and swam on the top of the water. The ochrous parts of the water were likewise separated, and falling to the bottom, their colour did not appear of a clear yellow, as usual, but was something milky.

All these experiments strongly indicate the existence of alum in this water. It retains its aluminous taste, and coagulates milk, after the chalybeate parts are almost all expelled by elixation. The coagulation of the milk demonstrates an acidity in the water, and the other appearances show that acidity to be owing to an aluminous salt.

The following experiments not only (the author thinks) ascertain the existence of alum in the water with greater certainty, but also that there is a particular kind of earth conjoined with this salt.

Exper. 26. An English quart of the water being kept boiling for a quarter of an hour, it turned thick, muddy, and yellow, by the separation of its ochrous parts; and, being set to cool in a clean bowl, the next day all the ochre was subsided to the bottom, from which the water was carefully filtered: by which it became almost as clear and limpid as before the elixation, retaining a sharp aluminous taste, but was deprived of the strong ferrugineous taste which it had at

first. This water was again boiled; by which means it was again turned a little yellow, by the separation of some more ochre. It was therefore again filtred, and rendered clear, and its aluminous taste was stronger than before. After this filtration, the water was evaporated in a sand-heat to about a 16th part of the original quantity, and then it tasted like a strong solution of alum joined with a small degree of a chalybeate taste. And this being totally evaporated in a glass, there adhered on its sides a pure white salt; and a larger quantity of the same salt remained in the bottom of the glass, which was not so white, but more impure than the former, and of a brown colour. 27. This salt, thus procured from the water, being mixed with distilled vinegar and spirit of vitriol, not the least effervescence was produced. 28. Some of the brown-coloured salt being put on a red-hot iron, it neither sparkled nor decrepitated; but was turned into a blackish cineritious substance, which in a short time became a white calx. And though some of the salt was put upon the iron finely powdered, yet it concreted, and ran together in a cinder, whose cohesion was afterwards destroyed when calcined by a further degree of heat. 29. As he was accidentally deprived of the opportunity of obtaining the crystals of this salt, which would have been the best means of knowing to what species it was to be referred; he dissolved the whole mass in a small quantity of spring-water, and by filtrating this solution, he obtained a large proportion of fine earth of a brown colour. 30. This solution of the salt afforded a deep blue tincture with galls. 31. The same solution being mixed with syrup of violets, became of a reddish colour. 32. Saccharum saturni being added to the solution precipitated a thick lactescent cloud. 33. Ol. tart. p. d. being also added to this solution, it caused no visible effervescence, yet raised some bubbles of air, and caused a coagulation of many small brown terrene nubeculæ in the water; which, after standing some time, subsided to the bottom, and left the water clear.

These experiments plainly evince (in his opinion), that this water contains an aluminous salt, conjoined with a fine terrene substance, which is probably a part of the matrix, whence the salt has been formed. This salt gave no signs of any alkaline principle; but on the contrary, of an acidity, as its solution reddens with syrup of violets. With this salt there are also intimately conjoined some very subtile chalybeate parts, which are not separable from it by elixation or evaporation.

Alum is distinguishable from all other mineral salts, by liquifying and bubbling on a red-hot iron, and turning into a white calx. But this could not be well expected from this aluminous salt, which he had extracted from the water, because it was extremely foul, by being combined with so large a proportion of earth; which earthy parts were the occasion of turning the salt of a blackish colour upon the iron. However, we see it turns white by a further degree of

heat. But if the salt had been dissolved, filtrated, and crystallized, till it had been purified and freed from this terrene matter, it would then certainly have had the same appearance on the red-hot iron, as a pure aluminous salt. Again, as it is peculiar to an aluminous salt to liquify in some degree with fire, so we see that this was evidently the case of this salt. Its eliquation indeed could not be so remarkable as in pure alum, because of its being mixed with so much earth; but that it did liquify in some degree is plain, because the whole mass of salt and earth, even when reduced to a powder, ran all together like a cinder. The experiment on the solution of this salt with ol. tart. p. d. is also a further proof of what we have already asserted: for though there was no visible effervescence, yet the bubbles of air show that there was an intestine conflict of the oil with the acid principle in the solution: which being absorbed by the alkali, the earth was precipitated, to which it formerly adhered.

The next 2 experiments were made to discover whether an acid or alkaline principle prevailed in the water.

Exper. 34. Forty drops of the syrup of violets being added to 1 oz. of the water, the mixture became of a bright sea-green colour. 35. A quantity of the water being kept boiling for 5 minutes, and afterwards allowed to stand till it became clear, was carefully filtrated from its ochrous sediment: after which, on its mixture with syrup of violets, it turned of a faint reddish colour.

From these experiments he inferred that this mineral water contained both an alkaline and an acid principle; the former consisting of the ochrous and ferruginous parts, which were separated from the water by elixation; and the latter of the aluminous salt, which remained in the water after elixation.

The following trials were made, to know what effects are produced in the water by being exposed to the air; and in what respects the waters of the two springs differed from each other.

Exper. 36. An English quart of the water of each of the springs being fully exposed to the air in 2 China bowls, the next day that of the under spring was neither altered in its taste, colour, or transparency, nor in any other shape whatever; but that of the upper spring appeared of a yellowish colour, though it was clear and transparent as the other.

On the 2d day the taste of the waters scarcely appeared to be any way diminished. No sensible change could be observed in the lower water; but the upper water was become more yellow than it was the day before, yet without any loss of its transparency. They both tinged of a deep blue colour with galls; which tinctures appeared equally deep and strong, as they did before the waters were exposed to the air. The 3d day the lower water appeared clear and colourless as before, only its surface was covered with a few small spots of cremor. The upper

water appeared more yellowish than formerly, and its surface was almost wholly covered over with the cremor. They both afforded a tincture with galls, which was not sensibly different from what they gave before their exposure. On the 14th day the water of the under well had precipitated a yellow ochrous sediment, but the other water a more considerable quantity. A large quantity of cremor continued also to swim on the surface of the upper water, but there was very little separated from the water of the under well. Both waters being now tried with galls, instead of the deep blue colour, which they formerly exhibited, they now became only of a deep purple colour. On the 20th day the visible appearance of both waters was the same as when last observed. On the 38th day they both afforded as deep a purple colour with galls, as they did 3 weeks before; and during that time also they had not precipitated any more of their ochrous parts, nor suffered any other sensible alteration.

The water of the upper well being filtrated from all the ferrugineous matter, which it had separated during these 38 days, was rendered almost as limpid and clear as when newly taken from the well: but being boiled for some time, it became of a turbid yellow colour; and being allowed to stand, it again precipitated abundance of an ochrous sediment; and being filtrated, and mixed with galls, it received a faint purple colour of a bluish hue.

Exper. 37. A bottle of the water of each of these springs being carefully sealed, carried to Moffat, and kept for 2 months, suffered not the least alteration during that time, but was as fresh as when immediately taken from the fountain. And he was informed, that after it was carried to Edinburgh, and to places at a greater distance, it kept a much longer time, without being any way spoiled.

From these observations he thinks it will appear, that this mineral water continues longer entire, and particularly that it retains the quality of tinging with galls longer than most others of the chalybeate kind: at least of a great number which he had seen described, he did not remember one that retained it nearly so long when exposed to the open air. Many of them lose this quality entirely in a few hours; and it is greatly impaired in the same time, even in those which retain it longest. But this water remains exposed to the open air for days, with scarcely any alteration. This might perhaps be owing either to the larger proportion of ferrugineous parts, with which it is impregnated; to their being attenuated to a greater degree; or to their more perfect commixture with the water, by means of the aluminous salt. The longer time that any mineral water remains entire, without any separation of its mineral parts; or the longer it retains the same form, which it has when newly taken from the spring; the more perfect is the commixture of these parts with their fluid vehicle: and he believed on that account would be more effectual for medicinal uses: for which reason

he supposed these waters might prove a more beneficial medicine than any others of the ferrugineous kind, whose mineral contents are not so intimately commixed with the aqueous fluid.

But to sum up the evidence, which these experiments, taken all together, afforded concerning the mineral ingredients of this spa: he thought they determined with some degree of certainty, that it contains 2 different principles of iron, both of which are fixed. The one, which is the ochrous earth, is a true minera ferri, and though it be a crude mineral, exists in the water in a very fine and subtile form; the other, which is the cremor or pellicle, whose parts are also extremely attenuated in the water, appeared to be iron, not in its mineral, but in its metalline form, and when thrown up to the surface of the water, showed itself like an extreme thin lamina of that metal. There seemed also to be some small proportion of sulphur joined with the metalline cremor. The other mineral ingredient, which entered into the composition of this spa, was a considerable proportion of an aluminous salt, conjoined with a small quantity of a light brown-coloured earth (probably a part of the matrix whence the salt is formed), and still more intimately connected with some of the chalybeate parts of the water, which were not separable from it either by elixation or evaporation. Whether these were saline or terrestrial, he could not determine.

Then follow some reflections on the origin of steel waters, and particularly of this spa (the Hartfell spa); the mineral impregnation of which he attributes to the adjacent strata of cliffery rock.* He concludes with this query; viz. Whether or not from such a knowledge of the origin of mineral waters, we may not acquire (prepare) artificial ones of as great or perhaps greater medicinal uses than those which are naturally produced?

XVIII. On the State of the Thermometer at the Hague on the 9th of January 1757. Extracted from a Letter of Mr. Abraham Trembley, F.R.S. p. 148.

I carefully observed the thermometer during the cold days, which we have had this winter. I made use of the same thermometer with which I made my observations in 1740, and for that purpose fixed it in the same place where it was that year, viz. in a window directly exposed to the north, and open to a large square. In 1740 Fahrenheit's thermometer was at 2 degrees below 0. This year, on the 9th day of January in the morning, it was at 3 degrees above 0; that is, only 5 degrees higher than in 1740.

XIX. Experimental Examination of Platina. By W. Lewis, M.B., F.R.S. p. 148.

Paper V. The account of this extraordinary mineral, formerly read to this So-

* Aluminous schistus and argillaceous stone, intermixed with martial pyrites.

ciety, being since published in the Phil. Trans. renders any recapitulation of the discoveries hitherto made unnecessary.

The near and remarkable relation between platina and gold, not only in point of gravity, but in many less obvious properties, hitherto supposed to belong to gold alone; and their as manifest disagreement in others, particularly colour, ductility, and fusibility; induced Mr. L. to examine what effects they might have in combination with each other in different proportions; and whether there is reason to credit the report of great frauds having been committed by mixing them together; how far such abuses are practicable; and what is of more importance, the means by which they are discoverable.

Experiments on the Mixture of Platina and Gold.

Exper. 1. Twelve carats* of fine gold, and the same quantity of the purer grains of platina, were urged in a blast-furnace, for near an hour, with a fire so strong, that a slip of Windsor brick, with which the crucible was covered, though defended by a thin coating of pure white clay, had begun to melt. On breaking the vessel, the metal was found in one smooth lump or bead; which, after being nealed by the flame of a lamp, and boiled in alum-water, appeared, both in the mass, and on the touchstone, of a pale bell-metal colour, without any resemblance to gold. It bore several strokes, and stretched considerably under the hammer, before it began to crack about the edges. On viewing the fracture with a magnifying glass, the gold and platina appeared unequally mixed; and several small particles of the latter were seen distinct: nor was the mixture entirely uniform after it had again and again been returned to the fire, and suffered many hours of strong fusion. 2. Eighteen carats of gold and 6 of platina (= 3 : 1) were melted together as the foregoing, in an intense fire continued about an hour. The bead, nealed and boiled, was less pale-coloured than the former, but had nothing of the colour of gold. It forged tolerably well, like coarse gold. To the naked eye it appeared uniform; but a good magnifier discovered in this, as well as in the other, some inequality of mixture, though the fusion was 2 or 3 times repeated, with the strongest degrees of heat we were capable of exciting by large bellows. 3. Twenty carats of gold and 4 of platina (= 5 : 1) were kept in strong fusion for above an hour and a half. These united into an equal mass, in which no granule of platina, or dissimilarity of parts, could be distinguished. The colour was still so dull and pale, that the compound could scarcely be judged by the eye to contain any gold. It hammered well into a pretty thin plate; but we could not draw it into wire of any considerable fineness. 4. Twenty-two carats of gold were melted in the same manner with 2 of platina (= 11 : 1) the

* The proportions were adjusted according to the carat weights, as it is by these that the fineness of gold is usually expressed. A carat is the 24th part of the whole compound: thus gold of so many carats, is a composition of which so many 24ths are fine gold, and the rest of an inferior metal.—Orig.

same that standard gold contains of alloy. The mixture was uniform, of a tolerable golden colour, but easily distinguishable from that of standard gold by a dingy bell-metal cast. It worked well, was forged into a thin plate without cracking, and drawn into moderately fine wire. 5. Twenty-two carats and a half of gold, and $1\frac{1}{2}$ of platina (= 15 : 1), melted into a uniform mass, which, after the usual nealing and boiling, proved somewhat tougher than the preceding, and of a better colour. 6. Twenty-three carats of gold were melted with one of platina; which is nearly half the proportion that standard gold contains of alloy. The compound worked extremely well, but was distinguishable from gold by a manifest dinginess, which it retained after repeated forgings, fusions, nealings, and boilings. 7. Twenty-three carats and $\frac{1}{4}$ of gold, and $\frac{3}{4}$ of a carat of platina (= 31 : 1), formed an equal mixture, very malleable, ductile like the three foregoing while hot as well as cold, but not entirely free from their peculiar dingy colour. 8. A mixture of $23\frac{1}{2}$ carats of gold, with $\frac{1}{2}$ a carat of platina (= 47 : 1), was very soft and flexible, of a good colour, without any thing of the disagreeable cast, by which all the foregoing compositions were readily distinguishable, in the mass as well as on the touchstone, from fine or standard gold. 9. A mixture of 23 carats and $\frac{3}{4}$ of gold, with $\frac{1}{4}$ of a carat of platina (= 95 : 1), could not be distinguished by the eye or hammer from the fine gold itself.

In all these processes, even where the proportion of platina was small, the fusion was performed by a vehement fire, that the mineral might be the more intimately dissolved, and equally diffused through the gold. The necessity of this precaution appeared from an experiment formerly related: in which one of platina having been melted with 4 of gold, the button appeared not much paler than standard gold with silver alloy. On a second fusion it lost its yellow colour, which had at first been only external, from an imperfect mixture, great part of the platina being concealed in the internal part of the mass, and covered as it were by a golden coat. The crucibles were rubbed on the inside with chalk, to prevent any particles of the metal from lodging in their cavities. A little borax was employed in each as a flux; with the addition of nitre, by which the colour of gold is somewhat heightened. On remelting some of the mixtures with sundry other additions, powdered charcoal seemed to improve the colour most.

Exper. 2.—The preceding compositions, after being gently hammered and boiled, were weighed hydrostatically with great care, by a very tender balance, in distilled water, wherein the gravity of standard gold turned out 17.788. All the mixtures proved heavier than standard gold. Their gravities were nearer to the medium of the gravities of the ingredients, than those of the compositions of platina with any of the other metals formerly given an account of; none falling considerably short of the mean gravity, and some rather exceeding it.

	Gravity, By experiment.	By calculation.	Difference.	
Platina	17.000			
Platina 1 gold 1	18.140	18.142	0. 02	diminution.
Platina 1 gold 3	18.613	18.714	0.101	
Platina 1 gold 5	18.812	18.904	0.092	
Platina 1 gold 11	18.835	19.094	0.259	
Platina 1 gold 15	18.918	19.142	0.224	
Platina 1 gold 23	19.089	19.189	0.100	
Platina 1 gold 31	19.128	19.213	0.085	
Platina 1 gold 47	19.262	19.237	0.025	increase.
Platina 1 gold 95	19.273	19.261	0.012	
Gold	19.285			

Exper. 3.—As a mixture of platina with an equal quantity of gold has been reported to be specifically heavier than gold itself, but turned out otherwise in the above experiments; some further trials were made on that head.

1. Instead of the crude mineral, whose gravity is but 17, we took platina, that had been cupelled with lead, one of the neatest of the buttons formerly mentioned, which, though retaining a portion of the lead, was nearly as ponderous as fine gold, viz. 19.240. This was melted with equal its weight of the gold, in a strong fire, and continued in fusion for about an hour; the mass proved spongy, and very light. We remelted it several times with vehement degrees of fire, suffering it to cool leisurely in the crucible; and, in order to separate as much as possible of the lead, to which its sponginess seemed owing, boiled it in aquafortis, and repeatedly injected corrosive sublimate on it during fusion; the mass, nevertheless, still turned out cavernulous and brittle, and specifically lighter than either the gold or platina by themselves. 2. Mr. L. likewise endeavoured to combine platina with small proportions of gold. By vehemence of fire, it was made to unite, though not perfectly, with half its weight and less; but the mixtures were extremely spongy and brittle; in specific gravity one scarcely 16, another less than 15. 3. As a cast metalline body from the Spanish West Indies, of which some account will be given hereafter, appears to have been confounded with the mineral platina, this also was melted with an equal quantity of gold. They united with great ease, by a moderate fire, into a uniform compound, tolerably compact, but whose specific gravity was only $16\frac{1}{2}$; which is nearly the mean gravity of the 2 ingredients.

Exper. 4.—As a small portion of copper somewhat heightens the colour of pale gold, platina was melted with 8 times its weight of standard gold made with copper alloy. The fusion was performed, as in the preceding experiments, in a close crucible, with a strong fire, but without any flux, and continued for about an hour. The metal appeared covered with a black scurf, and had lost about $\frac{1}{100}$. It was much duller coloured, harder to the hammer, and cracked sooner

about the edges, than mixtures of fine gold with a larger quantity of platina. By repeated fusion, and frequent nealing, it became a little softer and tougher, so as to be drawn into pretty fine wire; but the colour was still exceedingly dull, more resembling that of bad copper than of gold. The specific gravity of this compound was 17.915; a little less than the medium of the 3 ingredients unmixed, and a little greater than the mean gravity resulting from the platina by itself, and the copper and gold mixed; for copper, in the standard proportion, appears to diminish the gravity of gold more than it ought to do according to calculation.

From the foregoing experiments it appears, that platina is miscible with gold, in certain proportions, without injuring either its colours or ductility, or occasioning any considerable alteration in the gravity: experiments related in former papers have showed, that it stands aquafortis, and the other trials by which the purity of gold is estimated. It is to be hoped, that the abuses manifestly practicable by this mineral have hitherto been but rarely made use of. To guard against them is the object of this paper; to detect them, of the next.

PAPER VI.—*Experiments for Distinguishing and Purifying Gold mixed with Platina.*

1. *By Amalgamation with Mercury.*—In an experiment related in the 4th paper, an amalgam of 1 part of platina and 2 of gold, with a suitable quantity of mercury, having been triturated with water for a considerable time, and occasionally washed over, the platina was gradually thrown out, and the gold retained by the quicksilver. Repetitions of this experiment have showed, that though the separation succeeds in some cases, it does not perfectly in all: that if there is any particle of the platina imperfectly dissolved in the gold (which will generally be the case, unless the quantity of gold is 3 or 4 times greater than that of the platina), this part will be retained, after long trituration, undissolved by the mercury, uncomminuted by the pestle, and too ponderous to be washed off in its gross form. A variety of mixtures of platina and gold were treated in the manner above described; and the gold, recovered from the amalgams, submitted to further examinations. Where the proportion of platina was large, the microscope almost always discovered still some granules of it on the fracture of the ingot; where the proportion was small, the recovered gold was frequently, but not constantly, found to be pure.

From these experiments it appears, that mercury has a greater affinity with gold than platina, and that platina is capable of being totally separated by elutriation; but that the process is too vague and undetermined to be applicable in the way of assay, as we have no mark of the precise time for discontinuing it, and as we can never be certain, without making another assay, whether the whole of the platina is separated or not. As a preparatory examination, where

the quantities of platina and gold to be separated are large, it is however of good use, as greatest part of the platina may by this means be washed over with little trouble, and the gold brought into a less compass, so as to be commodiously submitted to a perfect purification by the means hereafter pointed out. This process has a similar effect on platina and gold to that of stamping and washing on metallic ores; which could not be reduced into pure metal in the furnace to advantage, without the previous separation of great part of the earthy and stony matter by water.

2. *By Precipitation with Alkalies.*—Gold is precipitated totally by fixed alkaline salts, but platina only in part. When solutions of the two metals are mixed together, so much of the platina remains suspended, after saturation with the alkali, as to be readily distinguishable by the yellow colour, which it communicates to the liquor. It has been objected, that though the platina was discoverable, when thus mingled superficially with the gold, it may nevertheless, when combined more intimately by fusion, elude this method of trial.

1. Mixtures of gold with small proportions of platina were therefore kept in fusion, by a very strong fire, for several hours, and afterwards dissolved in aqua-regis. The solutions being diluted with water, and a pure fixed alkaline salt gradually added, so long as any effervescence or precipitation ensued, the liquors remained manifestly coloured, though apparently paler than when the two metals had been dissolved by themselves. 2. A more convincing proof, that part of the platina remains suspended, after the precipitation of the gold, was obtained, by putting into the filtered liquors some plates of pure tin, which presently contracted an olive hue, and threw down a large quantity of a brownish precipitate, as from the common solutions of the crude mineral. It was observable, that the tin plates were often sensibly acted on, even while the liquor was overcharged with alkali. 3. It has been further suggested, and with great appearance of probability, that as a part of platina is precipitated as well as gold by alkaline salts, if only this part be mixed with gold, it will be thrown down by them again on dissolving the compound. To determine this point, a precipitate of platina made by fixed alkali was melted with thrice its weight of fine gold, and kept in strong fusion for above an hour: they united more easily than gold does with so large a proportion of the crude mineral, and formed a smooth neat bead, which hammered well into a pretty thin plate before it cracked, and appeared internally uniform and equal. This compound being dissolved in aqua-regia, and a fixed alkaline salt added by degrees till the acid was more than saturated, the liquor became indeed pale; but tin plates put into it quickly discovered that it held a very considerable quantity of platina. It appears therefore a constant property of this mineral to remain partially dissolved in the neutralized liquor; and that minute proportions of it, mixed with gold, are by this means distin-

guishable. 4. Many other experiments were made of the precipitations and precipitates of gold and platina, by alkalies, both of the fixed and volatile kind. The most remarkable effects were, that volatile alkalies added to both solutions, in quantity just sufficient to saturate the acid, precipitated gold entirely, but platina only in part, so much of it remaining suspended as to give the same colour to the liquor as when fixed alkalies were made use of: that, on adding a larger quantity of the spirit after the precipitation of the gold, the liquor became yellow, a part of the metal being taken up again; and that the platina was more copiously redissolved, the liquor becoming of a deep brownish red: that the washed precipitates of both metals, whether made by volatile or fixed alkalies, proved soluble, by moderate digestion, in spirit of salt; those of platina much more difficultly and sparingly than those of gold.

3. *By Inflammable Liquors.*—1. Inflammable spirits, which revive gold from its solutions in form of yellow films, have no such effect on solutions of platina. This experiment affords not only a criterion for distinguishing with certainty whether gold has been debased by platina, but likewise an infallible means of recovering it perfectly pure from any admixture of that mineral. If the compound be dissolved in aqua-regis, the solution mingled with twice its quantity or more of the spirit, and the mixture suffered to stand for some days in a glass slightly covered; the whole of the gold arises to the surface, leaving the whole of the platina dissolved. The golden pellicles may be collected, by pouring the matter into a filter just large enough to contain it. The dissolved platina passes through, leaving the gold on the paper, which is to be washed with fresh parcels of water till the liquor runs colourless. The paper is then to be carefully squeezed together, and burnt in a crucible previously lined with vitrified borax, when fully sunk down, a little fresh borax is to be injected, and the fire raised to melt the gold. The use of lining the crucible with borax is to prevent any moleculæ of the gold from lodging in its cavities.—This process is attended with one inconvenience, the slowness of the separation of the gold from the solution; this may be in some measure expedited by employing a spirit, which has been distilled from vegetables, that give over an essential oil. 2. As essential oils take up gold from aqua-regis, and keep it dissolved for a time on the surface of the acid; a pure colourless oil, that of rosemary, was poured into about half its quantity of a solution of platina, the mixture well shaken, and suffered to rest: the oil quickly arose, without taking up any thing from the platina, or receiving any colour: the acid liquor underneath remained coloured as at first. Compositions of platina and gold being dissolved in aqua-regis, and treated in the same manner, the whole of the gold was imbibed by the oil, and the whole of the platina remained dissolved in the acid. The oil, loaded with the gold, appeared of a fine yellow colour, and, on standing for a few hours, threw off great part of its

contents, in bright yellow films, to the sides of the glass. Sundry other distilled oils were made trial of, with the same event. The gold is easily recovered, by setting the oil on fire; and, when thoroughly burnt out, melting the residuum with borax, as in the preceding experiment. After the separation of the oil employed at first, it may be proper, for the greater security, to add a little more; which, if any part of the gold should happen to have been left in the liquor, will effectually take it up. 3. The experiment was repeated likewise with the subtile fluid, prepared from vinous spirits with the vitriolic acid, called by the chemists æther. The separation succeeded in the same manner as before; the æther receiving nothing from pure platina, but instantly taking up the gold from a mixture of the two. It is observable, that the gold imbibed by this fluid is kept permanently dissolved by it; without separating or reviving, as it does from the common essential oils and vinous spirits. 4. The liquors remaining in these experiments, after the extraction of the gold, appear on all trials the same with the common solutions of platina; and readily betray their being impregnated with that mineral by their colour, by the precipitation with tin, by their yielding a sparkling red precipitate with volatile spirits, &c. A far more minute proportion of platina, mixed with gold, is more distinguishable by these processes, than by those with alkaline salts abovementioned; these exhibiting the whole of the platina dissolved by itself, those only a part of it.

4. *By Metalic Solutions.*—All the metals, which precipitate gold from aqua-regia, have been already shown to precipitate platina also. As gold is thrown down by some metallic solutions, as well as by the metals in substance, particularly those of mercury, and iron, it remains to apply these liquors as precipitants for platina.

1. A saturated solution of mercury in aquafortis, which readily and totally threw down gold in its metallic form, being added to a solution of platina, the liquor became immediately turbid, and, on standing for a little time, nearly the whole of the platina fell to the bottom. A solution of mercury in the marine acid, or of corrosive sublimate, likewise precipitated platina, but less perfectly, and with this difference, that the former precipitate was of a greyish brown colour, the latter of a sparkling red. 2. Solutions of iron in the vitriolic acid, or of common green vitriol in water, which totally threw down gold, happily made no change in solutions of platina. Compositions of platina and gold being dissolved in aqua-regis, the solutions diluted with about twice their quantity of water, and a filtered solution of the vitriol gradually added; the mixtures instantly grew turbid, and, on standing, deposited the gold in form of a purplish grey calx, the whole of the platina remaining dissolved. It appeared, on numerous repetitions of this experiment, that no part of the platina was precipitated along with the gold, nor any of the gold kept suspended with the platina.

Where the quantity of the mixture to be assayed was very small, the precipitation was usually performed in a filter, that the gold, which separates in very minute moleculæ, some of which might possibly remain unobserved in the bottom of a glass, might be detained on the paper. The colourless sorts of filtering paper are preferable for this use to the coloured; as these last may be impregnated with astringent matter, which would extricate some of the ferrugineous part of the vitriol. The vitriol was dissolved in about 6 times its quantity of water, and a few drops of oil of vitriol added, to prevent the separation of any of its iron in the filter. This solution was put into the filter first, the solution of gold and platina immediately poured into it, the whole stirred together with a clean glass rod, and such part of the liquor, as had run through before they had been duly mixed, poured back to the rest. The gold remaining in the filter was washed with fresh parcels of water, the paper cautiously rolled up, and burnt in a crucible, as mentioned in a former experiment. 3. Solutions of the vitriol, recommended by Kunckel and others for precipitating gold of an uncommonly high colour, made no change in the solutions either of gold or platina. The bluish green did indeed precipitate the gold; not as blue vitriols, but by virtue of the ferrugineous matter, of which these kinds largely participate. White vitriol was likewise made trial of, but without producing any sensible effect in either solution. 4. The experiments with green vitriol were repeated on the solutions of platina and gold made in spirit of salt. The event was the same as with those made in aqua-regis; the gold being constantly precipitated, and the platina remaining dissolved.

Remarks.—It may be proper to observe that by the processes here pointed out, the gold is purified from other metallic admixtures at the same time that it is separated from platina; the inflammable spirits reviving, essential oils and æther imbibing, and green vitriol precipitating, gold alone. Care should be had that the piece of the mixture, taken for examination, be totally dissolved before any trials are made with the solution; the menstruum not acting with equal facility on the two metals, but dissolving the gold more readily than the platina. Where the acid has been dilute, and only a gentle heat applied, great part of the gold has appeared to be taken up before the platina was considerably acted on. Where the filter, with the gold in it, is burnt in the crucible, borax is the most commodious flux; but as this salt gives a sensible paleness to gold, a little nitre may be injected after the metal has come into fusion, to restore its colour. If the nitre was added at first, while the gold continued subtilely divided, some particles of the metal would be dissipated during the deflagration, which that salt produces with the coaly remains of the paper.

As the foregoing experiments exhibit platina and gold dissolved in a mineral fluid, which by simple mechanic agitation rejects the one and retains the other,

and which discovers this different appetite of union so much the more remarkably, as the two metals have been the more intimately combined:—as they further exhibit platina dissolved in liquors incapable of holding gold suspended,—gold dissolved in liquors incapable of holding platina suspended,—gold totally precipitated by substances, which precipitate no particle of platina,—and gold, when mixed per minima with platina, perfectly recovered from it by these means, without increase as well as without diminution:—it follows that platina is not, as some believe, gold naturally debased by the admixture of some other metallic body, but a metal of a peculiar kind, essentially different from all the others. Before the discriminating characters of platina were discovered, such a notion was highly plausible, and direct experiment seemed to confirm it: a portion of the platina might be separated in the process; a quantity of gold mixed with the remainder, in order to collect the gold supposed to be contained in it; the mixture submitted to operations, which gold alone was supposed capable of withstanding; and the augmentation, which the noble metal still retained, held to be true gold gained from the platina.

The methods of trial above related will, it is presumed, be sufficient to undeceive those who may have been imposed on by such appearances, and betrayed into the practice of unintended frauds: to convince them that all they have gained from platina, after the most laborious attempts to divest it of its imaginary coat, is no other than platina still: and, which is of more extensive utility, to distinguish all the abuses, that may be made with this metal, and restore the gold, so debased, to its original purity and value.

XXI. On the Temple of Serapis at Pozzuoli in the Kingdom of Naples. By the Rev. John Nixon, M. A., F. R. S. p. 166.

Before entering on a more particular consideration of this noble piece of antiquity, it may not be improper to premise the general account (and indeed the only one yet published,) given of it by Messrs. Cochin and Bellicard, in a little treatise printed at Paris in 1755. These gentlemen acquaint us, that in 1749 there were only 3 pillars of this building visible, and that they were buried half way within the ground: but that soon after, workmen being employed by order of the king of the Two Sicilies to dig at the place, they came to the pedestals of those pillars; and at length discovered the building to have been a temple, which, as it was judged by the principal idol found there, and some other circumstances, was dedicated to Serapis. They tell us further, that many statues and vases of excellent workmanship had been taken out of the ruins; and that the whole temple was extremely magnificent, being built, or cased throughout, with marble, even to the parts appropriated to the meanest offices.

In order to form any conjecture concerning the antiquity of the building

before us, it may be observed that the worship of Serapis, to whom it is supposed to have been consecrated, was not introduced at Rome till towards the end of the republic; and then tolerated in the suburbs only. However, at length he was allowed to have temples erected to him within the precincts of the city; chiefly by the authority of Vespasian, who was thought to have restored a blind man to his sight at Alexandria by the aid and direction of this deity. And on this account he continued to be held in high veneration by Titus and Domitian, the sons of that emperor, as appears by their stamping his image on the reverse of their coins. Now as it is reasonable to suppose that the other cities of Italy followed the example of the metropolis in this instance, as we find they did in others of a similar nature, we may with some probability place the foundation of this temple at Pozzuoli somewhere within the period above assigned.

As for the particular state of this building, it is situated on the west side of the town, near to, and on a level with the beach, (fig. 1, pl. 4.) Its grand entrance is towards the south, and seems to have been a vestibule supported by 4 columns. This introduces you into a spacious portico, or corridor, which was designed to defend such as assembled here to worship from the injuries of the weather; as also to afford a commodious passage into a range of rooms of different dimensions, disposed on all the 4 sides of the court. These chambers seem designed for preparing the sacrifices, lodging the priests, and keeping their vestments; as also the fuel, stores, and other things requisite for the service of the temple: not to omit the convenience of purifying both the priests and the worshippers by bathing or washing. This last destination is countenanced, with regard to the chamber on the north-west and that on the north-east corner, by the row of stone seats, which still remains on each of the sides of the former. These seats have a gutter, or channel running along at the foot of them on the floor; and are likewise perforated with holes of a proper size, with funnels passing from them below. On these benches probably the persons to be purified placed themselves, that the water might be let out upon them from pipes; or administered in vases or ewers by the attendants, and afterwards be carried off by the passages above-mentioned.*

One more particular concerning the apartment in the north-west angle of the temple, is, that when it was cleared of its rubbish, there was found in a niche in one of its sides a male and female figure naked, and in the most flagrant act of natural lewdness. It is now in a private room in the palace at Portici, nor can be seen without the king's special permission. In the same place probably may

* Mess. Cochin and Bellicard seem to think this room was intended for another purpose, by their calling the funnels under the holes in the seats of it, conduits des fossés d'aisance. Which of the two hypotheses is to be preferred, is submitted to the judgment of the learned; or rather, whether both of them may not be admitted, as in nowise incompatible together.

stand the statue of a satyr in an unnatural action with a goat, which was found at Herculaneum, and is, they say, of exquisite sculpture, but concealed in the palace above-mentioned with the same strict care as the former. In the grand entrance, having passed through the corridor above described, we come to a square court, or atrium, paved with large slabs of white marble streaked with blue or greyish veins. At the distance of 25 feet further, in the centre of the court, stood the temple properly so called, containing a circular area of 54 feet diameter, and elevated above the level of the pavement, so as to admit an ascent to it of 5 steps, in 4 different parts, answering to the 4 sides of the corridor. This area is surrounded with 16 pedestals, on which formerly were columns to support a rotundo or dome. Against each of these columns, on the outside, there seems to have been placed a statue, and, in the intermediate spaces, vases for incense, or lavers for washing, on low stands on the floor. In the middle of the temple was erected the grand altar, the traces of which still remain, with a sink or drain near it to receive and carry off the blood of the victims, &c.

Northward of the temple, and at the distance of 25 feet, being the same space that was between it and the corridor at the entrance, was once another vestibule or pavilion, supported by 4 columns 4 feet and a half in diameter, and of the Corinthian order, as appears by 3 of them which still subsist standing in a line with the outer face of the corridor. This pavilion (if we may judge by analogy from what we find in other temples) led to an inner recess or sacrarium, terminating probably in the segment of a circle: but of this we had no certain proof, as the rubbish was not yet removed from this part of the building.

I beg leave, says Mr. N. further to mention a remarkable appearance in some of the columns of this temple, viz. that that part of them which was lowest, as well as that which was nearest the capitals, is well preserved and pretty entire; while part of the intermediate space, for two or three feet together, is discoloured, as if it had suffered by burning; and also excavated in such a manner as to contain multitudes of little shell-fish, which appear, like the pholades in some stones, almost totally inclosed within their cells, so as not to be got out without breaking. I know no way to account for this so probable, as by supposing that the lower parts of these columns were secured by the mass of rubbish that inclosed them, as the uppermost were, by their elevated situation, from being perforated either by the corrosive quality of the sea, which (according to tradition) formerly covered the site of this temple, or by the animalcula bred in that element; while the middle parts standing in the water were (perhaps for ages) exposed to the injuries mentioned above. I had no opportunity of taking the height of the uppermost line, where the above-mentioned alteration in the columns ended, from the level of the sea in the bay; which would have showed

how high the water must have risen formerly above its present mark to produce the effect ascribed to it on this hypothesis. But, however that may be, the nature of the situation of this place being considered, the innovation supposed to have happened in it will not be thought improbable; especially in a country so plentifully stored with combustible matter in its interior parts, and consequently so liable to changes in its outward form as this is, and has been for many ages. For an extraordinary instance of this, we need go but a little way from this place, viz. to Il Monte Nuovo, a hill about 4 miles in compass, which in 1538 was cast up in one night's time by an eruption, by which the greatest part of the Lucrine lake was filled up, and the town of Tripergola, with a church, convent, hospital, and other buildings, entirely buried.

XXII. A Parthian Coin with a Greek and Parthian Legend, never before published. By the Rev. J. Swinton, M. A. of Christchurch, Oxon, F. R. S. p. 175.

This medal is in very good conservation, and approaches near the size of those of the middle Roman brass. It exhibits the head, or effigies, of a Parthian king, with a beard, diadem, and hair formed into such curls as was never yet observed on any ancient coins. Under the effigies, the Greek letters BACIΛEωN MEΓAC MO∴∴∴, which demonstrate the piece to be Parthian, appear; and, on the reverse, a victory, done something after the Roman manner, though the workmanship is pretty rude, presents itself to our view, together with a legend in a language and character at this time unknown. The legend consists of 10 complete elements, or letters, placed behind the victory above-mentioned; besides which, there is one in the field of the medal, being probably the initial letter of the name of the city where the coin was struck. The metal, though here termed brass, discovers something of a composition similar to that of the Duke of Devonshire's medal of Vologeses III, as described by Sig. Haym. Of this coin a draught is accurately shown, fig. 2, pl. 4.

Mr. S. has no doubt but this medal or coin was struck when Monneses filled the Parthian throne; and that the inscriptions on the two sides of the piece are of similar import, the one in Parthian Greek, and the other in Palmyrene characters, and importing as much as "Monneses the great king of kings." Also that it was probably struck in the 425th or 428th year of the Parthian era, that is, the 197th of Christ.

XXIII. Of a Red Coral from the East Indies, of a very singular Kind: in a Letter from Mr. John Ellis, F. R. S.* p. 188.

This piece, fig. 3, pl. 4, is of a very singular kind. The stem and branches

* The coral here described is the *isis ochracea* of Linneus.

appear evidently to the naked eye to consist of a combination of vermicular tubes closely connected together: and, if we trace these little tubes to their starry openings on the surface, at B, we shall plainly discover them to be the red testaceous coverings of certain marine polypes, which have raised themselves thus upright, and disposed themselves into this remarkable vegetable form.

In order to form some idea, how these masses are increased and extended to the sizes they are often met with, and where the same regularity of shape is preserved in the large that we find in the small; it is more than probable to suppose that the species of polypes that compose this coral, breed as we find all other polypes do: and this appears more evident from what Mr. E. has discovered in many kinds of corallines (see plate 38 of his Essay on Corallines,) where the young polypes in some species are produced in the egg state, while others fall in great numbers from their matrices, completely formed, down to the roots of their parent corallines, either to begin a new race of the same species near them, or to increase the trunk, and extend the ramifications of the plant-like figure from which they had just descended.

From observing this method in nature we shall the easier account for the progress of those generations of young testaceous polypes of this coral; which appear succeeding each other, and raising themselves up from the root or base, passing along the stem and branches, and covering the whole anew with their shelly cases: and in this their passage upwards we may observe, in the specimen before us, how they have involved and incrusted the small lateral branches of the former generation, so as almost to hide their appearance. Hence we may trace them extending to the extremities of the upper branches, and there forming a new series of slender twigs, proportionable to those they had just covered, still keeping order and exact symmetry in the whole structure.

The distinguishing character of this red coral, after we have considered its fistulous texture, is the knotty joints of which it is composed: these appear more distinct, and are placed at a greater distance in the smaller branches than the large; and, as we descend to the trunk, the traces of these inequalities but just appear. From these protuberances, or knots, the lateral branches take their rise; and as these and the leading branches grow up together, they frequently inosculate at these joints, forming a kind of network, like what we observe in many of those species of keratophyta called sea-fans.

The surface of this coral, when recent, is covered with a mealy friable matter, of a yellow colour, not unlike that of the true red coral, but much fuller of little raised starry cells. The figure of these cells is owing to the radiated position of the claws of the polypes. On removing this friable matter, we observe that the polypes of these cells have had a communication with a small hole or opening into one of the tubes that lie immediately under it. This frequent in-

tervention of the openings of the small tubes, or their ramifications, between the sides of the larger ones, makes the latter appear more irregular, and not so parallel, as in the true red coral; where we find fewer stars; and where those occur, we may observe it always alters the direction of the tubes.

Many of the tubes of this coral appear through a magnifying glass full of holes, like those he has described in the keratophyton (plate 26, fig. G, p. 62, of his Essay on Corallines;) and these holes appear more distinctly when we examine the half tubes, or broken irregular ones, on the stem and great branches of this coral. Further, if we compare the transverse section, at the base of this coral, with a section of a common rattan cane, they will both appear full of holes in the same regular order, and of nearly the same diameter: whereas the tubes on the surface of the stem of this coral, look as irregular as so many holes pierced or eaten out by worms.

I hope, says Mr. E. by this time our ingenious botanical friends, whom we could not persuade to part with these beautiful sea-productions from the vegetable kingdom, are thoroughly convinced that this mealy, friable, or calcareous covering, full of starry cells, which we are sure to find covering all the recent red corals and keratophyta, is not a mere blight of insects common to the sea vegetables as well as land ones, which they have formerly insisted on; but that they will consider this covering, for the future, as proper and necessary for the well-being of these little animals, as they do at present hair and wool for beasts, feathers and down for birds, and scales and slime for fishes.

On examining this coral in the microscope, it is observed that the outside tubes of the stem are chiefly stony, but that the inner parts are composed of as many divisions of spongy tubes as there are of stony ones. This arises from the smaller ramifications, which being spongy at the knobs, and stony in the spaces between them, are inclosed and united together into one common mass during the growth of this coral; so that both the soft and hard parts together make up the inside of its trunk or stem. When we examine minutely the two parts that compose the branches, we find that the knobs consist of little sponge-like tubes interwoven together, as they appear magnified at fig. D; and the shank or part between the knobs is composed of stony tubes, that are more erect (see the piece magnified at E:) these tubes appear to be branched from the lateral holes at FF. The fig. E likewise shows the appearance of the tubes on the surface of the main stem. The radiated openings in the little wart-like figures on the surface of the branches are guarded by 8 pointed valves, as magnified at fig. I. these inclose heads of the polype, one of which is figured at K.

The stem of this specimen is so entirely divested of its yellow mealy covering, that we may easily trace the manner in which the animals that compose it have carried up their stony tubular cells, which lie side by side along the surface.

These tubes have still some marks of sponginess at particular distances which, as they come to join together, form those irregular cross-lines that surround the stem in several places. See fig. A. The sponginess of the knobby joints occasions that excessive brittleness in the lesser branches; which accounts for the difficulty, which Rumphius mentions, of getting good specimens of this beautiful coral.

XXIV. *Effects of a Storm at Wigton in Cumberland. By Mr. T. Thomlinson.* p. 194.

On the 6th of Oct. 1756, at night, happened a most violent hurricane, such as has not been known in these parts in any one's memory. It lasted 4 hours at least, from about 11 till 3. The damage it has done is very deplorable. The corn has suffered prodigiously. Stacks of hay and corn have been entirely swept away: houses unroofed, and in several places driven down by its fury: trees without number torn up by the roots; others snapt off by the middles, and their fragments scattered over the adjoining fields. Some were twisted almost round, or split down to the very ground; and, in short, left in such a shattered, mangled condition, as scarcely any description can give an adequate idea of. The change in the face of the country was very surprising in one single night: for, to complete the dismally-desolate scene, the several tribes of vegetables (in all their verdure the day before,) as if blasted with æthereal fire, hung down their drooping heads. Every herb, every plant, every flower, had its leaves withered, shrivelled up, and turned black. The leaves on the trees, especially on the weather side, fared in the same manner. The evergreens alone seem to have escaped. The grass also, in a few days time, recovered itself in a great measure.

Mr. T. agreed at first with the generality of people in their opinion, that lightning had done all this mischief: but on recollecting that there had not been much seen any where, in many places none at all, but that the effect was general, as far as ever the wind had reached; he began to think that some other cause might probably be assigned. Accordingly, he examined the dew or rain, which had fallen on the grass, windows, &c. in hopes of being enabled, by its taste, to form some better judgment of the sulphureous or nitrous particles, or of whatever other quality they were, with which the air was so strongly impregnated that night, as to produce such strange effects. Nor was he deceived in his expectations: for on tasting it, he found it as brackish as any sea-water. The several vegetables also which he tasted were all salt, more or less, and continued so for 5 or 6 days after; the saline particles not being then washed off, from the corn and windows in particular; the latter of which, when the moisture on the outside was exhaled next day, sparkled and appeared exceedingly brilliant in the sunshine. This saltness he conceived had done the prin-

cipal damage: for common salt dissolved in water, he found on experiment on some fresh vegetables, when sprinkled 2 or 3 times, on them, has the very same effect, except that it does not turn them quite so black; but particles of a sulphureous or* other quality, may have been mixed with it. That this salt water had been brought from the sea,† every body will allow; but the manner how,‡ is not so easy to conceive.

XXV. The Effects of Lightning on the Steeple and Church of Lestwithiel, Cornwall. By Mr. John Smeaton, F. R. S. p. 198.

January 25, 1757, about 5 o'clock in the evening, returning home from the Edystone works near Plymouth, Mr. S. observed 4 flashes of lightning, within the space of 6 or 7 minutes, towards the west; but heard no noise of thunder, distance about 30 miles. A few days after, he was informed, that the same evening the lightning had shattered the church of Lestwithiel in a very surprising manner. At the time before-mentioned, the inhabitants were alarmed by a violent flash of lightning, accompanied with thunder so sudden, loud, and dreadful, that every one thought the house he was in was falling upon him; almost every one being within doors, on account of a violent shower of rain, which preceded the lightning: so that nobody knew any thing of the mischief done to the church, till it was observed accidently after the shower.

The steeple is carried up, plain and square, to about 49 feet, with a kind of slate-stone rough-cast on the outside; on which is formed a very elegant octagon Gothic lantern, about 9 feet high, and on it a stone spire about 52 feet high, with a spindle and vane rising about 3 feet above the stone: so that the whole together was about 113 feet. Each face of the lantern finishes above with a sort of Gothic pediment, with a little pinacle on each, separated from the body of the spire.

The vane was much bruised, which might be occasioned by the fall; but the socket was rent open, as if it had been burst by gunpowder; and in such a manner as could not well be occasioned by the fall. Under the spindle that carried the vane, was a bar of much the same size and length, that passed through the centre of several of the uppermost stones successively, to unite them the more firmly together, and was run in with lead: all which surrounding stones were broken off, except one, which, together with the bar, fell down within the tower.

* In an adjoining bleach-yard, a piece of cloth, which had been left out all night, was turned yellow; and was not without some difficulty washed out again. Some also, which was spread out the next day, contracted the same colour.—Orig.

† The wind was westerly, and consequently would sweep the Irish sea.—Orig.

‡ No rain, or however very little, during the hurricane.—Orig.

The shell of the spire as far down as 35 feet from the top, was no more than 7 inches thick, and the courses about the same height: so that scarcely any one stone in the spire could weigh more than 30 or 40 pounds; but they were joined together at the ends with mortoise and tenon in a curious manner. Above 20 feet of the upper part was entirely thrown down, and dispersed in all directions; and some pieces were found at the distance of 200 yards. A great many stones fell on the roof of the church, breaking the pews, and whatever they fell upon. Six feet still lower the spire was separated; the western half being thrown down; the eastern half was left standing, but disjointed, and in so critical a posture, that it seemed ready to fall every moment: so that this was ordered to be taken down immediately; and likewise to 6 feet below, the work being found remarkably shattered. The whole of the spire he found much cracked and damaged, but the remainder of the 7 inch shell so greatly, that there seemed scarcely a whole joint.

The pediments over every face of the lantern were damaged more or less; but the whole ashlering of that to the N. W. was torn off from the inner wall, to which it was connected. Several of the pediments were damaged, and even stones struck out, where the little pinacles above them were left standing.

About the top of the lantern is a bell for the clock to strike on: it is hung on a cross-bar, with gudgeons at each end; the whole being suspended to a beam laid across the tower. The cross-bar was so bent, that the clock-hammer would not touch the bell by above 2 inches. This could not be done by the falling of stones, because the beam would defend the bell from receiving any stroke in the direction to which the cross-bar was bent. As to the wire that drew the hammer, not one bit of it could be found.

The bells, 4 in number, for ringing, hung in the square part of the tower below the lantern, 2 above and 2 below: the wheels of every one were broken to pieces, and one of the iron straps by which they were fastened to the yoke, unhooked. Whether these accidents were occasioned by the lightning or the falling stones, he leaves undetermined. In the floor under the bells was placed the clock, cased up with slight boards. The verge that carries the pallets was bent downwards, as if a 10 pound weight had fallen 10 feet high right upon it. The crutch that lays hold of the pendulum, looked as if it had been cut off by a blunt tool, and heated by the blow, till it was coloured blue at the place where it was cut. It turned at a right angle, and might be about $\frac{4}{10}$ of an inch broad by $\frac{2}{10}$ thick. As to the pendulum which hung pretty near the wall, the upper part of the rod was struck with such violence against the wall, that a sharp impression of it was made in the plaster: and near the upper part of the impression appeared a circular shady ring, of a blackish colour, something like as if a pistol had been discharged of powder, and the muzzle held near the wall. In this

story several stones were forced out of the walls. The walls of the belfry or tower were much damaged; several stones driven out, and perforations made in the solid wall, particularly one of 14 inches square and 6 inches deep, so truly square and regular, as if cut out by art. All the windows in the church either broken out, or bagged outward.

XXVI. On the Case of the late Horace Lord Walpole; being a Sequel to his own Account published in the Phil. Trans. vol. xlvii. p. 43 *and* 472. p. 205.

I. Copy of a Letter from John Pringle, M.D., F.R.S. to Dr. Robert Whytt, Professor of Medicine in the University of Edinburgh, and F. R. S., relating to the Case of Lord Walpole; with Dr. Whytt's Answer. Communicated by Dr. Pringle. Dated London, 22 *Feb.* 1757. p. 205.

In the month of March 1756, about 10 months before his lordship's death, Dr. P. happened to meet him at a friend's house, where he dined, and never saw any man of his age with a more healthful appearance. He was then in his 78th year. He ate with an appetite, and of a variety of dishes; drank some Madeira, and was very chearful the whole time. His lordship said that he had enjoyed perfect health since he sent his case to the Royal Society; that he thought it probable there was still a stone in his bladder, but so diminished or smoothed, as to give him no uneasiness; that he did not think it safe to go about the streets of London in a coach, but that he went every where in a chair; and that in the country he could travel 40 miles a day in his post-chaise without fatigue, or feeling any of his old pains on the motion. That he continued to drink, for a constancy, 3 pints of oyster-shell lime-water daily; and to take as often from half an ounce to a whole ounce of soap, by way of lenitive.

From this time to the beginning of winter, Lord Walpole (as Mr. Graham, his apothecary, informed Dr. P.) continued in the same state of health; but some time after coming to town, his lordship was seized with a lingering feverish disorder, very much affecting his spirits, but entirely unconnected with the stone. Dr. Shaw, who attended his lordship for about a fortnight before his death, said that there had never been any stoppage of water, or passing of bloody urine, or any pain about his bladder or kidneys during his last illness; but that he now and then felt some irritation in making water, a symptom too inconsiderable to require any other medicine than the continuation of his lime-water; which in a smaller quantity he drank till within 2 or 3 days of his end.

Mr. Ranby and Mr. Hawkins, surgeons, with Mr. Graham, were present at the opening of the body; and from the last 2 Dr. P. received the account of the dissection. The coats of the bladder appeared to be a little thicker than natu-

ral, but were otherwise sound. The glandula prostata was of a large size, but not distempered. They found 3 calculi, 2 lying loose in the bladder, and the other a very small one sticking in the passage, at that part which is surrounded by the prostate gland. The first 2 were very much alike, being of the shape and size of the kernel of a Spanish nut; only the sides were irregularly flattened, but without forming any sharp angle. The surface of each was every where smooth, except where there had been a separation of some small scales, not so thick as one's nail; and the largest exfoliation from one of these stones appeared to have been nearly about the breadth of the nail of his little finger. The polish otherwise, as well as the colour of both, might be compared to a boy's marble. One of these calculi weighed 21 grains, the other 22 grains: they were heavy for their bulk, and seemingly of a hard substance. The smallest stone was about the size and shape of the seed of an apple, with the point broken off and the edge ragged. This, as before observed, was found in the passage, seemed to be coming away, and probably had occasioned that irritation the patient had now and then felt during his last illness. It weighed only about a grain. No parts could have a sounder appearance than both the ureters and kidneys. The first were not dilated; nor did the last contain any stone, mucus, or gravel: the pelvis in each was of a natural size.

The rest of the abdominal viscera were in the same healthful state, except the gall-bladder, which was full of stones. The largest was about the size of a small chestnut, but rounder. The surface was smooth, particularly at one part, where it seemed to have rubbed on a smaller calculus, of the shape of one of the vertebræ of a small animal, without the processes. This last had a hollow on each side, corresponding to the convexity of the large stone: and these cavities being finely polished, it seemed as if sometimes one side, sometimes the other, of the small stone had been turned to the great one, and had been shaped in that manner by the attrition. The largest calculus weighed 1 dr. 2 scr. 2 grs.; the small one but 9 grs.: they both sunk in water; and felt specifically heavier than any stones he had ever seen taken out of the gall-bladder. Besides these 2, there were several very small calculi of irregular shapes, and of rough surfaces, which altogether did not weigh above 5 grs. Mr. Graham, who had attended his lordship for about 40 years, assured Dr. P. that he never had any symptom that indicated a stoppage of the bile, or the passage of a stone from the gall-bladder into the intestines. Neither the head nor breast were opened.

II. Observations on the Case of the late Lord Walpole, of Woolterton. In a Letter to Dr. John Pringle, F. R. S. By Robert Whytt, M.D., F.R.S. Dated Edinburgh, March 16, 1757. p. 209.*

Physicians have not perhaps differed more widely in any thing than in their opinions of the medicines lately proposed for the cure of the stone. While some imagined, that Mrs. Stephens's medicines, or soap and lime-water, were in most cases to accomplish a dissolution of the stone; others have been positive, that nothing of this kind was to be expected from them: nay, they have condemned these medicines, when used in large quantities, and long persisted in, as hurtful to the stomach, guts, and urinary passages; and have ascribed the remarkable ease, which they almost always give to calculous patients, to their depositing a calcareous powder on the surface of the stone, by which it is rendered less hurtful to the bladder. And this opinion seems to have been not a little strengthened by the great quantity of white sediment observed in the urine of those patients who have used soap and lime-water in considerable quantities. Now as I am of opinion (says Dr. W.) that most of these objections and doubts, concerning the effects of soap and lime-water in the cure of the stone, may be cleared by a

* Dr. Robert Whytt, a celebrated professor of medicine in the university of Edinburgh, and author of some valuable physiological works, was born in 1714. He studied physic first at Edinburgh, and afterwards at Leyden, but took his degree of M.D. at Rheims. On his return from the continent, after passing some time in London, he repaired to Edinburgh, where he established himself in practice. About 9 years after he was elected to the professorship of the institutions of medicine, on the resignation of Dr. Sinclair. This office, together with that of clinical lecturer, he continued to discharge with great reputation upwards of 20 years; during which he published his Essay on the Vital and other Involuntary Motions of Animals; his Essay on the Virtues of Lime-water and Soap in the cure of the Stone; his Physiological Essays, in 2 parts; of which the first part is in a great measure supplementary to his Essay on Vital Motions, consisting of a disquisition on the causes which promote the circulation of the fluids in the smaller vessels of the animal body; the 2d part consists of observations on sensibility and irritability, a subject to which the attention of many philosophical inquirers was at that time directed, by the writings of Haller, against whose doctrine Dr. Whytt proved a most formidable opponent. Another work published by Dr. W. during the period above mentioned was his Observations on Nervous, Hypochondriac, and Hysteric Disorders, partly theoretical and partly practical.

By these and other works Dr. W. attained a high degree of celebrity, not only as a physiologist, but also as a practical physician; and for his ingenious and useful inquiries in these and other departments of medical science, he had the honour of being chosen F.R.S.; of being appointed first physician to the king in Scotland, and of being elected president of the Edinburgh College of Physicians. Two years after his decease appeared his Treatise on the Dropsy of the Brain. A complete collection of his works was printed after his death (which happened in 1766) under the superintendance of his son and Sir J. Pringle.

Dr. Whytt was one of the greatest ornaments which the medical school of Edinburgh has to boast of. Haller, some of whose physiological opinions Dr. W. took so much pains to controvert, has with candour and liberality rarely equalled, said of him, magni certe ingenii vir fuit et perspicacis.

candid consideration of Lord Walpole's case, I shall trouble you with a few remarks which have occurred to me, in comparing it with the appearances found in his lordship's body after death, of which you were so obliging as to send me a particular account.

1. Whatever doubts may have been entertained concerning the cause of Lord Walpole's complaints, yet it now appears evidently beyond dispute that they must have been owing, not to a scorbutic corrosive humour in his bladder, as was imagined by some, but to stones lodged in it. These stones may possibly have lain there since 1734; for from that time to Spring 1747, his lordship was free of any gravelish complaints, only passing some red sand at times. But at what time soever they may have first arrived in the bladder, in 1747 and 1748 they seem to have acquired such a bulk, or were become so rough or pointed in their surface, as to occasion great pain, frequent provocations to urine, and sometimes bloody urine; especially after any considerable motion. These complaints however were soon relieved by swallowing daily an oz. of Alicant soap, and 3 English pints of lime-water made with calcined oyster-shells: and from 1748 to 1757 his lordship was kept almost entirely free from any return of them, except for some months of 1750 and 1751, during which he took only one-third part of the quantity of soap and lime water above-mentioned.

2. It is highly probable, nay I think altogether certain, that the soap and lime-water not only relieved Lord Walpole of the painful symptoms occasioned by the stones in his bladder, but also prevented their increase. If these stones came into the bladder in 1734, they must, in so many years as his lordship lived after this, have acquired a very great bulk: nay, if we suppose them not to have been lodged in the bladder above a year before they began to occasion frequent inclination to make urine, with pain, and sometimes sudden stoppages of urine; yet from 1746 to 1757, they ought to have grown to a much larger size than that of the kernel of a Spanish nut. It is true, the stone may increase faster in some patients, and slower in others; but stones, after remaining a dozen or more years in the bladder, generally weigh several ounces. Some years since I saw a stone weighing near 6 oz. taken from a boy of no more than 14 years of age.

3. Lord Walpole's case not only shows the power of soap and lime-water to relieve the painful symptoms, and prevent the increase of the stone in the bladder, but also makes it probable that these medicines do communicate to the urine a power of dissolving the stone. In the beginning of 1749, his lordship voided with his urine a calculous substance of a flat shape, about the size of a silver penny, covered with a soft white mucus; and on the surfaces of the stones found in his bladder there were some inequalities, which seem to have been made by the separation of thin lamellæ or scales. Further, the small stone

found in the beginning of the urethra must have been in a dissolving state, and considerably lessened in the bulk: for if it had lain long in the bladder, and never been larger, it ought to have been voided through the urethra with the urine; and it could not have arrived lately in the bladder, since Lord Walpole had not had for several years before his death, any nephritic pains, or symptoms of stones passing from the kidneys; and since it is not likely that a stone of the size and shape of the seed of an apple would pass through the ureters without being felt. Now if this small stone, found in the urethra, was partly dissolved by the virtue of the soap and lime-water; it will appear at least probable, that the 2 larger stones in the bladder were so likewise. But though Lord Walpole's calculous concretions had remained undiminished, and without any symptoms of dissolution; it would not therefore follow, that soap and lime-water cannot dissolve the stone in other patients, where the concretion may be of a less firm texture.

The Rev. Dr. Richard Newcome, then Lord Bishop of Llandaff, while drinking 2 English quarts of lime-water daily, for the cure of the stone in his bladder, poured his urine every morning and evening on a piece of human calculus weighing 31 grains; by which, in the space of 4 months, it was reduced to 3 pieces, weighing in all only 6 grains. On one of these pieces, weighing 2.31 grains, he caused to be daily poured, for 2 months, the fresh urine of a person who drank no lime-water; at the end of which time the piece of calculus was found to weigh 2.56 grains, having increased in weight a quarter of a grain. This same piece being afterwards steeped in the bishop's urine (who continued to drink lime-water as above), from June 24 to July 9, was in these few days quite crumbled into powder. Since this experiment shows, beyond dispute, that lime-water, unassisted by soap, can communicate to the urine a power of dissolving the stone out of the body, it can scarcely be doubted that it must have the like effect on it, when lodged in the bladder. And that the dissolution of the stone in the bladder has been completed by soap alone, appeared evidently in the case of the Rev. Mr. Matthew Simson, minister of Pancaitland near Edinburgh; an account of which will soon be made public* by Dr. Austin, who opened his body after death. Mr. Simson had, from 1730, been afflicted in a less or greater degree with the symptoms of a stone in the bladder: and in November 1735 was sounded by Dr. Drummond of Perth, and Mr. Balderston, surgeon in this city, by whom a stone was not only plainly felt, but also by the patient himself. In February 1737 he began to take soap; and after 1743 never had any gravelish symptoms. He died in May 1756; and when his bladder was looked into, there was neither stone nor gravel found in it.

* It is printed in this vol. of the Phil. Trans. p. 221 et seq.—Orig.

4. It appears from Lord Walpole's case, that soap and lime-water, even when taken in large quantities, proceed very slowly in dissolving the stone. From July 1748, to the beginning of 1757, his Lordship drank 3 English pints of lime-water, and swallowed for the most part an oz. of soap daily; except from April 1750 to June 1751, during which time he took only 1 pint of lime-water, and one-third part of an oz. of soap daily. However speedily soap and lime-water may dissolve the greatest part of urinary stones out of the body, yet being mixed with the aliment and humours of the stomach and guts, and afterwards with the whole mass of blood, it is impossible but their force must be greatly impaired before they arrive with the urine at the bladder. When therefore urinary stones are of an uncommon hard texture, we are perhaps scarcely to expect any sensible dissolution of them by the use of soap and lime-water: but when they are of a softer kind, there is no reason to doubt that these medicines will in time dissolve them; and this will happen sooner or later, in proportion to the hardness of the stone, to the quantity of the medicine swallowed by the patient, and the exact regimen he observes as to diet.

But however slowly soap and lime-water may proceed in dissolving the stone, yet they generally give speedy relief to the patient. Lord Walpole did not take these medicines in the full quantity till the end of July 1748; and in a few months after he was not only greatly relieved of all his complaints, but in December was able to ride 100 miles in his coach, without finding any uneasiness, though the last 2 days of the journey, the horses went at a full trot. In winter 1750, and spring 1751, when his lordship swallowed only one-third part of the soap and lime-water, which he had been in use to take, his pains and frequent inclination to make urine returned in a good degree: but after taking the medicines in the full quantity, he soon became as easy as before.

It would seem, while Lord Walpole used only one pint of lime-water and one-third of an oz. of soap daily, that the petrifying quality of his urine was not entirely destroyed, and that the stony particles newly formed on the surface of the calculi occasioned, by their roughness, the return of his painful symptoms. However, when he had recourse to the medicines in a larger quantity, the petrescent quality of his urine was not only destroyed, but this fluid seems to have acquired a power of dissolving the rough stony particles deposited on the surface of the calculi; and in this way soon removed the pain, bloody urine, and frequent desire to make water, on using any considerable exercise.

Soap and lime-water not only relieve the painful symptoms occasioned by the stone, by wearing off its sharp points and rougher parts, which used to irritate the tender membrane which lines the bladder; but when this membrane has been wounded or lacerated by the stone, there is nothing that will heal it more speedily

than lime-water: which the ingenious Dr. Langrish has found to be remarkable also for its effects in curing the bladders of dogs, after being fretted with soap-lees.

The power of soap and lime-water to alleviate the painful symptoms attending the stone is so great, that as far as I remember I have only met with one patient who did not find himself considerably relieved by them. But it is to be observed that this patient neither took them in full quantity, nor persisted in their use for a long enough time: and when he was afterwards cut, the stone taken out of his bladder was almost as thick set with sharp prickles as the back of a hedge-hog: so that in this case no remarkable ease could be procured to the patient by the medicines, until they had quite dissolved these sharp points, and rendered the surface of the stone smooth and equal; which was not to be done but after a very long time, especially as the stone was of a pretty hard texture.

It may be proper to take notice, that when along with the stone there is any ulceration in the bladder, soap does mischief, and lime-water often fails of giving any considerable relief. However, even in this case it is perhaps one of the best remedies we know.

5. Soap and lime-water, taken in large quantities, and persisted in for many years together, appear to be innocent, and no way injurious to health. Lord Walpole, who used these medicines for upwards of 8 years, was not only relieved of the painful symptoms of the stone, but had his health improved by them in other respects. His appetite, healthful look, and a degree of spirits uncommon at his age, continued till the end of 1756, when his last illness began first to attack him. And as his health did not appear to be any way injured by these medicines; so when his body was opened after death, his kidneys and ureters were observed to be quite sound and natural, as was likewise his bladder; only its coats appeared a little thicker than usual, owing probably to the long-continued friction of the stones upon it. Neither the kidneys, ureters, nor bladder, were loaded or crusted with any calcareous matter; an effect most unjustly ascribed to soap and lime-water, since in the urinary passages, to which the air has no access, they cannot deposit their calcareous part; and since the white stuff observable in the urine of such patients as take these medicines in large quantities, is only the usual sediment of the urine changed in its nature and colour, with perhaps some of the dissolved particles of the stone.

As the urinary passages were no way injured, so neither were the stomach, guts, and other viscera of the lower belly. These had all a healthful appearance, except the gall-bladder, which was almost full of biliary concretions: nor is it surprising that soap and lime-water, which prevent the growth of urinary calculi, should have no effect on biliary stones, since, though these medicines dissolve the former out of the body, yet they do not make the smallest impression on the latter.

It will perhaps be needless to take notice, that the lingering nervous fever, of which Lord Walpole died, cannot, with any colour of reason, be ascribed to the large use of soap and lime-water; since, if they could have produced such an effect, they must have done it in much less time than 8 years and a half.

It may not be amiss to observe, that though soap and lime-water taken in large quantities are no way injurious to health, yet in some cases they may become improper, on account of the particular state of the patient. Thus, in a scorbutic or putrid disposition of the humours, soap at least ought to be totally omitted; and such patients, as are much troubled with the hæmorrhoids, ought to be sparing in its use, as the alkaline salt with which it abounds will scarcely fail to exasperate their pain. Where the patient is naturally very costive, less lime-water and more soap ought to be used; and on the contrary, where the body is too loose, little or no soap is to be taken, but the cure is to be trusted to lime-water alone; which in this case ought to be drank to the quantity of 2 English quarts a day."

III. Dr. Pringle's Paper read after Dr. Whytt's Letter. p. 219.

Dr. Pringle begs leave to inform the Society, that having read the copy of his letter, within these few days, to Dr. Shaw, Mr. Hawkins, and Mr. Graham, those gentlemen found his account agreeable to their several observations; only Mr. Graham took notice that of late years Lord Walpole, in his journies to Norfolk, had twice voided some blood with his urine, but with little uneasiness; and that at other times he had passed some sand and stony particles, though never larger than the head of a small pin, attended with frettings of the parts, scarcely painful. But Mr. Graham was not sure whether these accidents were prior or subsequent to the sequel of the case, communicated to the Society by his lordship.

Dr. Pringle thinks it may be also proper to acquaint the Society with another circumstance in Lord Walpole's case, which he had both from Dr. Shaw and Mr. Graham, viz. that after using the soap and lime-water for some time, his lordship was freed from a very obstinate dry and scurfy eruption, which had resisted several other medicines. But as there were no marks of a putrid scurvy, that species expressly alluded to towards the end of Dr. Whytt's letter, the Society will easily understand how the lithontriptic medicines may be prejudicial to one troubled with the true putrid scurvy, such as is most incident to sailors, and yet not be improper for those that are subject to the scurfy eruptions, which are commonly, though erroneously, called scorbutic.

XXVII. *On the Virtues of Soap in Dissolving the Stone, in the Case of the Rev. Matthew Simpson. Communicated by John Pringle, M.D., F.R.S.* p. 221.

This paper gives an account of the relief obtained in the case of the Rev.

M. Simpson, of Pencaitland, who had been afflicted with symptoms of gravel and stone, by the daily use of soap, taken in the form of pills, which he first took in doses of a drachm a day, but afterwards increased to the quantity of 5 or 6 drachms in 24 hours; and then diminished the dose again to $\frac{1}{2}$ oz. per diem. In this manner he persisted in the use of the soap-pills for about 17 years, and at length died of a diarrhœa in 1756, in the 83d year of his age. His body was opened; but no stone or gravel was found in the bladder, which appeared to be in a natural state, except at the neck, where the coats seemed to be scirrhous, and were about $\frac{1}{4}$ of an inch thick. It is supposed the stone, which previously existed in the bladder, as had been ascertained by the catheter in 1735, was of a soft texture, and had been dissolved by the soap.

A Letter from Dr. Adam Drummond to Dr. Adam Austin, relating to the Rev. Matthew Simpson's Case. Communicated by J. Pringle, M. D., F. R. S. p. 226.

In this letter it is stated by Dr. D. that he was present when Mr. Balderstone sounded Mr. Simpson, and that both of them perceived, very distinctly, a large stone; that Mr. Simpson himself felt it; which they were the more solicitous he should do, as he had been sounded before by Dr. Simpson, who had declared there was no stone. But the particular magnitude of the stone they could not well determine at the end of a large catheter; though he remembered Mr. Balderstone, who was well versed in that business, conjectured it to be pretty large.

XXVIII. On the Impressions of Plants on the Slates of Coals. By Mr. Emanuel Mendes da Costa, F.R.S. p. 228.

The impressions of various kinds of plants are frequently, Mr. C. thinks always, found in some of the strata lying over coal; but more particularly in a stratum of earthy slat, which always lies immediately on the coal-stratum, not only in the coal-pits of this kingdom, but of many other parts of Europe, as France, Saxony, Bohemia, Silesia, &c. Most of these impressions are of the herbæ capillares et affines, the gramineous, and the reed tribes: but among them are many rare and beautiful impressions undoubtedly of vegetable origin, and impressed by plants hitherto unknown to botanists. Besides these found over coal-pits, there are likewise found in some parts of this kingdom, as at Robinhood's-bay in Yorkshire, Coalbrookdale in Shropshire, &c. many curious impressions of the fern tribe in regular nodules of iron-stone; and, in the latter place, not only impressions of plants, but even the cones or iuli of some kinds of trees are met with, very perfect and fair, and curiously imbedded in masses of iron-stone.

Most part of the impressions of ferns, grasses, &c. are easily recognizable, they so minutely tally to the plants they represent. Others indeed, though they do not exactly answer any known species, yet have characters so distinctly expressed, that they are easily arranged under their respective genera. Pl. 4, fig. 4, 5, 6, 7, 8, 9, 10 exhibits 7 of these impressions, out of many in his possession. Fig. 4 is from Mr. Mytton's collieries at Drilt, near Oswestry, in Shropshire; as are also those figured N°. 5, 7, and 10: they are found sometimes 2 feet in length, and are generally covered with a thin crust of coal. N° 5 seems of the red tribe: the knobs placed in rows, which are like the vesicles on the quercus. N° 6, from a coal-pit in Yorkshire; seems to be owing to something of the fir kind. N° 7 seems to be of the same kind as N° 5. The extraordinary impression is from Mostyn-colliery in Flintshire. It is a little obscured; but, when attentively viewed, exhibits a reticular impression, the meshes, which are rhomboidal hollows, and the sides of the rhombs, or the network are raised, or in relief. N° 9 is from Newcastle.

These impressions are not only met with in small pieces; but large evident branches, some feet in length, have been found. He had, in the collieries of Derbyshire, frequently traced branches with, seemingly, long narrow leaves proceeding from them, and parts of other vegetables, above a foot in length: but the hardness of the substance they are immersed in renders it impossible to get them out without breaking them to pieces: And these impressions, he thinks, are to be ascribed to the Mosaic or universal deluge.

XXIX. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1756, *pursuant to the Direction of Sir Hans Sloane, Baronet.* p. 236.

This is the 35th presentation of this kind, completing a collection, to the number of 1750 different plants.

XXX. Remarks on the Opinion of Henry Eeles, Esq. concerning the Ascent of Vapour, published in the Philosoph. Transact. Vol. xlix, p. 124. *By Erasmus Darwin,* M. D. Dated, Litchfield, March* 23, 1757. p. 240.

The probability, supporting the hypothesis of Mr. Eeles, rests on this: 'That

* Dr. Darwin, F. R. S. was born at Elston, near Newark in Nottinghamshire, December 12, 1731; and he died near Derby, April 18, 1802, consequently in the 71st year of his age. His father was a gentleman who possessed a good landed estate, and had a taste for literature and science. And it would appear that the son's talents were first called into action by a correspondence with his father. After his early education at Chesterfield school, he was entered at St. John's college Cambridge, where he took the degree of M. B. in 1755, defending in his thesis an opinion that the motion of the

'every particle of vapour is endued with a portion of electric fire; and there is 'no other sufficient cause assigned for their ascending.' Dr. D.'s design is therefore first to attempt to show that another theory, founded on principles better

heart and arteries is produced by the immediate stimulus of the blood. After quitting Cambridge, and having prepared himself for his intended profession of the practice of medicine, by an attendance on the lectures of Dr. Hunter, in London, and a course of Studies at Edinburgh; he first attempted to fix himself as a physician at Nottingham, but being disappointed of his hopes of practice in that place, he repaired to Litchfield in 1756, where he settled for many years in considerable practice. In 1757 he married Miss Howard, of Litchfield, who died in 1770, and by her had three sons, the eldest of whom died at Edinburgh in 1778, whilst he was prosecuting with the utmost assiduity his medical studies in that university; the second an attorney at Derby, who died about 2 years before his father; the third son a physician, in great reputation at Shrewsbury, who married the daughter of the ingenious Mr. Wedgewood, so celebrated for his improvements in the porcelain manufacture, and to whose politeness we have been indebted for some particulars concerning the late Dr. D. Soon after the decease of his wife, Dr. D. commenced his laborious work, the Zoonomia, (which however was not published till 1794) or the laws of organic life; in the first part of this work, the author treats of physiology and pathology; in the second, or the practice of physic. His observations on the animal economy, though not always accurate, are always ingenious; but his arrangement of diseases is founded on distinctions by no means obvious, and therefore not likely to be of that use which the author intended to those who study medicine as a science, or practise it as an art.

About 1780 he married the widow of colonel Pole. Soon after his marriage with this lady, he removed to Derby, where he resided till within a few months of his death. Besides the Zoonomia, before mentioned, Dr. D.'s other prose works are Phytologia, in 1800, containing in the 1st part, a multitude of curious experiments and observations in natural history, and organized matter; the second part treating on the economy of vegetation, and, the 3d on agriculture and horticulture. And a Treatise on Female Education, written for the use of the two Miss Parker's (his natural daughters) whom he had established as conductors of a ladies school at Ashborne. He had moreover a principal concern in the System of Vegetables and Families ef Plants, translated from the Latin of Linneus, and published by a society at Lichfield, in 4 vols. 8vo. His poetical works consist of his Botanic Garden, or the Loves of the Plants, in 2 vols. 4to. the 2d vol. published first in 1790, and the 1st vol. in 1791. And his Temple of Nature. So that there are three eminent points of view in which the literary character of Dr. D. more particularly presents itself: first, as a medical philosopher; secondly, as a philosophical agricultor; and thirdly, as a poet: in every one of which, if his merit was not of the first rank, he was at least very acute, ingenious, and plausible. Besides his three grand works, he was the author of numerous other small pieces, in the Philos. Trans. and elsewhere. Thus, next to medicine and poetry, mechanics, botany, and other branches of natural history engaged his attention; he not only pursued these studies with ardor himself, but zealously encouraged them in others. Soon after he settled at Derby, he instituted a philosophical society and library, both of which were in a flourishing condition at the time of his decease. Dr. D. was above the middle stature, gross and corpulent in his person; his features coarse, and his countenance heavy; if not quite void of animation, it certainly was by no means expressive. In his gait and dress he was rather clumsy and slovenly.

Besides the printed works already mentioned, Dr. D. left various MSS. now in the possession of his son Dr. Darwin, of Shrewsbury, consisting of essays written when young, some poetical compositions, letters, &c. but none of them (it would appear) sufficiently finished or interesting for publication.

known, will sufficiently explain the ascent of vapours; and then, that some kinds of vapours are not endued with more or less than their natural share of electric æther. The immense rarefaction of explosive bodies by heat, depends either on the escape of air before condensed in them, or on the expansion of the constituent parts of those bodies. Where air is emitted, it cannot be condensed again into the same bulk by cold; but the expansion of the heated parts of bodies, as soon as that heat is withdrawn, ceases to exist.

Nitre comes under the first of these classes: in detonation it emits great quantities of air, not afterwards condensible to the like space. This may be seen by firing a few grains of gunpowder in an unblown bladder, or in a vessel nearly full of water with its mouth inverted. The same is true of all the solid parts of animals and vegetables, when subjected to fire; as appears from the experiments of Dr. Hales. But of water the contrary is evident. In the steam-engine, a jet of cold water instantly condenses that immense rarefaction; which could not be, if it was constituted of escaped elastic air. And though this steam must be acknowledged to have some properties of air, such as ventilating a fire, or that a taper blown out by it is capable of being again lighted immediately, and that without a crackling noise, which occurs when touched with water, this does not in the least invalidate our opinion, though it has certainly conduced very much to propagate the former one: since from this way of reasoning, the whole must be air, and we should have no water at all in vapour.

From considering this power of expansion, which the constituent parts of some bodies acquire by heat; many things before utterly inexplicable, became easily understood. Such as, why when bismuth and zinc are fused together, and set to cool, the zinc, which is specifically heavier, is found above the bismuth: Why the buff covering of inflammatory blood, the scum of heated milk, the sedative salt of borax, which are all specifically heavier than the liquids in which they are formed, are still formed at their surface. How benzoin, sulphur, and even the ponderous body mercury, may be raised into vapour, again to be condensed unaltered? And lastly, how water, whose parts appear from the æolipile to be capable of immeasurable expansion, should by heat alone become specifically lighter than the common atmosphere, without having recourse to a shell inclosing air, or other assistant machinery; and when raised, to support them floating, perhaps many days in the atmosphere.

But before we proceed to this 2nd part of our task, it will be necessary previously to consider, first, how small a degree of heat is required to detach or raise the vapour of water from its parent fluid. In the coldest day, or even the coldest night of winter, when the weather is not frosty or very damp, wet linen or paper will become dry in the course of a few hours. A greater degree of heat must indeed cause a quicker evaporation. But were it not for the pressure of

the superincumbent fluid, greatly less than that of boiling water; would instantly disperse the whole so heated into vapour. Secondly, that in the opinion of Sir Isaac Newton, well illustrated by the late lamented Mr. Melvil, the sun-beams appear only to communicate heat to bodies by which they are refracted, reflected, or obstructed; whence by their impulse, a reaction or vibration is caused in the parts of such impacted bodies.

This is supported by the experiment of approaching some light body, or blowing smoke near the focus of the largest glasses; and from observing that these do not ascend, it is evident that the air is not so much as warmed by the passage of those beams through it, yet they would instantly calcine or vitrify every opaque body in nature. And from this we may collect, that transparent bodies are only heated at their surfaces, and that perhaps in proportion to their quantity of refraction; which will further give and receive illustration from those very curious experiments, of producing cold by the evaporation of liquors, published by Dr. Cullen, in the late volume of Essays Physical and Literary, at Edinburgh. In these experiments a spirit-thermometer was immersed in spirit of wine, and being suddenly retracted, was again exposed to the air; and as the spirit of wine adhering to the glass evaporated, the spirit contained within the thermometer was observed to subside. Now as the difference of the refraction of spirit of wine and glass is exceedingly minute, compared with the difference of refraction of spirit of wine and air; we may consider in the above experiment, the heat to be communicated to the thermometer only at its surface: but here the adherent fluid escapes as soon as heated; by which means the glass, and its contents, are deprived of that constant addition of heat, which other bodies perpetually enjoy either from the sun-beams immediately, or from the emanations of other contiguous warmer bodies; and must thence, in a few minutes, become colder than before.

The use to be made of this principle, is this: 'That the little spherules of vapour will thus, by refracting the solar rays, acquire a constant heat, though the surrounding atmosphere remain cold.' And as for the minuteness of their diameters, if they are allowed to be globules, they must do this to a very great degree, he apprehends none of these objections will take place, with which Mr. Eeles has so sensibly confuted the former received theories on this subject. If it be asked, how clouds are supported in the absence of the sun? it must be remembered, that large masses of vapour must for a considerable time retain much of the heat they have acquired in the day; at the same time reflecting how small a quantity of heat was necessary to raise them; and that doubtless even a less will be sufficient to support them, as from the diminished pressure of the atmosphere at a given height, a less power may be able to continue them in

their present state of rarefaction; and lastly, that clouds of particular shapes will be sustained or elevated by the motion they acquire from winds.

Mr. Eeles has asserted, that the greatest possible rarefaction of water is when it boils. And it might be said, with equal propriety, that the greatest rarefaction of solids, was when they began to melt: and this may indeed be verbally true, if we chuse to alter the names of bodies, when they undergo any alteration by fire: for solids take the name of fluids, when they are in fusion; and water the name of vapour, when it is greatly rarefied in the steam-engine. Whence we find this assertion seems to be founded on a confusion in terms, and the fact far from being existent in nature.

He says also, the sphere of electrical activity is said to be increased by heat. If by electrical activity is here meant an increase of its repulsive power (the thing, which seems to be wanted in Mr. Eeles's hypothesis), there is no experiment to show it. If it be meant, that it is capable of being attracted to a greater distance; probably it may, as the heat will rarefy the ambient air, and we know the electric æther is attracted at very great distances in vacuo; but this cannot properly be called an increased activity of electric fire.

We are afterwards told that electric fire will not mix with air: whence, in the succeeding section, it is argued, 'That as each particle of vapour, with its surrounding electric fluid, will occupy a greater space than the same weight of air, they will ascend.' In answer to this, it must be observed, that there are some bodies, whose parts are fine enough to penetrate the pores of other bodies, without increasing their bulk; or to pass through them, without apparently moving or disturbing them. A certain proportion of alcohol of wine mixed with water, and of copper and tin in fusion, are instances of the first of these; the existence and passage of light through air, and probably of electric fire, are instances of the second.

To illustrate this, the following experiment was instituted. A glass tube, open at one end, and with a bulb at the other, had its bulb, and half way from thence to the aperture of the tube coated on the inside with gilt paper. The tube was then inverted in a glass of oil of turpentine, which was placed on a cake of wax, and the tube kept in that perpendicular situation by a silk line from the ceiling of the room. The bulb was then warmed, so that, when it became cold, the turpentine rose about half way up the tube. A bent wire then being introduced through the oil into the air above, high electricity was given. The oil did not appear at all to subside: whence he concluded, that the electric atmosphere flowing round the wire and coating of the tube, above the oil, did not displace the air, but existed in its pores. Hence it is evident, that electric matter surrounding particles of vapour, must increase their specific gravity, and cannot any-ways be imagined to facilitate their ascent.

With regard to the experiments that are given to show all vapour to be electrized: in these Mr. Eeles seems to have been led into error, by not having observed that many bodies electrized will retain that electricity for some time, though in contact with conductors. The Leyden phial may be touched 3 or 4 times by a quick finger before the whole is discharged. Almost all light, dry, animal, or vegetable substances, such as feathers and cork, do this in a much greater degree: and in general, the more slow any bodies are to acquire electricity, the more avaricious they are to keep it. Part of the plume of a feather, hanging to a green line of silk about a foot long, which was suspended from the midst of a horizontal line of the same, about 4 yards in length, was electrized with a dry wine glass according to the method of Mr. Eeles; and after being touched 9 times with a finger, at the intervals of 2 seconds of time, still manifested signs of electricity, by being attracted at the 10th approach of it.

A cork ball, on the same line and circumstances after being electrized, was touched at the intervals of 10 seconds repeatedly, for 7 times before it was exhausted. The fumes of boiling water were conveyed on this ball after being electrized; and after a fumigation for 30 seconds, it showed signs of electricity, by being attracted to the approaching finger; and after 30 seconds more without any fumigation, it again obeyed the finger; and again, after 30 more, but at less and less distances. The same appearances occurred from the fumes of resin. Hence he apprehends that Mr. Eeles, having dipped the electrized down of the juncus bombycinus in vapour for perhaps half a minute (for no time is mentioned,) and finding it still retained its electric attraction, was not aware that this same would have happened, if he had by intervals touched it with his finger, or any other known conductor of electricity.

As Mr. Eeles had here objected, that there was no real opposition in the electric æther of glass, and that from wax; the common experiment to show this was many times repeated with constant success; viz. the cork ball suspended as above, after being electrized by the wine glass and repelled from it, was strongly attracted by a rubbed stick of sealing-wax; and vice versâ. In the same manner Dr. D. observed the electric æther from a black silk stocking (which was held horizontally extended by the top and foot, and being rubbed in the midst with an iron poker, was applied to the cork ball,) to be similar to that of glass, and opposite to that of wax. But the following experiment appears to put this matter out of all doubt, and to demonstrate that this difference is only a plus and minus of the same specific æther, and not different qualities of it, as Mr. Eeles would suppose. A stick of dry sealing-wax was rubbed on the side of a dry wine glass, and a cork ball, suspended as in the former experiments played for some time between them: but glass rubbed with glass, or wax with wax did not manifest any electric appearance. Whence it would appear, that in

rubbing glass and wax together, the glass accumulated on its surface the identical æther that the wax lost. And if this opposition of the electricity of glass and wax be established, it contributes to demonstrate the fallacy of Mr. Eeles's experiments.

But what alone would entirely destroy this electric hypothesis, is, that from the experiments of Mr. Franklin and others, the clouds are sometimes found to be electrized plus, sometimes minus, and sometimes manifest no signs of electricity at all. Whence to say an accumulation of electric æther supports these clouds, seems an assertion built on a very unstable foundation, whose whole superstructure may well enough be termed an air-built castle, the baseless fabric of a vision. Add to this, that Mr. Eeles tells us, that he has passed through clouds resting on the sides of mountains. Ought not those clouds to have immediately discharged their electricity, and fallen? And common experience may remind us, that any cold bodies will condense vapour, whatever be their electric properties. So mirrors, or the glass of windows in damp rooms, are most frequently found covered with dew; which, of all other bodies ought most to be exempted from collecting vapours supported by electricity, as they are the least capable to attract or draw off that æther.

From all which, well examined, Dr. D. is persuaded that though clouds may sometimes possess an accumulation of electricity, yet that this is only an accidental circumstance, and not a constant one; and thence can have no possible influence either in the elevation or support of them.

XXXI. Of a New-discovered Species of the Snipe or Tringa. By Mr. George Edwards, Librarian of the College of Physicians.* p. 255.

This specimen of a new-discovered species of the snipe or tringa kind, was lately shot at Sowerby-bridge in Yorkshire. This bird is like in shape to most others of the tringa or snipe kind. By way of distinction he names it the coot-footed tringa, as it differs from other birds of that genus no otherwise than in having its toes webbed in the same particular manner as the fulica, or our bald-coot. The bill is black, and channelled on both sides of the upper mandible; in which channels the nostrils are placed near the forehead: it is compressed somewhat like a duck's bill, and ridged along its upper part. The eyes are placed farther backward from the bill than in many other sorts of birds, in which the wisdom of Providence is remarkable: for birds of this genus commonly feeding in soft muddy ground on the banks of rivers or the sea, have occasion to thrust their bills deep into the shores, to extract worms and insects; and their eyes would be in danger, were they placed more forward. The fore part of the head, the neck,

* This bird is the *tringa lobata* of Linneus.

breast, belly, thighs, covert-feathers withinside the wings and under the tail, are white: the top and hinder part of the head is black. The lower part of the neck behind, and the back are of a blueish ash or slate-colour, with a mixture of blackish or dusky: the upper sides of the wings and tail are of a blackish or dusky colour: the tips of the covert of the wings are white; the tips of the middlemost or shortest of the quills are also white, and form white transverse bars across the wings. Two or three of the middle quills are wholly white, and all of them have their inner webs white toward their bottoms. It has 12 feathers in the tail; the outermost of which, on each side, is edged with white. The covert-feathers on the rump, or upper side of the tail, are dusky and white. The legs are bare of feathers above the knees (as they are in most birds that wade in shallow waters,) and of an ash colour.

XXXII. Description of Several Small Marine Animals, by Job Baster, M.D. F. R. S. p. 258.

Dr. Baster's arguments, relative to the supposed vegetable nature of corallines, being recapitulated in Mr. Ellis's answer, it is unnecessary to preserve that part of the present paper. We shall therefore translate only the part in which he describes various small marine animals; some allied to the polype tribe, and others of different families.

If the sea water round our coasts be moved by night, either by throwing a stone into it, or by a stick, it exhibits innumerable fiery sparks, which are no other than minute shining animalcules, requiring a good microscope to show them distinctly. In order to collect these animalcules in sufficient plenty, the way is to take a quantity of sea-water in which they abound, and to strain it through a filtering paper, till only the quantity of about half an ounce, or less, remains on the paper: of this water a small drop, placed in a concave glass, and viewed by a microscope of considerable power, will exhibit them swimming very briskly about. Dr. B. observed three species, which are represented from the life at plate 5. fig. 22.

But the sea nourishes other animals in which this lucid quality exists, and of which some that were found in corallines are shown, fig. 25, 26, 28, 29; but these he does not particularize, since several authors have written concerning them.

If a large plant of coralline fresh taken from the sea, be placed in a China-dish, the bottom of which is of a deep blue colour, in a sufficient quantity of very clear and filtered sea-water, and its branches be carefully expanded, and viewed by a magnifier, one may often see a wood, as it were, in which a great many animals live, exclusive of various kinds of polypes infixed in the branches and extending their arms: there are also still several others, especially in the

lower parts of the corallines, have grown on oysters, which move about here and there, and perhaps serve by way of food for the oyster when it opens. Thus on the 23d of October, 1756, I took an oyster on which grew a large plant of coralline (fig. 15), in which, besides three different species of polypes, I found six different kinds of insects. The first was a worm, (fig. 22.) the head of which was furnished with six larger and two smaller horns. Another was a very small long-legged spider, much resembling the kind called by the French *le fancheur*, and of very slow motion. (fig. 30.) A third was a worm, as at fig. 27, which I lost while engaged in drawing it. A fourth, fifth, and sixth (fig. 32.) were not visible without a powerful microscope. Of these the animal at letter c is of a wonderful structure.

Examining in this manner different oysters and corallines, I saw several more such wonderful insects, the delineations of which are given at figures 26, 27, 28, 29, 31. On the 16th of the same month of October, several very small corallines were brought me, which had been taken from the surface of a buoy: in these, though often examined with great attention, I could not discover any polypes, but two other very wonderful animals.

Of these, which the letter A, fig. 23 exhibits, there were thousands, either creeping or swimming with an extreme brisk motion: they held or adhered to the small branches of the coralline with their 6 hinder feet, in the manner of the caterpillars called geometræ, sending themselves up and down in a wonderful manner, leaping with great activity from branch to branch. Among these were some few of a larger size, which are represented both in their natural size, and magnified. (see fig. 23, b and c.) Another animal, fig. 24, was not less surprizing; but an idea of all these animals will be better obtained by an inspection of the figure, than from a prolix verbal description. "But if, says Dr. B. "I should "attempt to delineate all the marine animals which I have discovered in dif- "ferent corallines, I should undertake a work of infinite labour; for their num- "ber and diversity are such as to exceed my powers."

"These observations however may, I hope, prove sufficient to demonstrate "that corallines are not the work or fabric of polypes, but that they merely "serve as a habitation and refuge, as well as food, to these and a great many "other small marine animals."

Explanation of the plate v.—Fig. 1. shows a plant of coralline called *corallina muscosa, sive muscus marinus tenui capillo spermophoros.*—Fig. 2. *Corallina ramulis dichotomis teneris capillaribus rubentibus.*—Fig. 3. A young plant of the *corallina tubularia laryngi similis.*—Fig. 4. Two species, a, b, (same as fig. 1 and 2), and c. *eschara papyracea utrinque cellulifera,* growing to one base, as often happens on buoys.—Fig. 5. A small branch of red coralline, which I kept for some weeks in sea-water, often renewed, during which time the small branches a, a, grew very considerably, and others, b, b, sprouted forth.—Fig. 6. Part of an oyster-shell, in which, besides certain green fila-

ments, two polypes, a, a, may be seen.—Fig. 7. Cancer arachnoides, on which were seated two species of polypes. A single or simple one at a, and many, inhabiting cells, at b.—Fig. 8. An animal called *narsgat*, which adheres to the wood of floodgates and ships: on this grew a small plant of coralline, in which I could not perceive any polypes; but several, b, b, were seated on the animal itself.

N. B. The delineator has represented the tails of these, and the polypes of the preceding figure too long, in order that they might come the better into view.

Fig. 9. A branch of red coralline in its natural size.—Fig. 10. The same viewed by the microscope, and in which three species of polypes may be seen.—a, b, Two different species, fixed by the tail or hinder part of the body to the coralline.—c. A third species, inhabiting the cells.—d. A dead polype.—e. The cells of the polypes.—Fig. 11. A plant of the *corallina tubularia laryngi similis*, in its natural size.—Fig. 12. A very large branch of this coralline viewed by the microscope, in which are discovered 5 different polypes.—a. The first and largest species, which I name the scarlet polype, and which is shewn at fig. 17, still more strongly magnified.—b. The same sort, but a smaller polype.—c. The third, which is the same as that at fig. 10, letter b.—d. The 4th, which is the same as letter c, fig. 10.—e. The 5th and least kind, and which is represented again at fig. 16, as well as very highly magnified.—f. The cells which the fourth species inhabits.—Fig. 13. *Corallina erecta pennata, denticulis alternis cauli appressis:* in this there were no polypes, unless in the cellules affixed here and there about the trunk.—b. The shells which are shewn magnified at B, fig. 14.—c. *Eschara millepora minima crustacea cellulis tubiformibus*, serving as a lodgement to animals, and magnified at c.—Fig. 14. *Corallina abietis forma:* this I received in December; its branches were beset with vesicles or eggs, a, a, disposed by pairs in regular order.—A. One of these vesicles or eggs shown by the microscope, b, shells, and c, *eschara minima*, as in the former figure, where they are represented magnified at B, C, d, d. Two brown corpuscles, which being viewed by the microscope, represent a nidus of worms, D.—Fig. 15. *Corallina pennata et siliquata*, taken from an oyster-shell: in this, besides 3 species of polypes, a, A, b, B. (the same as at fig. 10,) c, c, six other insects were observable, which are represented in fig. 25, 30, 32.—Fig. 16. A kind of very small branched polypes, allied to the *polypes a bouquet* of fresh water.—A. A polype of this kind adhering to a green marine conferva, and hardly visible to the naked eye.—B. The same viewed by a common magnifier, and at C, by a microscope of considerable power.—Fig. 17. The scarlet polype, represented both in its natural size, and magnified at fig. 11 and 12, and here shewn very highly magnified.—A. This polype, with its arms expanded, waiting for its prey.—B. The same animal, in the act of contracting its arms when catching its prey.—a. The larger inferior arms, 16, 18, or 20 in number.—b. The upper shorter arms, 12, 14, or 16 in number.—c. The upper, pear-shaped part of the body, affixed to the lower.—d. The lower compressed part of the body.—e. The place where the polype adheres to the coralline.—C. The same polype seen in front, with the body contracted into a globular form, which is more visible in the larger kind, figured at 19, 20, 21.—Fig. 18. A similar scarlet polype, larger than the rest, from the body of which (where the parts c and d are joined) 8 small branches grew, which on their tips had two or three globules with a red spot on the middle of each, and which I in vain hoped would have grown into young polypes.—a. The longer inferior arms of this polype.—b. The upper shorter arms.—c. This seems to be the mouth, in the middle of the pear-shaped body of the polype.—Fig. 19. A larger kind, as it should seem, of polypes, called *klapkonten*, seated on oyster-shells, and which, when irritated, withdraws its arms entirely into its body.—Fig. 20. The same polype, with its body extended, spreading its arms.—Fig. 21. The same contracting itself, having caught its prey.—22. Three species of luminous animalcules represented highly magnified in a small drop of sea-water.—Fig. 23. A wonderful animal found on corallines taken from buoys.—A. Of this kind were hundreds.—B. Ten or twelve were of this size.—C. The same animal seen through a microscope.—a. antennæ.—b. the first pair of arms or legs.—c. The second pair.—

d. The third or largest pair,—e, e, e, e. Four egg-shaped bodies, which the animal moved when swiming.—f, f, f, f, f, f. The six hinder legs or feet, with which it held a branch of the coralline, and was thus enabled to bend itself about in all directions.—g. The tail, at the end of which is the vent.—h. The eyes.—Fig. 24. Another animal found on the same corallines.—A. The animal in a prone situation.—B. In a supine situation.—C. Considerably magnified.—Fig. 25, 26, 27, 28, 29, 30, 31, 32 exhibit certain luminous and other animalcules found in different corallines magnified.—The animalcule c, fig. 32, was of a highly singular appearance, and had a great many limbs.

XXXIII. Remarks on Dr. Job Baster's Observationes de Corallinis, &c. in the foregoing Article. By Mr. John Ellis, F. R. S. p. 280.

Dr. Baster endeavours to prove, that corallines are not of an animal, but a vegetable nature; and has brought many arguments to support his system; which to gentlemen not well acquainted with the subject may appear plausible. It is to be wished that the Doctor had read and examined thoroughly what has been lately written on the subject.

His first argument is, that because he does not find as many polypes in the corallines adhering to ships, flood-gates, and buoys, as in deep water on oysters, muscles, and rocks, therefore he concludes that corallines are not formed by polypes. In answer to this, let us examine the pliable structure of these bodies, and how wisely nature has defended such tender substances with a tough thin membranaceous covering, and we shall find that the sea is calm enough often near the surface to give them leave to grow, even in the strongest currents: though doubtless they are more liable to be destroyed in such agitated situations than in the calm depths of the sea.

His 2d argument is, that finding polypes are not equally dispersed over the whole plant, how can they form it? and gives an example, pl. 5, fig. 13, of a coralline that is incrusted with many other corallines or polypes on the stem, but has none on the branches. Here we plainly see the mistake: the Doctor looks for the tender part of the polype on the surface of the coralline, considering it as a plant; and indeed, if this was the case, he ought so to do; but he never once takes notice of the internal hollow structure of the stem, branches, and denticles of those bodies, to inform us whether he found an animal in those parts or not. This material point he seems not to have thought on; which is really the true point in controversy at present among gentlemen who have not examined these bodies recent in sea-water.

His 3d argument is, that almost always one and the same coralline plant cherishes polypes of different kinds; and refers us to fig. 10 and 12. In fig. 10 he gives an elegant painting of a geniculated red conferva for a coralline, surrounded, as is very common, by many species of small corallines and escharas. And in fig. 12 he gives a drawing of one of the tubular corallines, with the head

of the animal at the top of it; the stem of this is incrusted with 4 different corallines and escharas, like the conferva fig. 10; and then he asks, which of these 5 polypes made the tubular coralline? To give him some proof of the animal nature of this coralline, let him consult Ray's Synopsis, ed. 3, p. 34, n. 4, and there he will find one of this species, called adianti aurei minimi facie planta marina, noticed so long ago as the year 1713, by Dr. Lloyd, as a zoophyte, from its stem or tube's being full of a thick reddish liquor, rather resembling blood than the juice of a plant; which on pressing the stem communicated with the little head at top.

His 4th argument is, that as upon one and the same coralline plant you shall find different kinds of polypes; so in different species of coralline the same polypes: and to confirm this, he quotes my Essay on Corallines; where it is remarked, that the polypes in the denticles of the setaceous or bristly coralline, N° 16, appear to be like those that are on the lobster's-horn coralline, N° 19. And to illustrate this, he observes that bees and wasps always build their cells invariably the same; and that therefore these 2 corallines should be the same.

But he also takes this matter wrong: he has considered, in all his observations, the heads of those parts of the polype in which are the mouths, arms, or tentacula, which appear coming out of the cups, denticles, and at the ends of the tubes of the corallines, as so many whole and entire animals, without ever observing that the body of the animal is contained in the tubular part of the root, stem, and branches; and that these differ from each other widely both in size and shape, as he may plainly see in the 2 corallines he has instanced. Further, his comparison to bees and wasps, and their cells, is not conclusive: for these ramified, hollow, and denticulated bodies, called corallines, which we so frequently find dead on our shores, are properly skins of certain marine polypes, and not nests, as those constructed by these little winged animals are. And yet we find as great a regularity in the same species of these corallines, as when we compare 2 oak-trees to one another, or 2 of Mr. Trembley's branched freshwater polypes to one another.

His 5th argument is, that if corallines were formed by polypes, neither the polypes, nor even their cells, would ever fix on living animals, or any other bodies. Here we may observe, that the consequence he draws does not follow: for corallines may be formed or produced by certain species of polypes, and yet polypes of another species may be found adhering to other bodies, and even to animal bodies.

By his 6th argument he endeavours to prove, that the vesicles which are found in regular rows on the sea-fir coralline in winter, fig. 14, do not belong to it; and are no more than the eggs of some sea-insect deposited on it, of which there may be a great variety. But to convince him of his mistake, let him take off

one of the vesicles, and apply a large magnifier to the place, and he will discover a hole, by which this vesicle or ovary has had a communication through the skin with the parent polype. For a further illustration of the manner in which these vesiculated polypes breed, let him consult the 38th plate of my Essay, where he will find several accurate figures of these vesicles, with the spawn of the polypes coming out of them; some of which spawn we evidently discovered to be young polypes with their arms formed; and as they fell from the vesicle, extending themselves in the watch-glass of sea-water.

In examining the drawings for his plates, it appears that fig. 2 is evidently a red conferva, which he calls a coralline. We have no corallines, but many confervas of this form and bright red colour on our coasts; and these shores are allowed to have similar marine productions with those of Holland. Fig. 5 he calls a branch of red coralline, which he says he kept several weeks in sea-water, and that often changed; during which time it sprouted and grew very much. This experiment is probably very true; because it is plainly a vegetable, as appears from his own exact drawing of it; and seems to be the fucus teres rubens minus in longum protensus of Ray's Synopsis, ed. 3, p. 51, n. 53. This is one of his principal arguments to prove the vegetation of corallines. Fig. 9 he calls a branch of red coralline; and at fig. 10 he has it magnified, where it appears to be a geniculated red conferva, drawn and painted with great exactness.

These arguments, and these figures of real vegetables, which the Doctor has given us for corallines, show how much he is willing to support the old opinion of the botanists: but he will soon alter his opinion, when he observes the remarkable difference of the texture of vegetable and coralline bodies, when viewed in sea-water through a good aquatic microscope. And to convince him more fully that corallines are an animal substance, let him burn them, and he will perceive the same pungent volatile alkaline smell, which he finds in burning horn, hair, or oysters; whereas burnt fucuses and confervas yield a smell not much unlike that of common land vegetables. Even the stony corallines, when their cretaceous covering has been dissolved in vinegar, the membranous part that remains of them, put into the fire, yields the same animal smell with other corallines.

XXXIV. Of an Extraordinary Operation performed in the Dock-yard at Portsmouth. By Mr. John Robertson, F. R. S. p. 288.

This account of the operation goes to show the effect of wedges in raising ships, or any great weights. In the present instance Mr. R., by an ingenious calculation, finds how much of the ship's weight was lifted by each screw and wedge: in particular he discovers that each man employed in the operation, with his mallet and wedge, must have lifted at each stroke about 2 tons weight of

the ship; which, he observes, is considerably less than he had seen raised by way of experiment. Besides, it may be observed, that the pressure is at least doubled by the friction.

XXXV. On an Evening, or rather Nocturnal, Solar Iris. By Mr. George Edwards, Librarian of the Coll. of Phys. p. 293.

On Sunday evening, June 5, 1757, walking in the fields near Islington, about half a mile north of the upper reservoir or basin of the New River, Mr. E. observed the sun to sink beneath the visible horizon to the north-west, it being very clear in that quarter, except some thin clouds a little above the horizon, which were painted of fine red and golden colours, as is usual when the sun sets in a calm clear evening. But about 20 minutes after sun-set, it being then darkish, he saw an iris in the dusky air, at a height greater than is seen at any time in the rainbow. It was in the contrary quarter of the heavens to the setting sun, and fell on the smoke, mists, and evening vapours arising from the city of London and its neighbourhood. The arch seemed to be a full half circle, though its lower parts fell some degrees short of the horizon. It was very distinctly seen for about 15 minutes; its colours the same as in the rainbow, but fainter. The lower ends of the bow arose gradually higher from the earth, as the sun declined beneath the horizon, till the whole arch disappeared. The centre of the arch was above the horizon at its first appearance. He could not believe that this arch proceeded from the sun-beams falling on rain: for there had been none that afternoon; nor was there any sort of signs of rain or rainy clouds to be seen: the wind being northerly, and the air cool, but somewhat hazy in the quarter where the bow appeared. It was not near so bright as the rainbow appears to be in the day time; and he believes that it would not have been visible at all in the presence of the sun. He imagines it was formed on the gross particles of the evening vapours, mixed with those of the smoke arising from the town. This could not be a lunar arch, the moon being then many degrees below the horizon, and the arch in a place, where it could not be affected by the moon's rays.

XXXVI. The Effects of the Opuntia, or Prickly Pear, and of the Indigo Plant, in Colouring the Juices of Living Animals. Communicated by H. Baker, F. R. S. p. 296.

Mr. Baker received a letter from Doctor Alexander Garden, of Charlestown in South Carolina, part of which he laid before the R. S., containing this account.

The Doctor tried the effects of the prickly pear in colouring the urine. He gathered some of the fruit, and gave 4 of the pears to a child of 3 years of age,

and 6 pears to one of 5. The next morning the urine of both appeared of a very lively red colour, as if tent wine had been mixed with clear water. The urine of the eldest was deeper coloured, and of a darker look: the youngest (who always naturally made clear urine) was of a more lively and beautiful red. Next day he gave 6 pears to a negro wench, who gave suck, and strictly forbad her suckling her child for 6 or 8 hours; and then taking some of her milk in a tea-cup, and setting it by for some hours, the cream had a reddish lustre, though it was very faint. He was led to this last experiment by an observation, which he made on the milk of cows, which had fed in an indigo-field: the indigo had not only tinged their urine blue, but the cream of the milk was of a most beautiful blue colour, and had a radiated appearance from the centre (Is it not hence probable, that the dye is the oily part of the plant?). The milk underneath was clear and white as usual.

XXXVII. Of an Extraordinary Shower of Black Dust that fell in the Island of Zetland, Oct. 20, 1755. By Sir Andrew Mitchell, of Westshore, Bart.* p. 297.

From Orkney Sir A. was informed, that about the time of the earthquake at Lisbon, Nov. 1, 1755, the tides were observed to be much higher than ordinary. He received from Zetland a letter, dated 28th May, 1756, from Mr. William Brown, master of the grammar-school at Scalloway in that country, a sensible and observing man; stating, on October 20, 1756, between the hours of 3 and 4 in the afternoon, the sky being very hazy, as it uses to be before a storm of thunder and lightning, there fell a black dust over all the country. It was very much like lamp-black, but smelt strongly of sulphur. People in the fields had their faces, hands, and linen, blackened by it. It was followed by rain. Some people assign the cause of it to some extraordinary eruption of Hecla. And the same was confirmed afterwards by several other persons in Zetland. At the time, the wind was from the S.W. which does not seem to favour the opinion that the dust proceeded from an eruption of mount Hecla, which lies about N.W. from Zetland; unless it may be supposed, that a north wind happening just before had carried this dust to the southward, and the south-west wind immediately following had brought it back to the northward.

The distance from mount Hecla to Zetland is between 500 and 600 miles.

XXXVIII. Of some Thermometers for Particular Uses. By the Right Hon. the Lord Charles Cavendish, V. P. R. S. p. 300.

The thermometer, pl. 7, fig. 1, is designed for showing the greatest degree of heat, which happens in any place during the absence of the observer. It con-

* See vol. 10, p. 687, of these Abridgements.

sists of a cylinder of glass joined to a tube, and differs from common thermometers only in having the top of the stem drawn out into a capillary tube, which enters into a glass ball c, joined on to the stem at the place where it begins to be contracted. The cylinder, and part of the tube, are filled with mercury; the top of which shows the common degrees of heat as usual. The upper part of the tube above the mercury is filled with spirit of wine, and some of the same liquor is left in the ball c, so as to fill it almost up to the top of the capillary tube. Now when the thermometer rises, the spirit of wine will be driven out of the tube, and will fall into the ball c. When the thermometer sinks again, as the spirit cannot return back from the ball, the top of the tube will remain empty, and the length of the empty part will be proportional to the fall of the thermometer. Therefore, by means of a proper scale, the top of the spirit of wine will show how many degrees it has been higher than when observed; which being added to the present height, will give the greatest degree of heat it has been at.

To fit this thermometer for a new observation, it is necessary to fill the upper part of the tube with spirits; which may be done by inclining the instrument till the spirits in the ball c cover the end of the capillary tube. For if the cylinder is then heated, by applying the hand to it, or by the flame of a lamp held at some distance, till the spirits rise to the top of the tube and run over into the ball c, and is then suffered to cool in the same position, the tube will remain full of spirits, and the thermometer will be fitted for a new experiment.

The top of the capillary tube is made to stand pretty near to one side of the ball, and also to the top of it, that a less inclination of the instrument may be sufficient to make the spirit of wine in the ball cover the end of the tube. The ball c is joined on as high as possible, so as to hide no part of the tube, except that where the bore is contracted. By this means the top of the spirit of wine begins to appear before the thermometer has sunk one degree. It is convenient to leave some mercury in the ball c, which may be made to cover the end of the capillary tube, by inclining the thermometer more than what is necessary to make the spirit of wine cover it. By this means some mercury may be got back into the tube, in case any of it should happen to be driven into the ball by the thermometer's being exposed to too great a heat.

The scale of degrees at top, which shows the descent of the thermometer from the highest point it has arrived at, ought not in strictness to be the same at all times of the year; for those degrees exceed the common degrees of heat pointed out by the top of the mercury, as much as the column of spirit of wine expands, and therefore are greatest when that column is so; that is, when the greatest heat to which the instrument has been exposed is least. A difference of 30 degrees of Fahrenheit's scale, in the greatest rise of the thermometer,

would require the scale to be altered one 60th part: and the error arising from making use of the same scale will be about one 6th of a degree, if the thermometer is observed when it has fallen 10 degrees.

If the weight of the mercury be thought inconvenient, it may be avoided by the construction described in fig. 2, where the bottom of the tube is bent so as to point upwards, and is joined to a ball A, which communicates with a cylinder placed above it. In all other respects it is the same as the instrument before described. It is filled with spirit of wine and mercury; the quantity of the latter being sufficient to fill the whole tube and ball A. No part of the spirit in the cylinder can get into the tube as long as the instrument is kept in an erect position, or even if it be carefully laid down flat on a table. For though in this last case some of the spirits may get into the ball A, it will rise to that part of the ball which is then uppermost, and will not touch the orifice of the tube n; which was the reason for adding this ball, which would be unnecessary if the instrument was kept constantly erect, or nearly so. If the spirit should come to touch the orifice of the tube n, it would work up between the mercury and the glass; which would put the instrument out of order.

The thermometer fig. 3 is designed for showing the greatest cold which happens in any place during the time the instrument is left in it. The tube is bent into the shape of a syphon of unequal legs standing parallel to one another, the bend being at the bottom. The top of the shorter leg is bent to a right angle, and immediately opens into a ball A, which, by means of a short bent tube on the opposite side, communicates with a cylinder standing parallel to the legs of the syphon, and pointing downwards. This cylinder contains the greatest part of the fluid; and is added only to make the thermometer more sensible than it would be, if the ball A was made of a sufficient size to contain the proper quantity of fluid. This instrument is filled with spirit of wine, with the addition of as much mercury as is sufficient to fill both legs of the syphon, and about a 4th or 5th part of the ball A. The common degrees of heat are shown by the top of the mercury in the longest leg, or by the top of the spirit, in case any of it is left above the mercury. When the mercury in the longest leg sinks by cold, that in the shorter leg will rise, and will run over into the ball A: from whence it cannot return back when the thermometer rises again, as the surface of the mercury in the ball is below the orifice of the tube n. Therefore the upper part of the shorter leg will be filled with a column of spirits of a length proportional to the increase of heat; the bottom of which, by means of a proper scale, will show how much the thermometer has been lower than it then is; which being substracted from the present height, will give the lowest point that it has been at.

If no further contrivance was used, the mercury would fall into the ball A in

large drops; which would make the instrument less accurate. For the thermometer's beginning to rise immediately after a drop is fallen, or just as it is going to fall (in which case it will return back into the tube,) will make a difference of such part of a degree nearly as that drop answers to. To prevent this inconvenience, the top of the shorter leg close to the ball is contracted, by being held in the flame of a lamp; and the passage is further straitened by a solid thread of glass placed within the tube, and extending from the bottom of the shorter leg to the part near the ball A, where it is most contracted. By this means, as soon as any small portion of mercury is got beyond the end of the thread of glass, it breaks off and falls into the ball in very small drops. This thread of glass is fastened by the heat given to the tube in making the bend next to the ball. In order to fill the shorter leg with mercury, to fit the instrument for a new experiment, it must be inclined till the mercury in the ball covers the orifice of the tube n. The cylinder being then heated, the mercury will be forced into the shorter leg, and will run down the thread of glass in drops, which will soon unite. By this means such a quantity of mercury must be got into the shorter leg, as on the cooling of the instrument, will be sufficient to drive all the spirit of wine into the ball with a less degree of cold than what the thermometer is likely to be exposed to. The scale of degrees on the shorter leg will, in different seasons, be liable to an error of the same kind as that which was explained in the first-mentioned thermometer; but in this it will be less considerable, as the space between the two scales is filled with mercury, whose expansion is about 6 times less than that of spirit of wine.

The thermometer for finding the greatest cold, if applied to the purpose of immerging it deep in the sea to find its temperature, must be left open at top, for fear of breaking by the pressure. There is another inconvenience to be avoided; which is, that the mercury in the ball A, by the tossing of the instrument, might sometimes get into the shorter leg of the syphon, which would spoil the experiment. To prevent such an accident, the most convenient construction which occurred, was that of fig. 4, which differs from fig. 3, in having the ball A omitted; so that the mercury running out of the shorter leg will fall to the bottom of the cylinder, and will not be so liable to get back into the tube by motion. The cylinder is made to stand not quite parallel to the legs of the syphon, that the contained mercury may more easily be brought to touch the end of the tube, in order to fit the instrument for a new experiment. If by means of a bladder the sea-water can be kept out of the glass, this instrument may be made to show the common degrees of heat. If thermometers of this kind were to be sent up into the air by means of a kite, they might be made like those proposed for the sea; but it would not be necessary to leave them open.

XXXIX. *On a Double Female Monster which was born at Szony in Hungary, Oct. 26th 1701, and which died Feb. 23d 1723, at Presburgh, in the Convent of the Nuns of St. Ursula, and was buried there. By Justus Johannes Torkos, M. D., F. R. S.* p. 311. *From the Latin.*

1. Dr. T. premises his account of this monstrosity by observing, that it affords a remarkable instance of the power of the mother's imagination on the fœtus in utero. For at the beginning of her pregnancy, the mother 'attentius contemplabatur canes coëuntes, arctius cohærentes, et capitibus erga se invicem quodammodo conversos, eosque sibi crebrius præfigurabat.'

2. At the time of parturition, the body of Helen was first excluded as far as the navel; three hours after the feet were delivered, together with the body of Judith, joined to that of Helen. Of the two, Helen was the tallest and straightest. Notwithstanding they were joined together at the back below the loins, yet they were turned with their faces and bodies half sideways (semilateraliter) towards each other, so that they could sit down, and move backwards and forwards. One anus, situated between the right femur of Helen and the left femur of Judith, was common to both. 'Unam quoque habebant vulvam, intra 4 pedes reconditam, ut dum erectis starent corporibus, ne vestigium ejus conspicuum esset.' It was observed that when one of them wanted to go to stool, the other felt a similar desire; but that in regard to the urinary evacuation, when one of them felt an inclination to make water the other did not.* Hence in their youth, though in other respects they were exceedingly fond of each other, there frequently arose violent contentions between them, in which one would carry the other on her back, or drag her along to the place to which she wanted to go.

3. In the 6th year of her age, Judith was affected with a palsy of the left side, the consequence of which was, that, although she recovered, she ever afterwards remained weaker, more sluggish and dull; on the contrary Helen became more active, more lively, and more beautiful.

4. Not only was there this difference in their persons, but a difference was also observable in regard to their vital, animal, and natural functions, in health as well as in disease. And although they had the small pox and measles at one and the same time; yet they had other disorders separately. Judith was often convulsed, while Helen remained free from indisposition. Helen had a pleuritic affection. Judith had a fever. One of them had a catarrh and a colic, while the other continued well.

5. At the age of 16 the menstrua appeared and continued, but not at equal

* Another account, noticed further on, states that the alvine, as well as the urinary evacuation, was performed by each at different times.

times or in the same manner, or in the same quantity. Sometimes one, sometimes the other, would be most disordered at such periods; but Judith was more frequently convulsed, and was subject to various hysterical and pectoral affections.

6. On the 8th of Feb. 1723, in the 22d year of their age, Judith was seized with violent convulsions, succeeded by coma, which terminated fatally on the morning of the 23d of February. During this time Helen was affected with fever, accompanied with frequent faintings, by which she was so much debilitated, that although she was still sensible and could speak, she fell into an agony three minutes before Judith; after a short struggle, they both expired almost at the same instant.

7. On opening their bodies, each was found to be provided with distinct viscera; which were all in a healthy condition in Helen; but the heart of Judith was preternaturally enlarged and inclosed in a very strong pericardium; the right lobe of the lungs was in a putrid state. The descending aorta and descending vena cava, before they send off the iliac arteries and iliac veins, were found coalesced together. All the viscera in the abdomen were in a sound state. Each of the 2 bodies had its own liver, its own spleen, its own pancreas, its own kidneys, its own bladder, its own uterus, with the ovaria and Fallopian tubes, and separate portions of vagina, terminating in one common vagina. 'Partes genitalium externorum, præter commune orificium vaginæ, cuilibet erant propriæ velut clitoris, nymphæ, orificium urethræ; alæ seu labia utrinque ad perinæum concurrentia fossulam navicularem densiorem constituerant.' The stomach and intestines were situated in the natural manner, in both; but the 2 intestina recta were united into one at the os sacrum, so as to form a sufficiently large and common canal. The os sacrum was concreted together at the 2d division, and forming one body, terminated in one os sacrum and one os coccygis, common to both.

Dated *Presburgh, July* 3*d*, 1757.

The interval between the reading of this paper before the R. S. and its publication, was occasioned by the long indisposition, and afterwards death, of their President Martin Folkes, Esq. who having taken it to his house with a view of collecting and adding to it some further particulars, it could not be found after his decease. But Dr. Torkos, the writer, being again applied to, immediately transmitted the copy of it printed above: and, in order to supply in some measure the want of what Mr. Folkes's extensive reading and industry might have furnished the public with, in relation to so very remarkable a fact, the following accounts, printed and manuscript, are subjoined as a supplement to the preceding article.

Extract of a Letter from William Burnet, Esq. F. R. S. eldest Son of Dr. Gilbert Burnet, Lord Bishop of Salisbury, to Doctor Hans Sloane, dated Leyden, May 9, 1708, *N. S.**

In this letter Mr. Burnet states that he had seen, and attentively examined this union of twin sisters at the Hague. They were Hungarians: the urinary passage was between the 2 foremost thighs. The situation of this, as well as of the anus, was the same to outward appearance as naturally, with this difference, that they were between 2 different bodies here, whereas in the course of nature they are between the 2 parts of the same body. It seems probable that their parts were distinct; but that the most remote labia of each were outwardly visible, and the 2 contiguous ones within. There seemed to be no cheat in the thing; and the skin where they were joined was perfectly smooth, without any scar. They were then about 6 years old. They spoke French and High German. They were very full of action, and talked one more than the other. When one stooped to take up any thing, she carried the other quite from the ground; and that one of them often did, being stronger as well as more lively than the other. They had not their feeling common any where but in the place of their conjunction.†

This letter was read to the R. S. on the 12th of May 1708.‡

Soon after the date of Mr. Burnet's letter, the twin sisters were brought to England and publicly shown in London, as appears from the following MS. note in a copy of the print bound up by the writer with Fortunius Licetus de Monstris,‖ edit. Amstelod, 1665, 4to. in the possession of T. Wilbraham, M. D. F. R. S. "Londini 14 Junii 1708, has vidi gemellas (plus annis 6 natas) quarum forma et vivacitas elegantior et vegetior quam pictura et descriptio."

Another account of them by an eye-witness in London is in a MS. vol. among those of Sir Hans Sloane, Bart. in the British Museum, intitled, a short History of human Prodigies and monstrous Births, of Dwarfs, Sleepers, Giants, strong Men, Hermaphrodites, numerous Births, and extreme old Age, &c. The name of the writer was James Paris du Plessis. In p. 39, under the title Two Sisters conjoined, he gives a drawing of them, and the following description: "These 2 monstrous girls were born at Szony in Hungary in the year 1701. They were born conjoined together at the small of the back. I asked the father and mother, if they could not be separated one from the other? but they answered, no;

* Original letters to Sir Hans Sloane, Bart. vol. A—B. in the British Museum.—Orig.

† This letter was accompanied with a print of these conjoined twin sisters.

‡ Journal, vol. xi. p. 143.—Orig.

‖ In this treatise, L. 2. p. 80. is the following passage: in pago Rorbachio non procul Heydelbergâ, Parei etiam relatu, gemini utriusque sexûs obversis tergoribus annexis orti sunt.—Orig.

because the urinary and fœcal vessels and passages were so united, as to have but one issue for the urine, and another for the excrements, betwixt both. They were brisk, merry, and well-bred; they could read, write, and sing very prettily: they could speak 3 different languages, as Hungarian, High Dutch, [German] and French, and were learning English. They were very handsome, very well shaped in all parts, with beautiful faces. Helen was born 3 hours before her sister Judith. When one stooped, she lifted the other from the ground, and carried the other upon her back; neither could they walk side by side. They loved one another very tenderly. Their clothes were fine and neat. They had 2 bodies, 4 sleeves, and 1 petticoat served both bodies, and their shifts the same. When one went forward, the other was forced to go backward."

A later and more particular account is contained in p. 41, & seqq. of a book very seldom met with in this country, being printed at Vienna in 1729, intituled, Gerardi Cornelii Drieschii Historia magnæ Legationis Cæsareæ, quam Caroli VI. auspiciis suscepit Damianus Hugo Virmondtius, &c. The account given in this book coincides in most respects with the preceding accounts. This author however, states that neither the alvine nor the urinary evacuations were always performed at the same time by both sisters; that the menstrua happened at different times, one having them a week or more after the other; that when one was asleep the other was often awake; that one had a desire for food, when the other had not, &c. That between the 2 sisters there was a striking difference, not only as to bodily strength and activity, but also in regard to intellectual powers; that Helen was of very engaging manners, and excited much interest by showing that she was sensible of her own unfortunate situation and that of her sister; that although they both tenderly loved and often kissed each other, yet in their early youth they not unfrequently quarrelled and fought in consequence of their desires for the alvine and urinary evacuation coming on at different times; that during their travels, they had learned various foreign languages, some of which they had forgotten, but that at the time this account was written, they could still speak German, French, and Hungarian; that after they were placed in the convent at Presburgh, they were taught to read and write, instructed in religion, and employed in needlé work, the manufacture of lace, &c.

XL. Observations on the Origin and Use of the Lymphatic Vessels of Animals: being an Extract from the Gulstonian Lectures, read before the Coll. of Physicians of London, By Mark Akenside, M. D. Fell. of the Coll. of Physicians, and F. R. S.* p. 322.

It is proved, by a multitude of experiments, that the lymphatics communicate with the blood-vessels. They may be distended by blowing air, or by injecting water or mercury, into an artery: and the lymph, which they carry, is frequently

* This ingenious poet and physician was a native of Newcastle upon Tyne, where he was born in 1721, and where he received his grammatical education. His father, who was a butcher of the

in a morbid state, found tinged with a mixture of the red globules or crassamentum of the blood. Upon this foundation 2 different theories have been raised, concerning the connection of the lymphatics with the arteries.

Of these, we shall first consider that of the famous professor Boerhaave. He observed, that every artery of the body is greater, in its diameter, than any of its branches; and this observation being found true, as far as our eye and the microscope can inform us, he inferred, by analogy, that it held good even through the most minute subdivisions of the arterial system. But, says he, proportionable to the diameter of the canal is the size of the particles moving through it: therefore, if an ultimate capillary artery, admitting only one red globule at once to pass through it, send off lateral branches, these branebes will be capable of receiving such particles only as are smaller than a red globule. But the particles next in magnitude below the red globules are the yellow serous ones; and the lateral vessel, thus receiving them, is a serous artery, and the trunk of a 2d order of vessels. In like manner, this trunk, being continued on through many lessening branches, will at last grow so minute, as to admit only 1 serous globule: its lateral branches, therefore, will receive only such particles as are smaller than the serous ones: but these are the particles of the lymph; and this lateral branch is a lymphatic artery, and the trunk of a 3d order of vessels. Thus, in the red arteries are contained all the circulated fluids of the body; in the serous arteries, all except the red blood; in the lymphatics, all except the red blood and

Presbyterian sect, not being in a situation to afford the money necessary to procure a liberal education for his son, who shewed a taste for intellectual pursuits, some assistance was offered and received from the dissenters' fund, for defraying the expences of his further education; and he was sent to Edinburgh, to qualify himself for the office of a dissenting minister. He had not been long at this university before he determined to relinquish the study of divinity for that of physic; repaying that contribution (says Dr. Johnson) which being received for a different purpose, he justly thought it dishonourable to retain. In 1741 he went to Leyden, and 3 years after took his degree of M.D. there. On his return from Holland, he first settled at Northampton, but not getting into much practice, he removed from thence to Hampstead, and afterwards to London.

At London he was known as a poet, but had still (as we are also informed by Johnson) to make his way as a physician; and would perhaps have been reduced to great exigencies, but that Mr. Dyson, with an ardour of friendship that has not many examples, allowed him £300 per annum. Thus supported, he advanced gradually in medical reputation. He was appointed physician to St. Thomas's hospital, became F. R. S. was admitted by mandamus to the degree of M. D. in the university of Cambridge, and was afterwards elected a Fellow of the Royal College of Physicians. He had further the honour of being appointed physician to the queen. The last of these honours he did not long enjoy, being cut off by a putrid fever in the summer of 1770, when he was in the 49th year of his age.

The medical writings of Dr. Akenside consist of various papers, published in the Medical Transactions of the College of Physicians, of an Harveian oration, and of his Dissertatio de Dysenteria, the elegant latinity of which has entitled him to the same height (as an able critic has remarked) among the scholars, as he possessed before, in consequence of his poetical writings, among the wits. Of his poems, that on the Pleasures of Imagination, the merits and defects of which have been fully pointed out by Johnson, is the most celebrated.

serum; and this subordination is, according to the same laws, continued down through fluids more subtle than the lymph, to the smallest vessel, which is propagated from the aorta. Such was Boerhaave's doctrine concerning the vascular system of animal bodies; like many of his other notions, ingenious, plausible, and recommending itself, at first sight, by an appearance of geometrical and mechanical accuracy, but founded on insufficient data, and by no means to be reconciled to appearances.

For, in the first place, should we admit his hypothesis, it is certain, that the conical or converging form of the aorta, and the change of direction in its branches, must, in the distant blood-vessels, occasion a great resistance to the moving blood, and a great diminution of its velocity. Suppose that this resistance be, in any capillary red artery, to the resistance in the trunk of the aorta, as any larger assignable number is to unit: the resistance, then, in a capillary serous artery will, to that in the aorta, be as the square of that number is to unit; in the capillary lymphatic, as the cube; and so in progression: that is, the velocity of the fluids, in the remoter series of vessels, will be, physically, nothing. But we know, on the contrary, that some very remote series of vessels have their contents moved with a very considerable velocity; particularly the vessels of the insensible perspiration: and in anatomical injections, the liquor thrown into an artery scarcely returns more easily or speedily by the corresponding vein than by the most subtle excretory ducts. Moreover, there are an infinite number of observations of morbid cases, in which the red blood itself has been evacuated through some of the most remote series of vessels, merely from an occasional temporary obstruction in one part, or a preternatural laxity in another; and without any lasting detriment to the structure and subordination of the vessels; which yet, upon this hypothesis, must have been utterly destroyed before such an irregularity could have happened.

The other theory concerning the origin of the lymphatics has been maintained by some very eminent physiologists later than Boerhaave; and supposes, that these vessels receive their lymph from the blood-vessels, or from the excretories of the larger glands, by the intermediation of only one small vessel, which these authors term a lymphatic artery, invisible in its natural state, nor yet rendered subject to the senses by experiments. But to this it may be answered, that the lymphatics are traced into many parts of the body, and lost there; and therefore most probably have their origin there, where no large gland nor blood-vessel is to be found in their neighbourhood: that it contradicts the whole analogy of nature, to suppose the motion of an animal fluid more discernible in the veins than in the arteries; and, finally, that it seems rather an instance of want of thought, and of being imposed upon by words, to call the lymphatic vessels veins, because they are furnished with valves; and then, because they are called veins, to take for granted, that of course they must be the continuation of arteries.

In attempting to investigate matters too subtile for the cognizance of our senses, the only method, in which we can reasonably proceed, is by inferring from what we know in subjects of the same nature: and our conclusion thus inferred, concerning the subject sought, will be firmer and more unquestionable, in proportion as it resembles the subject known. But if the subjects be really of the same kind; if no difference can be shown between them, in any respect material to the inquiry, in which we are engaged; in this case our inference from analogy becomes the very next thing to a physical certainty: and this he apprehends to be true in relation to the problem before us, concerning the origin of the lymphatic vessels. Though in general we cannot, by experiments, arrive at the extremities of those tubes, nor satisfy ourselves, by inspection, in what manner they receive their fluid; yet in a very considerable number of them we can do both. There is a certain part of the human body very abundantly provided with lymphatics; in which part we can actually force injections through those vessels into a cavity, where their extremities open: and from this cavity, on the other hand, we can at pleasure introduce a coloured liquor into their extremities, and trace it from smaller into wider canals; from capillary tubes, without valves, into large lymphatic trunks copiously furnished with them. We know likewise, that into this cavity are continually exhaling an infinite number of watery and mucous vessels, both arterial tubes and excretory ducts; that these keep it moist with a perpetual vapour, which the extremities of those lymphatics are, in the mean time, perpetually imbibing. Does it not seem strange, while these particulars are known and acknowledged by all the world, that the great authors of anatomy and physiology should never have reasoned from them, but should run into complex and obscure suppositions, in order to explain a process, which they may at any time examine with their own eyes? But perhaps this inadvertency may be accounted for, if we recollect, that at the time when these vessels, and the structure of this part, were discovered, the lymph, and every thing belonging to it, was utterly unknown; and that the vessels in question were first seen and considered as performing another and more remarkable office; which circumstance, it should seem, has prevented succeeding authors from being duly attentive to them in the capacity of lymphatics. However this be, it is certain, that the lymphatics of the mesentery, commonly called the lacteals, differ from those of the other parts in no one particular, save that occasionally they carry chyle instead of lymph, or rather carry lymph mixed, at stated times (that is, for 2 or 3 hours after the creature has taken food) with an emulsion of vegetable and animal substances, and coloured white by that mixture. At other times, that is, during 16 or 18 hours out of the 24, they contain nothing but lymph; and are, in every respect, mere lymphatic vessels, not to be distinguished from those in any other part of the body. Their structure is the same: the membrane of which they are formed, their valves, the lymph which they contain, the glands through which they pass, their direction from smaller tubes to larger,

and from these to the blood, differ in nothing from what we observe of the other lymphatics. Their lymph, in the mean time, is without doubt or controversy supplied from the cavity of the intestines; being the watery moisture continually exhaled there for the purposes of digestion, and for the preservation of the alimentary canal, and as continually taken up by the roots or extremities of these vessels, in order to be carried back to the blood, after it has performed its office in the bowels. Let it also be remembered, that these vessels, in other places of the body, are generally, when we trace them, lost in muscular, tendinous, or membranous parts; and then, it is presumed, it may fairly, and with a good degree of evidence, be concluded, that the lymphatics of the body, in general, have their origin among the little cavities of the cellular substance of the muscles, among the mucous folliculi of the tendons, or the membranous receptacles and ducts of the larger glands; that their extremities or roots do, from these cavities, imbibe the moisture exhaled there from the ultimate arterial tubes, just as the lacteals (the lymphatics of the mesentery) do on the concave surface of the intestines: and that the minute imbibing vessels, by gradually opening one into another, form at length a lymphatic trunk, furnished with valves to prevent the return of its fluid, and tending uniformly, from the extremities and from the viscera, to reconvey to the blood that lymph, or that fine stream, with which they are kept in perpetual moisture; a circumstance indispensably necessary to life and motion: while, at the same time, the continual reabsorption of that moisture by the lymphatics is no less necessary, in order to preserve the blood properly fluid, and to prevent the putrefaction, which would inevitably follow, if this animal vapour was suffered to stagnate in the cavities where it is discharged.

XLI. On the Variation of the Magnetic Needle; with a Set of Tables exhibiting the Result of upwards of Fifty Thousand Observations, in Six Periodic Reviews, from the Year 1700 *to the Year* 1756, *both inclusive; and adapted to every Five Degrees of Latitude and Longitude in the More Frequented Oceans. By William Mountaine and James Dodson, FF. R. S.* p. 329.

Mr. M. and Mr. D. sometime before, by advertisements, announced their intention of preparing a work of this kind; and having, by application to many corporate and public bodies, as the Commissioners of the Navy, the East India Company, the Hudson's Bay Company, the Astronomer Royal, and numerous commanders of ships, and other individuals; by which means, having amassed a vast number of observations of the variation of the magnetic needle, they have here arranged them in very neat order in regular tables, as above announced in the title of the paper, which might be of considerable use on many occasions. Mr. M. and Mr. D. had formerly published some general charts, traversed by many curve lines, showing the corresponding degrees of variation; and on this occasion they now present more complete and correct charts of the same kind, and many curious remarks on this subject.

From the first instant, say these gentlemen, that we made this affair the object of our particular consideration, we have attended to the mode of increase and decrease in the variation; and as a considerable number of observations made at periodic times, and duly registered, seem to be the most essential toward determining the laws of its mutation, or proving its irregularity, we have therefore formed a set of tables, from actual observations collected for the years 1710, 1720, 1730, and 1744, the date of our last chart; which, together with Dr. Halley's for the year 1700, and the present chart now publishing, complete 6 reviews: These are tabulated, and show the quantity of the variation at those several periods, to every 5 degrees of latitude and longitude in the more frequented oceans. Under the equator, in longitude 40° E. from London, the highest variation during the whole 56 years appears to be $17°\frac{1}{4}$ W. and the least $16°\frac{1}{2}$ W.: and in latitude 15° N. longitude 60° W. from London, the variation has been constantly 5° E. but in other places the case has been widely different; for in the latitude 10° S. longitude 60° E. from London, the variation has decreased from 17° W. to $7\frac{1}{4}$ W.; and in latitude 10° S. longitude 5° W. from London, it has increased from $2°\frac{1}{4}$ W. to $12°\frac{3}{4}$ W.; and in latitude 15° N. longitude 20° W. it has increased from 1° to 9° W.

But there is still a more extraordinary appearance in the Indian seas: for instance, under the equator. Where the west variation in the longitude 40° E. is the same in both the above years; and in 1700 the west variation seemed to be regularly decreasing from longitude 50° E. to the longitude 100° E.; but in 1756 we find the west variation decreasing so fast, that we have east variation in the longitude 80°, 85°, and 90° E; and yet, in the longitude 95° and 100° E. we have west variation again.

Longitude from Lond.	Variation in 1700.	Variation in 1756.	Longitude from Lond.	Variation in 1700.	Variation in 1756.
Deg.	Deg.	Deg.	Deg.	Deg.	Deg.
40 E	$16\frac{3}{4}$ W	$16\frac{3}{4}$ W	75 E	$9\frac{3}{4}$ W	1 W
45 E	$17\frac{3}{4}$ W	$14\frac{1}{2}$ W	80 E	$7\frac{3}{4}$ W	$0\frac{1}{4}$ E
50 E	$17\frac{1}{2}$ W	$11\frac{3}{4}$ W	85 E	$5\frac{1}{2}$ W	$1\frac{1}{4}$ E
55 E	$16\frac{1}{2}$ W	$8\frac{3}{4}$ W	90 E	$4\frac{1}{4}$ W	1 E
60 E	$15\frac{1}{4}$ W	$6\frac{3}{4}$ W	95 E	$3\frac{1}{4}$ W	$0\frac{1}{2}$ W
65 E	$13\frac{1}{2}$ W	$4\frac{1}{2}$ W	100 E	$2\frac{1}{2}$ W	1 W
70 E	$11\frac{1}{2}$ W	$2\frac{3}{4}$ W			

Such are the irregularities that experience has shewn us in the variation of the magnetic needle; which appear so considerable, that we cannot think it wholly under the direction of one general and uniform law; but rather conclude with Dr. Gowen Knight, in the 87th prop. of his treatise on attraction and repulsion, that it is influenced by various and different magnetic attractions, in all probability occasioned by the heterogeneous compositions in the great magnet, the earth. Notwithstanding all which, should the sagacity of some eminent philosopher be able to exhibit rules, whereby the quantity of the variation may be computed for future times, yet then such a review as we have now made, will be necessary at a proper interval to prove the truth of them: and should no such rules appear, then will a continued succession of such reviews be necessary so long as commerce and navigation subsist among us. The tables follow.

A Table, *exhibiting the different Variations of the Magnetic Needle in the more frequented Oceans, from the Years* 1700 *to the Year* 1756.

Latitude	Longitude. From London.	Variation. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees	Degrees	Degrees	Degrees	Degrees	Degrees	Degrees	Degrees
0	0	4½ w			10¼ w	14¼ w	15¼ w
0	5 w	2¾ w			8¾ w	12 w	13¼ w
0	10 w	1½ w	3½ w	5½ w	7 w	10 w	11 w
0	15 w	0½ w	2 w	3½ w	5 w	7½ w	9 w
0	20 w	0¾ E	0½ w	1¾ w	3 w	5¼ w	6½ w
0	25 w	1½ E	0½ E	0½ w	1½ w	3 w	4 w
0	30 w	2½ E	2 E	1½ E	1 E	0¾ w	0½ w
0	35 w	3¼ E	3 E	2¾ E	2½ E	1½ E	1½ E
0	40 w	4¼ E	4¼ E	4 E	3¾ E	3½ E	3½ E
0	45 w	5½ E	5½ E	5¼ E	5 E	4¾ E	5 E
0	50 w	6¾ E			5¾ E	6 E	6½ E
0	5 E	6 w			12½ w	15¾ w	16½ w
0	10 E	7¾ w			14¼ w	17 w	17½ w
0	40 E	16¾ w	17 w	17¼ w	17 w	16½ w	16¾ w
0	45 E	17¾ w	17¼ w	16¾ w	16¼ w	15¾ w	14½ w
0	50 E	17½ w	16¾ w	16 w	15 w	14 w	11¾ w
0	55 E	16½ w	15¼ w	14 w	13 w	11½ w	8¾ w
0	60 E	15¼ w	13¾ w	12½ w	11 w	9 w	6 w
0	65 E	13½ w	11¾ w	10 w	8¼ w	6½ w	4½ w
0	70 E	11½ w	9¾ w	7¾ w	6 w	4 w	2¾ w
0	75 E	9¾ w	7¾ w	5¾ w	4 w	1¾ w	1 w
0	80 E	7¾ w	6 w	4¼ w	3 w	0¼ w	0¼ E
0	85 E	5½ w	4 w	2½ w	1½ w	0¼ E	1¼ E
0	90 E	4¼ w	3½ w	1½ w	0½ w	1¼ w	1 E
0	95 E	3¼ w	2¼ w			2 w	0½ w
0	100 E	2½ w	1 w			2½ w	1 w
5 N	0	4¾ w			10½ w	15 w	15¼ w
5 N	5 w	3¼ w			9 w	13 w	13 w
5 N	10 w	1¾ w			7½ w	10¾ w	11¼ w
5 N	15 w	0¾ w	2¼ w	3¾ w	5½ w	8½ w	9¼ w
5 N	20 w	0	1½ w	2½ w	3½ w	6 w	7¼ w
5 N	25 w	1 E	0	1 w	2 w	3¾ w	4½ w
5 N	30 w	1¾ E	1¼ E	0½ E	0¼ w	1½ w	1½ w
5 N	35 w	2½ E	2¼ E	1¾ E	1½ E	0½ E	0½ E
5 N	40 w	3½ E	3¼ E	3 E	2½ E	2¼ E	2¼ E
5 N	45 w	4½ E	4¼ E	4¼ E	4 E	3¾ E	4 E
5 N	50 w	5½ E	5½ E	5¼ B	5¼ E	5 E	5¼ E
5 N	55 w	6¾ E				6¼ E	6½ E
5 N	5 E	6¼ w			12¾ w	16¼ w	16½ w
5 N	10 E	7¾ w			14¼ w	17½ w	17½ w
5 N	45 E	16¾ w	16 w	15¼ w	14¾ w	14 w	13¾ w
5 N	50 E	16¾ w	15¾ w	14¾ w	13½ w	12¼ w	11 w
5 N	55 E	15½ w	14¼ w	13 w	12 w	10 w	8¼ w
5 N	60 E	14¼ w	12¾ w	11¼ w	10 w	8¼ w	6 w
5 N	65 E	12¾ w	11 w	9½ w	8 w	6 w	4½ w
5 N	70 E	10¾ w	9 w	7¼ w	5½ w	3¾ w	2¾ w
5 N	75 E	8¾ w	7 w	5¼ w	3¾ w	1¾ w	0¾ w
5 N	80 E	6½ w	5 w	3½ w	2½ w	0¼ w	0¼ E
5 N	85 E	4¾ w	3¾ w	2¾ w	1½ w	0¼ w	1¼ E
5 N	90 E	3¾ w				1¼ w	0½ E
5 N	95 E	2¾ w				2¼ w	0½ w
10 N	15 w	1¼ w		4½ w	6 w	9¼ w	10 w
10 N	20 w	0½ w	2 w	3¼ w	4½ w	7 w	8 w
10 N	25 w	0¼ E	0¾ w	1¾ w	2¾ w	4½ w	5½ w
10 N	30 w	1 E	0½ E	0	1 w	2¼ w	3 w
10 N	35 w	1¾ E	1¼ E	0¾ E	0¼ E	0½ w	1 w
10 N	40 w	2½ E	2¼ E	2 E	1¾ E	1¼ E	1 E

Latitude.	Longitude, from London.	Variation. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees	Degrees	Degrees	Degrees	Degrees	Degrees	Degrees	Degrees
10 N	45 w	3½ E	3½ E	3¼ E	3 E	2¾ E	2½ E
10 N	50 w	4½ E	4½ E	4¼ E	4¼ E	4 E	4 E
10 N	55 w	5½ E	5½ E	5½ E	5¼ E	5¼ E	5½ E
10 N	60 w	6½ E				6½ E	6¾ E
10 N	50 E	16 w	15 w	14 w	12¾ w	11¼ w	10½ w
10 N	55 E	15 w	13½ w	12 w	11 w	9¼ w	8 w
10 N	60 E	13½ w	12 w	10½ w	9¼ w	7¾ w	6 w
10 N	65 E	12 w	10¾ w	9 w	7½ w	6 w	4½ w
10 N	70 E	10 w	8¾ w	6½ w	5 w	3¾ w	3 w
10 N	75 E	8 w	6½ w	5 w	3½ w	1¾ w	1 E
10 N	80 E	5¾ w	4½ w	3¼ w	2 w	0½ w	0¼ E
10 N	85 E	4½ w	3½ w	2¼ w	1¼ w	0	1 E
10 N	90 E	3½ w				1½ w	0½ E
10 N	95 E	2½ w				2¼ w	0½ w
15 N	20 w	1 w	2½ w	4 w	5½ w	7 w	9 w
15 N	25 w	0½ w	1½ w	2½ w	3½ w	4¾ w	6½ w
15 N	30 w	0¼ E	0¼ w	1 w	1¾ w	2¾ w	4½ w
15 N	35 w	1 E	0½ E	0	0½ w	1¼ w	2½ w
15 N	40 w	1½ E	1¼ E	1 E	0½ E	0	0½ w
15 N	45 w	2¼ E	2¼ E	2 E	1¾ E	1½ E	1 E
15 N	50 w	3¼ E	3¼ E	3¼ E	3 E	2¾ E	2½ E
15 N	55 w	4 E	4 E	4 E	4 E	4 E	3¾ E
15 N	60 w	5 E	5 E	5 E	5 E	5 E	5 E
15 N	65 w	6 E			5 E	6 E	6 E
15 N	70 w	7 E			5½ E	6¾ E	7 E
15 N	75 w	7¾ E				7 E	7¾ E
15 N	80 w	8¼ E				7 E	8 E
15 N	50 E	15½ w	14¼ w	13 w	11¾ w	10½ w	9¾ w
15 N	55 E	14¼ w	12¾ w	11½ w	10¾ w	8¾ w	7¾ w
15 N	60 E	13 w	11¾ w	10¼ w	9 w	7½ w	6 w
15 N	65 E	11½ w	10 w	8¾ w	7½ w	6 w	4½ w
15 N	70 E	9¾ w	8¼ w	6¾ w	5½ w	4 w	2¾ w
15 N	75 E	8 w	6½ w	5 w	3½ w	2¼ w	0¾ w
15 N	80 E	5¾ w	4½ w	3½ w	2½ w	1¼ w	0
15 N	85 E	4¼ w	3¼ w	2¼ w	1 w	0¼ w	0¾ E
15 N	90 E	3¼ w	2½ w	1¼ w		0¾ w	0½ E
15 N	95 E	2½ w	1½ w			2¼ w	0½ w
20 N	20 w	1½ w	3 w	4¼ w	5¼ w	7 w	10 w
20 N	25 w	1 w	2 w	2¾ w	3½ w	4¾ w	8 w
20 N	30 w	0½ w	1 w	1½ w	2¼ w	3¼ w	5¾ w
20 N	35 w	0¼ E	0¼ w	0¾ w	1½ w	2¼ w	4 w
20 N	40 w	0¾ E	¼ E	0	0½ w	1 w	2½ w
20 N	45 w	1½ E	1¼ E	1 E	¾ E	0¼ E	0¾ w
20 N	50 w	2 E	2 E	1¾ E	1½ E	1¼ E	0¾ E
20 N	55 w	2¾ E	2¾ E	2¾ E	2½ E	2½ E	2 E
20 N	60 w	3¾ E	3¾ E	3¾ E	3½ E	3½ E	3¼ E
20 N	65 w	4¾ E				4¼ E	4 E
20 N	70 w	5½ E				5 E	5 E
20 N	75 w	6¼ E				5¼ E	5¼ E
20 N	80 w	7 E				5¼ E	6 E
20 N	60 E	12¾ w	11¼ w	10 w	9 w	7½ w	6 w
20 N	65 E	11½ w	10 w	8½ w	7¼ w	6 w	4½ w
20 N	70 E	9¾ w	8½ w	7 w	5¾ w	4½ w	2¾ w
20 N	90 E	3½ w	2½ w	1½ w	1 w	0½ w	1 E
25 N	20 w	2 w	3¼ w	4 w	5¼ w	7 w	11 w
25 N	25 w	1¾ w	2¼ w	2¾ w	4 w	5¼ w	9½ w

Latitude.	Longitude, from London.	Variation. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees	Degrees	Degrees	Degrees	Degrees	Degrees	Degrees	Degrees
25 N	30 W	1¼ W	1¾ W	2¼ W	3 W	4 W	7½ W
25 N	35 W	1 W	1½ W	2 W	2½ W	3 W	5½ W
25 N	40 W	0½ W	0¾ W	1 W	1½ W	2¼ W	3¾ W
25 N	45 W	0¼ E	0	0½ W	1 W	1½ W	2½ W
25 N	50 W	0¾ E	0½ E	0¼ E	0	0½ W	1 W
25 N	55 W	1¼ E	1 E	1 E	¾ E	0½ E	0
25 N	60 W	2 E	2 E	2 E	1½ E	1½ E	1¼ E
25 N	65 W	2¾ E				2¼ E	2¼ E
25 N	70 W	3½ E				3¼ E	2¾ E
25 N	75 W	4¼ E				3½ E	3 E
25 N	80 W	4¾ E				3½ E	3 E
25 N	60 E	12¾ W	11½ W	10¼ W	9 W	7½ W	6 W
25 N	65 E	11½ W	10¼ W	9 W	7½ W	6 W	4½ W
25 N	70 E	10 W	8¾ W	7½ W	6 W	4½ W	2¾ W
30 N	10 W	3½ W				11½ W	13¾ W
30 N	15 W	3¼ W				10¼ W	12¾ W
30 N	20 W	3 W	4¼ W	5½ W	6¼ W	8½ W	12 W
30 N	25 W	2¾ W	3½ W	4¼ W	5½ W	6¾ W	10¾ W
30 N	30 W	2½ W	3 W	3½ W	4¼ W	5¼ W	9 W
30 N	35 W	2¼ W	2¾ W	3¼ W	3¾ W	4¼ W	7 W
30 N	40 W	1¾ W	2 W	2½ W	3 W	3½ W	5¼ W
30 N	45 W	1¼ W	1½ W	2 W	2¼ W	2¾ W	4 W
30 N	50 W	0¾ W	1 W	1½ W	1¾ W	2¼ W	3 W
30 N	55 W	0¼ W	½ W	1 W	1¼ W	1½ W	2¼ W
30 N	60 W	0¼ E	0	¼ W	0½ W	1 W	1½ W
30 N	65 W	0¾ E	½ E	¼ E	0	¼ W	1 W
30 N	70 W	1½ E	1¼ E	1 E	¾ E	¼ E	½ W
30 N	75 W	2 E	1½ E	1¼ E	1 E	½ E	0
30 N	80 W	2¼ E				¾ E	0
35 N	10 W	4¼ W			9¾ W	12¼ W	14¼ W
35 N	15 W	4 W			9¼ W	11½ W	13¾ W
35 N	20 W	4 W			8¼ W	10¼ W	13 W
35 N	25 W	3¾ W	4¾ W	6 W	7½ W	9 W	12¼ W
35 N	30 W	3¾ W	4½ W	5½ W	6¾ W	8 W	10½ W
35 N	35 W	3¾ W	4¼ W	5¼ W	6 W	7 W	8¾ W
35 N	40 W	3¾ W	4 W	4½ W	5¼ W	6¼ W	7¼ W
35 N	45 W	3½ W	3¾ W	4¼ W	4¾ W	5½ W	6¼ W
35 N	50 W	3½ W	3¾ W	4 W	4¼ W	5 W	5½ W
35 N	55 W	3½ W	3¾ W	4 W	4¼ W	4¾ W	5 W
35 N	60 W	3¼ W	3¾ W	4 W	4¼ W	5 W	5¼ W
35 N	65 W	3 W				5¼ W	6 W
35 N	70 W	2½ W				5¾ W	6¾ W
35 N	75 W	2¼ W				6½ W	7 W
40 N	10 W	5 W			10¾ W	13¼ W	15 W
40 N	15 W	5¼ W			10½ W	12¾ W	14½ W
40 N	20 W	5¼ W			10 W	12¼ W	14¼ W
40 N	25 W	5½ W			9½ W	11¼ W	13½ W
40 N	30 W	5½ W	6¾ W	8 W	9 W	10¼ W	12¾ W
40 N	35 W	5¾ W	6½ W	7¼ W	8¼ W	9½ W	11¼ W
40 N	40 W	5¾ W	6¼ W	7¼ W	8 W	9 W	10 W
40 N	45 W	6 W			7½ W	8½ W	9½ W
40 N	50 W	6¼ W			7¼ W	8½ W	9½ W
40 N	55 W	6½ W			7¾ W	8¾ W	10 W
40 N	60 W	6¾ W			8 W	9¼ W	11 W
40 N	65 W	7 W			8½ W	10¼ W	12 W
40 N	70 W	7 W			9 W	11½ W	12¾ W
45 N	5 W	6 W			2½ W	15½ W	16½ W
45 N	10 W	6½ W			12¼ W	15 W	16½ W
45 N	15 W	6¾ W			11¾ W	14¾ W	16¼ W
45 N	20 W	7 W			11½ W	14½ W	16 W
45 N	25 W	7½ W			11½ W	14 W	16 W
45 N	30 W	8 W			11¾ W	13¾ W	15¾ W
45 N	35 W	8¾ W			11¾ W	13¼ W	15¼ W
45 N	40 W	9½ W			12 W	13 W	15 W
45 N	45 W	10½ W			12¾ W	13¼ W	15½ W
45 N	50 W	11½ W			13½ W	14 W	16 W
45 N	55 W	12½ W			14 W	15¼ W	17 W
45 N	60 W	13¾ W			14 W	16 W	18½ W
50 N	5 W	7½ W				17 W	19¼ W
50 N	10 W	7¾ W				17¼ W	19½ W
50 N	15 W	8½ W				17½ W	20 W
50 N	20 W	9 W				17½ W	20½ W
50 N	25 W	9¾ W				17¾ W	21 W
5 S	0	4¼ W			10 W	13¼ W	15 W
5 S	5 W	2½ W			8½ W	11¼ W	13 W
5 S	10 W	1 W	3 W	4¾ W	6½ W	9¼	10¾ W
5 S	15 W	0	1½ W	3 W	4½ W	6¾ W	8½ W
5 S	20 W	1¼ E	0¼ E	0¾ W	2¼ W	4 W	6 W
5 S	25 W	2¼ E	1¼ E	0¼ E	0¾ W	2 W	3 W
5 S	30 W	3¼ E	2¼ E	1¾ E	1 E	0¼ E	0
5 S	35 W	4¼ E	4 E	3½ E	3¼ E	2¾ E	2¾ E
5 S	5 E	6 W			12½ W	15 W	16 W
5 S	10 E	7½ W			14¼ W	16½ W	17 W
5 S	40 E	18 W	17¾ W	17¾ W	17½ W	17½ W	18 W
5 S	45 E	18½ W	18¼ W	18 W	17½ W	17 W	16 W
5 S	50 E	18½ W	17¾ W	17 W	16¼ W	15½ W	12¾ W
5 S	55 E	17½ W	16½ W	15½ W	14¾ W	13 W	9¼ W
5 S	60 E	16¼ W	14¾ W	13¼ W	12 W	10 W	6½ W
5 S	65 E	14¾ W	12¾ W	10¾ W	9 W	7 W	4½ W
5 S	70 E	13 W	11 W	9 W	6¾ W	4½ W	3 W
5 S	75 E	11 W	9 W	7 W	5 W	2¼ W	1 W
5 S	80 E	9 W	7 W	5 W	3 W	0¾ W	0
5 S	85 E	7 W	5½ W	3¾ W	2½ W	0½ W	0¾ E
5 S	90 E	5 W	4½ W	2¾ W	2 W	1¼ W	0¾ E
5 S	95 E	3¾ W	3½ W	1½ W	1½ W	2 W	0½ W
5 S	100 E	3 W	2½ W	1½ W	1 W	2¾ W	1½ W
10 S	0	3¾ W			9½ W	12½ W	14¼ W
10 S	5 W	2¼ W	4¼ W	6¼ W	8¼ W	10½ W	12¾ W
10 S	10 W	0¾ W	2¾ W	4¾ W	6½ W	8¼ W	10¼ W
10 S	15 W	½ E	1 W	2½ W	4 W	5¾ W	7¾ W
10 S	20 W	1¾ E	0½ E	0	1½ W	3 W	4¾ W
10 S	25 W	3 E	2½ E	1¾ E	0¾ E	0¾ W	2 W
10 S	30 W	4 E	3½ E	3 E	2½ E	1¾ E	1 E
10 S	35 W	5¼ E	5 E	4¾ E	4½ W	4 E	3½ E
10 S	5 E	5¾ W			12 W	14½ W	15¾ W
10 S	10 E	7½ W			14 W	16 W	16¾ W
10 S	15 E	9¼ W			15½ W	17¼ W	17¾ W
10 S	40 E	18¾ W	18¾ W	18¾ W	18¾ W	19 W	19¼ W
10 S	45 E	19½ W	19¼ W	19 W	18¾ W	18½ W	18 W
10 S	50 E	19½ W	19 W	18½ W	17¾ W	16¾ W	14¼ W
10 S	55 E	18½ W	17½ W	16½ W	15½ W	14½ W	10½ W
10 S	60 E	17 W	16 W	15 W	14 W	11¼ W	7¼ W
10 S	65 E	15¾ W	13¾ W	11¾ W	10 W	8 W	5 W
10 S	70 E	14½ W	12 W	10 W	8 W	5½ W	3½ W
10 S	75 E	12½ W	10¼ W	9 W	5¾ W	3½ W	2 W
10 S	80 E	10½ W	8 W	5½ W	4 W	1¾ W	1 W
10 S	85 E	8½ W	6¾ W	5 W	3¾ W	1½ W	0¼ W
10 S	90 E	6¾ W	5½ W	4¼ W	3 W	1¾ W	0¼ W

Latitude	Longitude. From London.	Variation. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees	Degrees	Degrees	Degrees	Degrees	Degrees	Degrees	Degrees
10 s	95 E	5 w	4½ w	4 w	3¾ w	2½ w	1 w
10 s	100 E	3¾ w	3½ w	3¼ w	3 w	2¾ w	2 w
10 s	105 E	2¾ w	2¾ w	2½ w	2½ w	3¼ w	2¾ w
10 s	110 E	2 w				3¾ w	3¼ w
15 s	0	3½ w	5½ w	7½ w	9½ w	11¾ w	14 w
15 s	5 w	1¾ w	3¾ w	5¾ w	7¾ w	9½ w	12 w
15 s	10 w	¼ w	2 w	3¾ w	5½ w	7½ w	9½ w
15 s	15 w	1¼ E	0¼ w	1¾ w	3¼ w	4¾ w	7 w
15 s	20 w	2½ E	1½ E	0¾ E	0½ w	1¾ w	4 w
15 s	25 w	3¾ E	3 E	2¼ E	1½ E	0½ E	1 w
15 s	30 w	5 E	4½ E	4 E	3½ E	3 E	2 E
15 s	35 w	6½ E	6¼ E	5¾ E	5½ E	5 E	4½ E
15 s	40 w	7¾ E				6¾ E	6½ E
15 s	5 E	5½ w			11½ w	13¾ w	15¼ w
15 s	10 E	7½ w			14 w	15½ w	16½ w
15 s	40 E	19¾ w	19¾ w	20 w	20 w	20 w	20 w
15 s	45 E	20½ w	20½ w	20¼ w	20¼ w	20 w	19½ w
15 s	50 E	20½ w	20 w	19½ w	18¾ w	18 w	16¼ w
15 s	55 E	19½ w	18½ w	17½ w	16½ w	15½ w	12½ w
15 s	60 E	18¼ w	17 w	16 w	14½ w	12½ w	9 w
15 s	65 E	17 w	15½ w	13½ w	12 w	9¾ w	6 w
15 s	70 E	15½ w	12¾ w	10¾ w	9 w	7¼ w	4¼ w
15 s	75 E	14 w	12 w	9¾ w	7½ w	5 w	3¼ w
15 s	80 E	12 w	10 w	8 w	6 w	3½ w	2½ w
15 s	85 E	10 w	8 w	6¼ w	4¾ w	2¾ w	2¼ w
15 s	90 E	8½ w	7¼ w	6 w	4½ w	3 w	2¼ w
15 s	95 E	6½ w	5¾ w	5 w	4¼ w	3¼ w	2¾ w
15 s	100 E	5 w	4¾ w	4½ w	4¼ w	3¾ w	3½ w
15 s	105 E	3½ w	3½ w	3¾ w	3¾ w	4¼ w	3¾ w
15 s	110 E	2½ w				4½ w	
20 s	0	3¼ w	5¼ w	7¼ w	9 w	11 w	13½ w
20 s	5 w	1½ w	3¼ w	5 w	6¾ w	8¾ w	11¼ w
20 s	10 w	0½ E	1¼ w	3 w	4¾ w	6½ w	8¾ w
20 s	15 w	1¾ E	0½ E	0¾ w	2 w	3½ w	5¾ w
20 s	20 w	3 E	2¾ E	1½ E	0½ w	0½ w	3 w
20 s	25 w	4¾ E	4¼ E	3½ E	2¾ E	2 E	0
20 s	30 w	6 E	5½ E	5¼ E	4¾ E	4¼ E	2½ E
20 s	35 w	7¾ E	7½ E	7¼ E	6¾ E	6½ E	5 E
20 s	40 w	9¼ E				8 E	7½ E
20 s	5 E	5½ w			11 w	13¼ w	15 w
20 s	10 E	7½ w			13½ w	15 w	16¾ w
20 s	15 E	9½ w			15½ w	16½ w	17¾ w
20 s	35 E	19 w	19¼ w	19¾ w	20¼ w	23¾ w	22 w
20 s	40 E	20½ w	20¾ w	21¼ w	21½ w	21¾ w	22 w
20 s	45 E	21¼ w	21¼ w	21½ w	21½ w	21¾ w	21¼ w
20 s	50 E	21¼ w	21 w	20¾ w	20¼ w	19¾ w	18¾ w
20 s	55 E	20½ w	20 w	19¼ w	18½ w	17 w	15 w
20 s	60 E	19½ w	18¼ w	17 w	5¾ w	14½ w	11¼ w
20 s	65 E	18¼ w	17 w	15¾ w	14½ w	12 w	8 w
20 s	70 E	16¾ w	15½ w	13½ w	12 w	10 w	6 w
20 s	75 E	15 w	13 w	10½ w	9 w	7¾ w	4¾ w
20 s	80 E	13½ w	11¾ w	9¾ w	8 w	6 w	4½ w
20 s	85 E	11½ w	10 w	8½ w	7 w	5 w	4¼ w
20 s	90 E	10 w	8¾ w	7½ w	6¼ w	4¾ w	4½ w
20 s	95 E	8 w	7¼ w	6½ w	5¾ w	4¾ w	4½ w

Latitude.	Longitude, from London.	Variation. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees	Degrees	Degrees	Degrees	Degrees	Degrees	Degrees	Degrees
20 s	100 E	6½ w	6¼ w	6 w	5½ w	5 w	4½ w
20 s	105 E	4¾ w	5 w	5 w	5 w	5 w	4¾ w
25 s	0	3 w	5 w	7 w	8¾ w	10½ w	12½ w
25 s	5 w	1 w	2¾ w	4½ w	6¼ w	8 w	10 w
25 s	10 w	1 E	0½ w	2¼ w	4 w	5½ w	7½ w
25 s	15 w	2½ E	1¼ E	0	1¼ w	2½ w	4½ w
25 s	20 w	4 E	3¼ E	2 E	1¼ E	0½ E	2¼ w
25 s	25 w	6 E	5¼ E	4½ E	3¾ E	3 E	1 E
25 s	30 w	7½ E	7 E	6½ E	6 E	5½ E	3½ E
25 s	35 w	9¼ E				7½ E	6 E
25 s	40 w	11 E				9 E	
25 s	5 E	5¼ w	7 w	8¾ w	10½ w	12½ w	14½ w
25 s	10 E	7½ w				14½ w	16 w
25 s	15 E	9½ w				16¼ w	17¾ w
25 s	35 E	19½ w	20¼ w	20¾ w	21½ w	22¼ w	23½ w
25 s	40 E	21 w	21½ w	22 w	22½ w	23¼ w	23¾ w
25 s	45 E	22¼ w	22½ w	22¾ w	23 w	23¼ w	23 w
25 s	50 E	22½ w	22½ w	22¼ w	22¼ w	22 w	21 w
25 s	55 E	22 w	21½ w	21 w	20½ w	19¾ w	18 w
25 s	60 E	20¾ w	19¾ w	19 w	18¼ w	17 w	14½ w
25 s	65 E	19¼ w	18¼ w	17¼ w	16¼ w	15 w	11¾ w
25 s	70 E	17¾ w	16¾ w	15¾ w	14½ w	13 w	9½ w
25 s	75 E	16¼ w	15 w	13¾ w	12½ w	11¼ w	8 w
25 s	80 E	14¾ w	13½ w	12¼ w	10¾ w	9¼ w	7½ w
25 s	85 E	13 w	11¾ w	10½ w	9¼ w	8 w	7¼ w
25 s	90 E	11¼ w	10½ w	9¼ w	8¼ w	7¼ w	7 w
25 s	95 E	9½ w	9 w	8½ w	7¾ w	7 w	6½ w
25 s	100 E	7¾ w	7½ w	7¼ w	7 w	6¾ w	6 w
30 s	0	2¾ w	4¼ w	6 w	7¾ w	9½ w	11¾ w
30 s	5 w	0½ w	2 w	3½ w	5¼ w	7 w	9 w
30 s	10 w	1½ E	0	1½ w	3 w	4½ w	6¼ w
30 s	15 w	3¼ E	2¼ E	0¾ E	0½ w	1½ w	3½ w
30 s	20 w	5 E	4¾ E	3¾ E	2¾ E	1½ E	0½ w
30 s	25 w	7¼ E	6½ E	5¾ E	5 E	4 E	2 E
30 s	30 w	9 E				6½ E	4½ E
30 s	35 w	11 E				8½ E	7¼ E
30 s	5 E	5 w	7 w	8¾ w	10¼ w	11¾ w	13¾ w
30 s	10 E	7¼ w	9¼ w	11¼ w	12¾ w	14 w	15½ w
30 s	15 E	9½ w				16 w	17½ w
30 s	30 E	17½ w	18½ w	19½ w	20½ w	21¾ w	23¼ w
30 s	35 E	20¼ w	21 w	21¾ w	22½ w	23½ w	24¾ w
30 s	40 E	21¾ w	22½ w	23¼ w	24 w	24¾ w	25¼ w
30 s	45 E	23 w	23½ w	24 w	24½ w	25¼ w	24½ w
30 s	50 E	23½ w	23¾ w	24 w	24¼ w	24½ w	23¼ w
30 s	55 E	23 w	23 w	23 w	22¾ w	22½ w	21 w
30 s	60 E	21¾ w	21½ w	21 w	20½ w	20 w	18 w
30 s	65 E	20¼ w	19½ w	19 w	18½ w	17¾ w	15 w
30 s	70 E	18¾ w	18 w	17¼ w	16½ w	15½ w	13 w
30 s	75 E	17¼ w	16½ w	15¾ w	14¾ w	13¾ w	11½ w
30 s	80 E	15¾ w	15 w	14 w	13 w	12 w	10¾ w
30 s	85 E	14 w	13¼ w	12½ w	11½ w	10½ w	10¼ w
30 s	90 E	12½ w	11¾ w	11 w	10¼ w	9½ w	9½ w
30 s	95 E	10½ w	10 w	9½ w	9¾ w	8¾ w	9 w
30 s	100 E	8¾ w				8¼ w	

Latitude.	Longitude, from London.	Variation. Anno 1700.	Anno 1710.	Anno 1720.	Anno 1730.	Anno 1744.	Anno 1756.
Degrees	Degrees	Degrees	Degrees	Degrees	Degrees	Degrees	Degrees
35 s	0	2½ w	4 w	5½ w	7 w	8½ w	10½ w
35 s	5 w	0	1 w	2½ w	4 w	5¾ w	7¾ w
35 s	10 w	2¼ E	1⅓ E	0½ w	1¾ w	3¼ w	5½ w
35 s	15 w	4¼ E				0¼ w	2¼ w
35 s	20 w	6¾ E				2½ E	0½ E
35 s	25 w	8¾ E				5 E	3 E
35 s	30 w	10¾ E				7¼ E	5¾ E
35 s	35 w	12¾ E				9¼ E	8¼ E
35 s	5 E	5 w	6½ w	8 w	9½ w	11 w	13 w
35 s	10 E	7¼ w	8½ w	10¼ w	11¾ w	13½ w	15¼ w
35 s	15 E	9¾ w	11¼ w	12¾ w	14¼ w	15½ w	17½ w
35 s	20 E	12½ w	14 w	15½ w	17 w	18½ w	19¾ w
35 s	25 E	15¼ w	16½ w	18 w	19¼ w	20¾ w	22½ w
35 s	30 E	18¼ w	19½ w	20½ w	21½ w	22¾ w	24¼ w
35 s	35 E	21 w	22 w	22¾ w	23½ w	24½ w	26 w
35 s	40 E	22¾ w	23½ w	24¼ w	25 w	26 w	26¾ w
35 s	45 E	24¼ w	25 w	25 w	26 w	27 w	26 w
35 s	50 E	24¾ w	25¼ w	25¾ w	26 w	26½ w	24¾ w
35 s	55 E	24¼ w	24½ w	24½ w	24¾ w	25 w	23 w
35 s	60 E	23 w	23 w	23 w	22¾ w	22¾ w	21 w
35 s	65 E	21½ w	21¼ w	21 w	20¾ w	20¼ w	18¾ w
35 s	70 E	19¾ w	19¼ w	18¾ w	18¼ w	17¾ w	16¾ w
35 s	75 E	18¼ w	17¾ w	17¼ w	17½ w	16 w	15¼ w
35 s	80 E	16¾ w	16¼ w	15¾ w	15 w	14¼ w	14¼ w
35 s	85 E	15 w	14½ w	14 w	13½ w	13 w	13½ w
35 s	90 E	13½ w	13 w	12¾ w	12½ w	12 w	12¾ w
55 s	95 E	11½ w	11½ w	11¼ w	11 w	10¾ w	
40 s	0	2 w	3½ w	5 w	6½ w	7¾ w	9¼ w
40 s	5 w	0¾ E	0½ w	2 w	3½ w	5 w	6¾ w
40 s	10 w	3¼ E				2½ w	4¼ w
40 s	15 w	5½ E				0½ E	1½ w
40 s	20 w	8 E				3¼ E	1¼ E
40 s	25 w	10½ E				5½ E	4 E
40 s	30 w	12½ E				8 E	6½ E
40 s	5 E	4½ w	6 w	7½ w	9 w	10¼ w	12 w
40 s	10 E	7¼ w	8½ w	10 w	11½ w	12¾ w	14½ w
40 s	15 E	9¾ w	11¼ w	12¾ w	14 w	15½ w	17¼ w
40 s	20 E	12¾ w	14¼ w	15½ w	17 w	18½ w	20 w
40 s	25 E	16 w	17¼ w	18½ w	19¾ w	21¼ w	22¾ w
40 s	30 E	19 w	20 w	21¼ w	22¼ w	23½ w	25¼ w
40 s	35 E	21¾ w	22¾ w	23½ w	24½ w	25½ w	27 w
40 s	40 E	23¾ w	24½ w	25 w	26 w	27 w	28½ w
40 s	45 E	25¼ w	26 w	26¾ w	27½ w	28¼ w	27¼ w
40 s	50 E	26 w	26½ w	27 w	27½ w	28¼ w	26 w
40 s	55 E	25¼ w	25½ w	25¾ w	26 w	26½ w	24½ w
40 s	60 E	24 w	24 w	24 w	24¼ w	24¼ w	22¾ w
40 s	65 E	22½ w	22½ w	22¼ w	22¼ w	22 w	20¾ w
40 s	70 E	20¾ w	20½ w	20¼ w	20 w	19½ w	19½ w
40 s	75 E	19¼ w	18¾ w	18¼ w	17¾ w	17¼ w	18¼ w
40 s	80 E	17½ w	17 w	16½ w	16 w	15½ w	17¼ w

Variation of the Magnetic-Needle, from the Islands of Orkney to Hudson's Straits, for the Year 1757.

West Longitude from London.		Degrees of North Latitude. 56	57	58	59	60	61	62	63
Degrees.	Degrees of West Variation.								
4					18	18	19	19	
10				19	19	20	20	21	
27				24	24	25	25		
45		29	29	30	31				
55									
65						39	40	41	

Variation in Hudson's Bay and Straits, for the Year 1757.

West Longitude from London.		Degrees of North Latitude. 52	55	56	57	58	59	60	61	62	63
Degrees.	Degrees of West Variation.										
65								39	40	41	
71									41	41	
79											43
81									38	39	40
83		18	20							39	40
86									35	37	
92					17	17					
94						17	18				
95							18				

We have been informed, that in Hudson's Bay, there has been very little alteration in the variation of the compass during the 20 years last past.

XLII. Of some Extraordinary Tumors on the Head of a Labouring Man in St Bartholomew's Hospital. By James Parsons, M. D., F. R. S. p. 350.

This poor man, whose name was John Tomlinson, gave this account of himself: that he was about 25 years of age: that when he was a boy of 4 or 5 years old at play with other children, he received a blow from one of them on the top of his head, and believes that hurt he then received was the beginning of the tumours. The tumour on the top of his head however grew first, and after having spread all over the vertex, extended gradually downwards over his right shoulder, and forwards over the os frontis, on the same side, till it stretched downwards into a lax flabby substance all over the right side of his face and shoulder: then the upper of three anterior tumours arose from the large one; the middle one from the ala nasi, pulling it down by its weight: and the lower one was pendulous from the inside of the great tumour by a narrow neck. These were the appearances which presented themselves at first sight; but those under the great tumour were no less extraordinary; for on lifting up the great tumour, and looking up under it, his right eye came in sight, with which he saw very well, and the eye was clear and sound; but the under lid was pulled down and stretched to 6 or 7 inches long, to which a tumour hung also, as large as that anterior one at the chin, the lowest of the three; besides several flaps and rugæ of skin and smaller tumours.

The hairy scalp was so stretched by the vertical tumour, that the hairs were driven asunder; so that the tumour was in some places bald, and the whole was rugged and uneven. At its basis all round, till arriving at the extended part that went away to the right shoulder, a bony edge might be distinctly felt, as if the skull was depressed at the top: and yet probably there was no depression of the arch of the inner table, because the man was from his childhood ever very healthy; being never troubled with those symptoms which usually attend a depression of the cranium. From this seeming edge the os frontis shot out a great way over the ossa nasi, perhaps to 2 or 3 inches beyond the frontal sinuses; and was the basis from which the great pendulous tumour hung downwards and forwards.

From the root of the nose under the upper of the 3 smaller tumours arose a large trunk of a vein, which ramified up to the vertical tumour, and to the right over the upper part of the great pendulous one: these were very conspicuous, and served to bring back the residual blood from the tumours: nor was it unlikely that the arteries bore a proportion with these veins in their size, in order to supply the tumours with the matter which had given them their great increase; but these lying concealed, could not be spoken to with any certainty.

If this growth of the frontal bone were compared with that of other exostoses, there might this difference be rationally observed; that other exostoses are generally attended with ulcerous tumours, which are for the most part cancerous; and these might commence at any age. He had drawings taken from the right hand of a man of 50, which represent risings of the radius and ulna, with the fingers, to a most frightful degree; and these began but 6 years before, and were attended with foul running ulcers; and then the bones of the arm and hand on the left side, were beginning to have the same appearances: whereas the frontal bone of the present subject appeared sound, as far as could be judged: nor did there appear the least disposition to ulceration in any part of it. When this is the case, the growth generally begins while the subjects are young. His sensation on every part of these tumours was exactly like that of every other part of his skin, having not the least uneasiness on being handled. This poor man worked at day-labour in the fields till some months before he came to town.

Perhaps it might not be improper to lay down the dimensions of these tumours, as the case was so extraordinary; for the size of them was almost incredible. The vertical tumour was about 7 inches diameter at the basis, where the bony edge was felt, and about 4 inches high from that edge. From that edge, or the basis of the vertical tumour to the bottom of the great tumour, was 10 inches; so that the length of both, from the vertex to the end of the great one, was about 14 inches: and on viewing it, when he turned his side towards you, the whole mass was 8 or 9 inches over all the way; hard at top, and flabby downwards, hanging in a kind of plaits. From the eye to the opposite outline of the great tumour was 6 inches; and lower down, from the left corner of his mouth to the opposite outline of the same tumour, 8 inches. The upper small tumour over the nose, was $1\frac{3}{4}$ inch long by 1 and $\frac{1}{2}$ inch; the middle tumour was 2 inches long from the ala nasi, to which it hung, and of the same breadth; and the lowest tumour shaped like a goose's egg, was $4\frac{1}{2}$ inches long by near 3 nches over.

This man was under the care of Mr. Crane of St. Bartholomew's hospital, who had just then taken off the lowest of these 3 anterior tumours, and also the tumour which hung underneath to the under lid of his right eye. He intended to take off that at the ala nasi next, and so on till he takes away all the smaller tumours first: afterwards the larger would be considered. The substance of those cut off was entirely fat; nor was there the least speck of blood in the lowest of the 3 smaller tumours: but there was an hæmorrhage from a vessel divided in taking off that hanging to the right eye-lid; which soon yielded to the methods he made use of, and went on successfully till quite healed.

XLIII. An Extract from the Register of the Parish of Great Shefford, near Lamborne in Berkshire, for Ten Years: with Observations on the same. By the Rev. Mr. Richard Forster, Rector of Great Shefford. p. 356.

From Lady-day 1747, to ditto 1757, baptized, males 73, females 75; in all 148.

Buried, males 44, females 39; in all 83.

The number of people 425; the number of houses 90; the number of acres 2245, of which $\frac{1}{6}$ is waste.

XLIV. A Remarkable Case of an Aneurism, or Disease of the Principal Artery of the Thigh, occasioned by a Fall. To which is prefixed a Short Account of the Uncertainty of the Distinguishing Symptoms of this Disease. By Jos. Warner, F.R.S. p. 363.

[This case may be consulted in this author's tracts published in 1760, under the title of Cases in Surgery.]

XLV. Further Experiments for Increasing the Quantity of Steam in a Fire-Engine. By Keane Fitzgerald, Esq. F. R. S. p. 370.

Mr. F. here, on further experiments, retracts an account he before gave in N° x of this 50th vol. and then proceeds. In order to try what difference the air passing through a thinner body of water might occasion, he brought the horizontal pipe, which was placed 12 inches under the surface of the water, to within 6 inches; and found on setting the engine to work, that the leaden pipe, for the conveyance of air from the bellows into the boiler, became much hotter than he had perceived it before; which could not happen if a constant cool air had passed through: and on shutting the cock, which was fixed in the leaden pipe to hinder the steam from ascending into the bellows before the engine should be set to work, though no air could then possibly pass through, yet the bellows still continued to move with the same regularity as before; which, on examination was found defective on the inside, where the middle board that divides the two bodies, was warped and cracked in several places, through which the air passed very regularly from one body to the other at each stroke, instead of passing through the pipe into the boiler as imagined. By this the cause of deception was evident; which he was still in hopes of remedying, by having a new pair of bellows made somewhat larger, and much stronger. When this was fixed and the engine worked a few strokes, he was surprised to find the bellows did not come down, but remained fully charged with air, though it had 400 lb. weight upon it; and that on increasing the weight gradually to 1400 lb. which was as much as the bellows could support, the air was not forced through

He also made several experiments, by lowering the horizontal pipe 2 feet unde the surface of the water, and raising it at different times to within 4 inches of the surface, and could not at any depth force the air through, while the engine worked; but on opening the steam-pipe, which is a pipe for letting the steam pass from the boiler whenever the engine stops, the bellows could then readily force the air through, though the water boiled ever so strong, and seemingly made a surprising increase of steam. It is a very doubtful matter, whether air forced through boiling water would have answered the purpose intended: but he believes it was never imagined, that air could not be readily forced through, until proved by the foregoing experiments.

XLVI. Observation of a Lunar Eclipse, March 27, 1755, at Lisbon. By Father J. Chevalier, F. R. S. p. 374.

Beginning of the penumbra	10^h	29^m	50^s
Beginning of the eclipse, doubtful	10	33	35
It was now certainly begun	10	34	5
The total immersion	11	33	50
Beginning of the emersion	12	4	38
End of the eclipse	13	13	2
End of the penumbra, doubtful	13	16	50

XLVII. Observations of a Lunar Eclipse, Feb. 4, 1757, &c. at Lisbon. By Fa. J. Chevalier, F. R. S. p. 376.

Beginning of the penumbra	4^h	52^m	49^s morn.
Beginning of the eclipse, doubtful	4	55	29
It had now certainly begun	4	57	30

Clouds prevented further observations.

Observations of the Eclipses of Jupiter's Satellites.

An. 1757, March 21, the 1st sat. immersed	11^h	13^m	1^s aftern.
22, the 3d sat. immersed	0	13	32 morn.

XLVIII. Observations of the Eclipses of Jupiter's Satellites at Lisbon, in the Year 1757. By J. Chevalier. p. 378.

June 7, the 1st sat. emerged	10^h	29^m	21^s aftern.
8, the 2d sat. emerged	8	32	48 aftern.
8, the 3d sat. emerged	9	36	25 aftern.
15, the 2d sat. emerged	11	6	15 aftern.
16, the 3d sat. immersed	0	0	29 morn.

XLIX. A Remarkable Case of the Efficacy of the Bark in a Mortification. By Mr. Richard Grindall, Surgeon to the London Hospital. p. 379.

June 28, 1757, Mary Alexander, aged 31, was brought into the London hospital, having a mortification in both hands, which reached 1 inch and ½ above the wrists. All her toes, and about 1 inch of 1 foot beyond the last joint, were mortified; her nose was also destroyed by a mortification; and all these happened at the same time. On inquiry into the cause of this misfortune, Mr. G. found, that on the 30th of May she was seized with a quotidian ague, which usually began about 3 o'clock in the afternoon, and lasted nearly 2 hours; succeeded by a hot fit, and then a violent sweat. And in this manner she was afflicted 7 days without any material alteration; when, being informed by a neighbour, of a person who had an infallible remedy for the cure of an ague, she applied to him. He brought her 2 phials, containing about 1 ounce and ½ each, of a pale yellowish liquor; one of which he directed her to take directly, promising that she should have no return of the fit of consequence; and that if she had any small return, the 2d bottle should cure her effectually. In consequence of which she took one dose, which was at the time the cold fit had been on about ¼ of an hour: she had no sooner swallowed it, but, as she said, her stomach was on fire, and felt as if she had swallowed the strongest dram possible. The cold fit left her instantly; but she was immediately seized with so violent a fever, as to make her burn, and be extremely thirsty, the following night, till the next morning, when a sweat a little relieved her from the violent heat. When she rose in the morning, she was much troubled with an itching in the hands, feet, and nose; and soon after all those parts began to feel numbed, or, as she described it, as if her hands and feet were asleep; which she took but little notice of, till the evening of that day, when she found the nails of both hands and feet were turning black, and, at the same time feeling great pain in both, as also in her nose, and that they appeared of a darkish red colour, like the skin in cold weather. Whereupon at 9 o'clock that night she sent for an apothecary, from whom, Mr. G. had been informed, the person before mentioned had bought the medicine which he gave her. The apothecary was not at home; his journeyman went, and finding the woman had a difficulty of breathing, ordered her a mixture with spermaceti and ammoniacum to be taken occasionally. The apothecary did not see her himself till the 16th of June, when finding her in a very bad condition, that her hands, and feet, and nose, were entirely black, and had many vesicles or small bladders on them, filled with a blackish bloody water; he opened them, and let out the fluid, and dressed them with yellow basilicon; and in this manner continued treating her till the 20th of

the same month, when, finding no material alteration for the better, he ordered her a brownish mixture, of which she was to take four spoonfuls every 4 hours; which, he informed Mr. G. was a decoction of the bark; and said, on taking this, she was better, as the mortification seemed inclined to stop. But as it was a bad case, he advised the woman to be carried to an hospital: and in this condition was she brought in, when she was immediately put into a course of the bark, taking 1 dr. of the powder every 4 hours; and in 48 hours taking it there was a perfect separation of all the mortified parts. She was then ordered to take it only 3 times in 24 hours; and pursuing this method for 8 days, there was a very good digestion from the parts above the mortification. The mortified part became now so offensive, that the poor woman pressed him much to take off her hands, assuring him she would go through the operations with good courage, being very desirous to live, though in this miserable condition.

July the 24th he took off both her hands: he had very little more to do than saw the bones, nature having stopped the bleeding, when she stopped the mortification. In a day or two after, he took off all the toes from both feet, and now discontinued the bark, the parts appearing in a healing condition; which went on so for 5 weeks, when, on a sudden, the parts began to look livid, her stomach failed her, and she was feverish; but, on taking 1 oz. of the bark, in 36 hours her sores began again to look well. She was not suffered to leave off the bark so soon this time, but continued taking it twice a day for a month. She was then almost well: that part of her face from which the nose mortified, was healed in 7 weeks; the stumps of both arms were entirely healed; and both feet were well, only waiting for 1 piece of bone scaling off, which he believed would be in a very short time; and she was then in good health.

L. A Letter from John Pringle, M. D., F. R. S. inclosing Two Papers communicated to him by Robert Whytt, M. D., F. R. S. p. 383.

In this letter Dr. P. states that he had communicated to Dr. Birch Dr. Whytt's postscript to his observations on Lord Walpole's case, with some reflections on Dr. Springfield's account of the lithontriptic quality of the Carlsbad mineral waters: also an instance of the efficacy of electricity in the cure of a palsy by Mr. Brydone.

Postscript to Dr. Whytt's Observations on Lord Walpole's Case.

Making some experiments with different calculi, there was one almost as white as chalk, but less hard than the others; and which was not in the least degree dissolved or softened by being infused 20 days in oystershell lime-water, but yielded somewhat to a solution of Spanish soap in common water. From this experiment it may be concluded, that it is better to prescribe both soap and lime-water for the stone, than either of them alone; and that if one of these reme-

5

dies has failed of giving relief, the other ought to be tried: for as the above white calculus, which yielded a little to the solution of soap, resisted lime-water; so there may perhaps be others, that are readily dissolved by lime-water, but little affected by soap.

Dr. Springsfeld's experiments with lime-water are not just; for in several calculi Dr. Whytt found the dissolving power of oystershell lime-water above 8 times greater than he makes it.

Observations on the Lithontriptic Virtue of the Carlsbad Waters, Lime-water, and Soap. By Robert Whytt, M. D. F. R. S. p. 386.

From the experiments related in Dr. Springsfeld's Commentatio de Prærogativa Thermarum Carolinarum, &c. it appears (says Dr. W.) that these waters are not only possessed of a very extraordinary power of dissolving the stone, but that in this respect they greatly exceed lime-water.

(A) Thus, Dr. Springsfeld having infused, for 14 days, in a heat of 96 degrees of Fahrenheit's scale, 3 pieces of the same calculus, each weighing 30 grains, in eggshell lime-water, the Carlsbad water, and in the urine of one who daily drank this last water, renewing these several menstruums every day, he found, on the 15th day, that the calculus in the lime-water had lost 1 gr. the calculus in the Carlsbad water 6 grs. and that in urine 5 grs.

(B) Again, having divided another calculus into 4 parts, each of which was reduced to 80 grs. he put the first in oystershell lime-water, the 2d in Carlsbad water, and the 3d in the urine of a person who drank this water. After 20 days, during which time the menstruums were renewed every day, and kept in a heat of 96 degrees, the dried calculi had lost of their weight as follows: the first 3 grs. the second 18 grs. and the third 14 grs.

Although Dr. W. made no doubt that Dr. Springsfeld, who appeared to be a man of candour, as well as learning, had faithfully related the event of the experiments, which he made; yet either the lime-water he used must have been very weak, or some other mistake must have happened in his experiments: for in all the numerous trials Dr. W. made, about 15 years before, of lime-water, as a solvent for the stone, Dr. W. always found its dissolving power much greater than it appeared in Dr. Springfeld's experiments. And as in these trials different urinary stones were used, it can scarcely be imagined, that it was owing to the peculiar hardness of Dr. Springsfeld's calculi, that the lime-water made so little impression on them. However, to be still further satisfied of this matter, Dr. W. made the following experiments.

1. He put a piece of a very hard calculus, x, weighing 80 grains, in oystershell lime-water, renewing the lime-water every day, and keeping it in a heat between 90 and 106 degrees of Fahrenheit's scale. After 20 days he took out the calculus; and having set it by for some days, till it was become quite dry, he brushed

away all the rotten part of it, which was reduced to a kind of chalky powder, and found that the undissolved part of it weighed 57 grs.—2. At the same time a piece of another calculus, z, weighing 15 grs. was, after a like infusion of 29 days in oystershell lime-water, reduced to 10 grs.—3. He put a piece of z, weighing 14 grs. in a solution of ½ an oz. of the internal part of Spanish soap in 9 ounces of water, and every 3d day renewed the solution, which was kept in a heat of about 60 degrees. After 14 days he found the undissolved part not to exceed 11 grs.—4. A piece of a white chalky calculus, y, weighing 30 grs. had nearly 4 grs. of its substance dissolved, by being 14 days infused as above in a solution of soap.

From N° 1 above, compared with Dr. Springsfeld's exper. (B,) it appeared, that the dissolving power of oystershell lime-water, was to that of the Carlsbad water, as 23 to 18; supposing the calculi used in these experiments to have been equally easy to dissolve.—N° 3, compared with Dr. Springsfeld's exper. (A) showed, that the dissolving power of a solution of the inner part of Spanish soap, in a heat of 60 degrees, was to that of the Carlsbad water in a heat of 96 degrees, as 15 to 14.—From N° 4, compared with (A) the dissolving power of soap was to that of the Carlsbad water, only as 4 to 6; but it was probable, that had the solution of soap been kept in a heat of 96 degrees, its dissolving power would, even in this experiment, have nearly equalled that of the Carlsbad water. It should perhaps be observed, that a piece of the white chalky calculus of N° 4 was not in the smallest degree dissolved by lying in lime-water 20 days.

5. In exper. 19 of his essay on the virtue of lime-water, a piece of a calculus, b, weighing 31 grs. lost 7 grs. by being infused 36 hours, in a heat of above 100 degrees, in very strong oystershell lime-water. And in the same water, of a moderate strength, another piece of b lost, in the same time, 5 grs.—In this last experiment, the lithontriptic virtue of lime-water appeared to be stronger than in N° 1 and 2 above; and greatly exceeded that of the Carlsbad water in Dr. Springsfeld's exper. (A) and (B).

But although, from what had been said, it appeared not only that lime-water, but also a solution of soap, dissolved the stone in close vessels as fast, nay faster, than the thermæ Carolinæ; yet these last waters, when the calculi were so placed in open vessels, that the water from the fountain might constantly flow along them, effected a much quicker dissolution than lime-water, or even soap-lee, or indeed any known menstruum, except perhaps strong spirit of nitre: for, in the first experiment made by Dr. Springsfeld, a calculus of 2 and ½ oz. was, in this manner, quite dissolved in 6 days. From this experiment, compared with that of Dr. Springsfeld mentioned above (B), it will be found, on calculation, that the dissolving power of the Carlsbad water, when it is allowed to flow constantly from the fountain along the stone, is nearly 39 times greater than

when it is only poured fresh on the calculus once a day. What may have been the reason of this surprizing difference of the lithontriptic power of the Carlsbad water in these different circumstances, he pretends not to say. He thought it could scarcely be accounted for from the gentle motion of the water along the surface of the calculus. Was it then owing to some very volatile active part, which the water quickly lost, after being taken from the fountain?

But how great soever the dissolving power of the Carlsbad waters might be, when they issued from the bowels of the earth, yet that they did not communicate a much greater dissolving power to the urine, than lime-water, would appear from comparing the 2 following experiments.—In Dr. Springsfeld's exper. (A) above, the urine of a person, who drank the Carlsbad waters, reduced, in 14 days, a piece of calculus, weighing 30 grs. to 25 grs. And in an experiment made by Dr. Newcome, now Lord Bishop of Llandaff, who drank 4 English pints of oystershell lime-water daily, his Lordship's urine reduced, in 4 months, a piece of calculus, weighing 31 grs. to 3 small bits, weighing in all 6 grs. Whence it followed, that the dissolving power of his Lordship's urine must have been to the dissolving power of the urine of the person who drank the Carlsbad waters, nearly as 35 to 65. But if we consider, that the calculus infused in the urine of the person who drank the Carlsbad waters was kept always in a heat of 96 degrees, while in Dr. Newcome's experiment, which was made during part of the autumn and winter, no artificial heat was used, it will appear probable, that the dissolving power of his Lordship's urine was little inferior to that of the person who drank the Carlsbad waters; for lime-water, in a heat of 96 degrees, dissolves the calculus at least twice as fast, as in the common heat of the air in winter. Further, if it be attended to, that the quantity of Carlsbad waters drank every day before dinner is from 6 to 8 lb. while his Lordship only drank 4lb. of lime-water in 24 hours, it will follow, that whatever the different dissolving powers of the lime-water and Carlsbad waters may be out of the body, yet the former seemed, in proportion to the quantity drank, to communicate at least an equal dissolving power to the urine.

But without presuming to decide certainly, as to the comparative virtue of the Carlsbad waters and lime-water, he concluded with observing, that though the Carlsbad waters are less disagreeable to the taste, and may be drank in larger quantity, than lime-water, yet this last may be drank equally good in all places, and at all seasons of the year; which is not the case with the Carlsbad waters.

An Instance of the Electrical Virtue in the Cure of a Palsy. By Mr. Patrick Brydone, Minister of Coldingham. p. 392.

Eliz. Foster,* aged 33, in poor circumstances, unmarried, about 15 years

* Of the parish of Coldingham, where she had lived all her life. Her father had died of the palsy seven years before.—Orig.

before was seized with a violent nervous fever, accompanied with an asthma, and was so ill, that her life was despaired of. She recovered however from the violence of her distemper, but the sad effects of it remained. For from this time she continued in a weakly state of health till July, 1755, when she was again taken ill of the same kind of fever; and after it went off she was troubled with worse nervous symptoms than ever, ending at last in a paralytic disorder, which sometimes affected the arm, sometimes the leg, of the left side; in such a manner as that these parts, though deprived of all motion for the time, yet still retained their sensibility. In this condition she remained till the spring 1756, when unexpectedly she got much better; but not so far as to get quite rid of her paralytic complaints; which in cold weather seldom failed to produce a numbness, trembling, sensation of cold, and a loss of motion in the left side. About the end of August, her symptoms gradually increased, and in a short time she lost all sensation in her left side. In this state she continued throughout last winter, with the addition of some new complaints; her head shook; her tongue faltered; her left eye became so dim, that she could not distinguish colours with it; and she was often seized with such a universal coldness and insensibility, that those who saw her at such times scarcely knew whether she was dead or alive.

While the woman was in this miserable condition, observing that she had some intermissions, during which she could converse and use her right leg and arm, in one of those intervals Mr. B. proposed trying to relieve her by the power of electricity. With this view, he got her supported in such a manner as to receive the shocks standing, holding the phial in her right hand, while the left was made to touch the gun-barrel. After receiving several very severe shocks, she found herself in better spirits than usual; said she felt a heat, and a pricking pain, in her left thigh and leg, which gradually spread over all that side; and after undergoing the operation for a few minutes longer, she cried out with great joy, that she felt her foot on the ground. The electrical machine producing such extraordinary effects, the action was continued; and that day the woman patiently submitted to receive above 200 shocks from it. The consequence was, that the shaking of her head gradually decreased, till it entirely ceased; that she was able at last to stand without any support; and on leaving the room quite forgot one of her crutches, and walked to the kitchen with very little assistance from the other. That night she continued to be well, and slept better than she had done for several months before, only about midnight she was seized with a faintishness, and took notice of a strong sulphureous taste in her mouth; but both faintness and that taste went off, on drinking a little water. Next day, being electrified as before, her strength sensibly increased during the operation, and when that was over she walked easily with a stick, and could lift several pounds weight with her left hand, which had been so long paralytic before. The

experiment was repeated on the 3d day; by which time she had received in all upwards of 600 severe shocks. She then telling them that she had as much power in the side that had been affected as in the other, they thought it unnecessary to proceed further, as the electricity had already, to all appearance, produced a complete cure. And indeed the patient continued to be well till the Sunday following, viz. about 3 days after the last operation; but on going that day to church, she probably took cold; for on Monday she complained of a numbness in her left hand and foot; but on being again electrified every symptom vanished, and she was perfectly well ever afterwards.*

LI. Of some Fossil Fruits, and other Bodies, found in the Island of Shepey. By James Parsons, M.D., F.R.S. p. 396.

It will not be amiss to give a short detail of such bodies as are capable of either being petrified themselves, or of leaving their impressions in stony matter. By being petrified, is meant being impregnated with stony, pyritical, or any other metalline or sparry matter; for there are innumerable specimens, where all these are apparent.

Testaceous and Crustaceous Animals.—The shelly matter of these is of so compact and dry a nature, that they will endure for ages: and if in a soil or bed where moisture has access, they will receive stony matter into their pores, and become ponderous in proportion to the quantity imbibed. If in a dry place, they will remain fair and sharp, suffering very little change by any length of time; while the flesh of these, being subject to putrefaction, is soon destroyed; and yet some of these may be replaced in due form by stony particles.

Wood.—The kinds of wood found fossil are of very different texture; and this too is according to the places where they are deposited. Some are seen so highly impregnated with a fine stony and pyritical matter, as to bear a polish like a pebble; some, though quite reduced to stone, yet preserving the fibrous appearance of the original state; and some which is found in boggy bottoms, being not at all changed, except in colour: this is called bog oak, or bog deal, well known to country people in many places of these three kingdoms, who light themselves about their business with slips of this wood, cut on purpose instead of candles, as it burns with a clear and durable flame. It is remarkable, that though oak or fir shall lie ages immersed in water under-ground, it shall not putrefy, but acquire such sulphureous particles by lying in steep in the bog-water, as to qualify it for this use. Other wood, deposited in marly ground, is found incrusted over, trunk and branches, with a white crust, the wood remain-

* This account was attested by the patient herself, and by the minister of Coldingham, in Berwickshire, where she lived. The truth of the above statement was also further corroborated by a letter from Dr. Whytt to Dr. Pringle.

ng entire within. At other times wood thus incrusted is eroded by the matter which covers it, having something acrimonious in its substance. We may add to these, clusters of the twigs of shrubs, and small wood, which we find flakes of, incrusted with sparry or calcareous matter, in many places; parts of which are totally changed into that matter, whilst others are only enveloped with it.

Bones of Animals—We see by daily experience, that the human skeleton moulders to dust in a very few years, when buried in mould: so it does even in vaults, where the coffins are kept dry. In the first case, the moisture and salts of the earth divide and dissolve the texture of the bones; in the latter, those of the air, which gradually insinuate themselves into them, and at length destroy them. How long a skeleton whose bones are well dried and prepared, being totally deprived of its medullary substance, will last, as they are now ordered for anatomical purposes, is unknown; but it may be reasonably conjectured, that they will undergo the fate of the softer kinds of wood, such as beech, which grows rotten in no great number of years; because their internal substance is spongy and cellular, and their crust very thin, except about the middle of the bones of the arms and thigh. The same destruction would happen if bodies were deposited in a sandy soil; because water finds its way either by dripping downwards, or by springs underneath. But human skeletons have been found entire within a rock, where neither moisture nor air could get at them. Mr. Minors, an eminent surgeon and anatomist of the Middlesex hospital, when he was in the army at Gibraltar, saw an entire skeleton, standing upright, in a dry rock, part of which had been blown up with gunpowder, in carrying on some works in the fortifications, which left the skeleton quite exposed. Indeed, the bones of elephants have been found in Shepey island, but much destroyed; their size and substance being so considerable, as to resist for a long time that decay which those of the human could not withstand. To these may be added the horns of large animals, as the elk, and others, which have been found in bogs, preserved as the bog-oak, &c. above-mentioned.

Teeth and Palates of Fishes and other Animals.—These are of so hard and firm a texture, as to suffer no great change, wherever found; for no erosion appears in them, their enamel and its polish being entirely preserved; yet sometimes their roots will be found changed, especially in the yellow ones, having no enamel to guard them in their roots.

Parts of Vegetables.—The leaves of plants, whose fibres are firm and dry, will endure for a long time; but those of a succulent nature never can, as they putrify very soon. We see the leaves of ferns of several kinds, polypodium, trichomanes, and other capillary plants, with nodules of stone formed about them; flags, reeds, rushes, equisetum, and many such, of a firm texture, are found in

slate and stone; and even the iuli of trees are said to have been found fossil as their leaves.

Seeds and Fruits.—All seeds and the stones of fruits, having a firm texture, are also capable of being strongly impregnated with stony and pyritical matter; and doubtless the smaller seeds, if carefully looked for, might be found fossil, as well as these now produced, viz. such as have a firmness in the covering; but being small, and mixed with the dirt, sand, &c. probably is the reason of their being overlooked. Fruits of various kinds are found petrified; but this is only in their green state, when they are hard enough to endure till they are impregnated with stony or mineral particles. The rudiments of fruits, when once well formed, and a little advanced, are firm and acid: and the more remote they are from maturity, the more secure from putrefaction; and their acid juice is no small help to their preservation from growing soon rotten. But when the fruit advances in growth, the texture becomes gradually more lax; the acid juices are now beginning to be replaced by saccharine or others more soft; the fibres are driven farther asunder, and they now arrive at their most ripe state: and the utmost maturity of fruits is the next step to putrefaction. Hence they are destroyed before stony or other particles can have time enough to impregnate them: and this is exactly the case with the flesh of animals of every kind. The husks and hard calyces of fruits, as well as their stones, are also susceptible of petrifaction.

If these fruits, now produced, are antediluvian, one would be apt to imagine they in some measure point out, with Dr. Woodward, the time of year in which the deluge began; which he thinks was in May: and yet this very opinion is liable to some objections; because though fruits capable of being petrified, from their green state, may be pretty well formed in May here, as well as in the same latitude elsewhere, in favour of this opinion; yet the stones of fruits are found fossil so perfect, as to make one imagine they were very ripe when deposited in the places where they are discovered; which would induce us to think the deluge happened nearer autumn, unless we could think them the productions of more southern latitudes, where perhaps their fruits are brought to perfection before ours are well formed.

The following observation of Dr. Mason, Woodwardian professor, is well worth notice. It regards the impressions of fishes upon slate. Now there are several kinds of slate, which have such impressions on them: in some there remains only the bare impression, without any part of the fish; in others the scales only, but retaining the entire form of the animal; and in others no part adheres to the slate but the skeleton, or part of it, most commonly the spine. He says that he always observed, that the bones are never seen but on the grey or

blue slate, or their impressions; and that the scales or skin are to be found only on the black stone or slate; which makes him conjecture, that something erosive in the grey slate destroys every part but the bony system; but that the black, being of a more soft and unctuous nature, preserves the scales, and often the very skin. The catalogue of these fossil-fruits, &c. is as follows.

Fig. 1, 3, pl. 6, seem to be figs, petrified when hard and green.—Fig. 2 appears to be a myrobalan, distinguished from the other species of that name by its round figure; and is called the emblica myrobalan. It is nearly destroyed by the pyritical matter, and will not long remain whole.—Fig. 4 seems to be a species of phaseolus, one of those especially distinguished by the fruits. Fructibus splendentibus nigris.—Fig. 5, another phaseolus.—Fig. 7, another. See fig. 4.—Fig. 8, semen cucurbitæ, a large species of American gourd.—Fig. 9, coffee-berries.—Fig. 10, 11, two species of beans, very apparent,—Fig. 12, unknown. This however appears to be a fruit, with the calyx running up, and embracing it, in its hard green state; being somewhat compressed on the upper part, as it lay confined in the earth.—Fig. 13, perhaps a species of staphylodendron.—Fig. 14, a compressed pod of the Arachidna, or underground-pea.—Fig. 15 is evidently an acorn. We have of this species here, and in America also.—Fig. 16, an exotic fruit, like a small melon; but uncertain. Fig. 17 seems to be a fungoides of a very pretty kind.—Fig. 18, probably a seed of a species of water melon.—Fig. 19, seems a small plumb-stone.—Fig. 20, unknown. The calyx seems to run up and embrace this fruit towards the apex.—Fig. 21, unknown. This resembles an American seed, which he had.—Fig. 22, a lachryma Jobi?—Fig. 23, a cherry-stone.

Fig. 24. If this be an euonymus, it is not so far advanced as to form the seeds; and is therefore to be considered only in its progress from the flower towards seeding.—Fig. 25, a berry of the sapindus, or soap-tree, of America, being not at all deformed, only having a little lump of pyrites on it: but there is another quite free.—Fig. 26, huræ germen. This is undoubtedly the young sand-box, or fruit of the hura, so well known for its beautiful form to the curious, who collect specimens of natural history; and seems to show the time of the deluge.—Fig. 27, is certainly the stone of an eastern mango; such as comes over to us pickled, and the stone being opened on one side, is generally stuffed with spices.—Fig. 28, is a large species of euonymus, perhaps of Clusius.—Fig. 29 seems to be a milleped, or woodlouse.—Fig. 30, a small long bean, like our horse-bean; but longer than any we have in England.—Fig. 31, unknown.—Fig. 32, a species of horse-chestnut from America.—Fig. 33, the external husk of the fruit of the sapindus, or soap-tree.—Fig. 34, either an olive, or the yellow myrobalan; but probably the latter.—Fig. 35, seems a small palma-coco.—Fig. 36, 37, 38, 39, unknown. The reason of the four last being not to be distinguished is, that they seem to be the buds of their several species, before they were perfectly formed. So that while some of the antediluvian productions are mature, others appear to be premature; and consequently one would be inclined to think them the inhabitants of places of different latitudes.—Fig. 40, a species of foreign walnut, injured and compressed.—Fig. 41, a plumb-stone.—Fig. 42, the claw of an American crab; which, being on the opposite side of the mass containing the body, could not come in view with it at the same time.—Fig. 43, the body of the crab, with other parts, appearing through the stony matter that invelopes it, which appears to be an induration of yellow clay.—Fig. 44 seems a long American phaseclus. Part of the petrified husk is upon it.—Fig. 45, an American echinite of the flat kind, much resembling that species which Rumphius calls echinus sulcatus primus.—Fig. 46, has all the characteristics of an ear of corn, or some species of grass, of which there are many. Fig. a, a manifest species of pediculus marinus crumped up.—b, a seed-vessel, given him by Mr. Da Costa, found in a clay-pit in Staffordshire. c, Cocculus Indicus.

LII. Observations on the Comet that appeared in Sept. and Oct. 1757, *made at the Royal Observatory by Ja. Bradley, D.D., F.R.S.* p. 408.

When Dr. B. first discovered this comet, it appeared to the naked eye like a dull star of the 5th or 6th magnitude; but viewing it through a 7-foot telescope, he could perceive a small nucleus, surrounded as usual with a nebulous atmosphere, and a short tail extended in a direction opposite to the sun. By comparing its situation with some near stars, Dr. B. collected, that the comet's right ascension was 89° 29′ 10″, and its declination 35° 0′ 20″ north.

In like manner he collected the places of the comet on a great many following days, till the 19th of October, on the morning of which day it appeared so faint, that he could not observe its place. Its elongation from the sun was then but about 20 degrees; and from that day it became always less; which is the principal reason why it was invisible at the time when in its perihelion. The elongation will indeed soon become greater, and yet it is probable that we shall not be able to see the comet again; because its real distance from the sun will be greater than it was when he first saw it, and it will be also 4 times further from us than it was at that time. The comet kept nearly at the same distance from the earth for ten or 12 days together after he first saw it; but its brightness gradually increased then, because it was going nearer to the sun. Afterwards, when its distance from the earth increased, though it continued to approach the sun, yet its lustre never much exceeded that of stars of the 2d magnitude, and the tail was scarcely to be discerned by the naked eye.

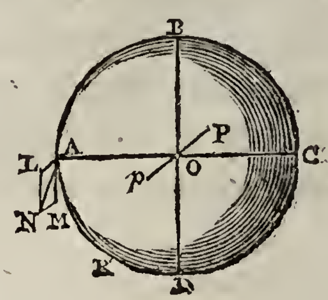
Supposing the trajectory of this comet to be parabolic, Dr. B. collected from the foregoing observations, that its motion round the sun is direct, and that it was in its perihelion October the 21st, at 7^h 55^m mean or equated time at Greenwich. That the inclination of the plane of its trajectory to the ecliptic is 12° 50′ 20″; the place of the descending node ♉ 4° 12′ 50″; the place of the perihelion ♌ 2° 58′ 0″; the distance of the perihelion from the descending node 88° 45′ 10″; the logarithm of the perihelion distance 9.528328; the logarithm of the diurnal motion 0.667636. From these elements, which are adapted to Dr. Halley's general Table for the Motion of Comets in parabolic Orbits, Dr. B. computed the places of this comet for the respective times of the foregoing observations, as in the following table, which contains likewise the longitudes and latitudes deduced from the observed right ascensions and declinations, and also the differences between the computed and observed places. These differences, (no where exceeding 40″) show, that the elements here set down will be sufficient to enable future astronomers to distinguish this comet on another return; but as they do not correspond with the elements of the orbit of any other comet hitherto taken notice of, we cannot determine at present its period.

Greenwich, 1757. Mean time.	Comet's Long. observed.	Latitude observed.	Longitude computed.	Latitude computed.	Diff. Long.	Diff. Lat.
Sept. 12d 16h 2m	♊s 29° 34′ 13″	11° 32′ 16″ N.	♊s 29° 34′ 11″	11° 32′ 20″ N.	− 2″	+ 4″
13 12 37	♋ 2 35 34	11 12 13	♋ 2 35 47	11 12 11	+13	− 2
14 14 0	6 27 45	10 44 3	6 27 42	10 43 43	− 3	−20
17 13 0	17 49 40	9 3 3	17 50 16	9 3 11	+36	−20
19 15 17	26 6 8	7 36 49	26 5 50	7 36 30	−18	−19
23 15 57	♌ 11 19 18	4 33 38	♌ 11 19 4	4 33 32	−14	− 6
24 15 21	14 44 19	3 49 37	14 44 3	3 49 39	−16	+ 2
28 16 22	27 23 43	1 3 44 N.	27 23 32	1 3 52 N.	−11	+ 8
30 16 24	♍ 2 45 43	0 5 30 S.	♍ 2 45 39	0 5 17 S.	− 4	−13
Oct. 2 16 48	7 37 43	1 5 50	7 37 42	1 5 32	− 1	−18
3 16 45	9 51 36	1 32 22	9 51 29	1 31 55	− 7	−27
4 17 0	12 1 4	1 56 42	12 0 25	1 56 23	−39	−19
7 16 54	17 51 3	2 56 48	17 51 6	2 56 24	+ 3	−24
8 16 53	19 39 45	3 13 7	19 39 33	3 12 28	−12	−39
11 16 52	24 47 22	3 48 49	24 47 47	3 49 29	+25	+40
17 17 12	♎ 4 38 58	4 15 42 S.	♎ 4 38 36	4 15 2 S.	−22	−40

LIII. The Resolution of a General Proposition for Determining the Horary Alteration of the Position of the Terrestrial Equator, from the Attraction of the Sun and Moon: with some Remarks on the Solutions given by Other Authors to that Difficult and Important Problem. By Mr. Tho. Simpson, F.R.S. p. 416.

Since the time that Dr. Bradley published his observations and discoveries concerning the inequalities of the precession of the equinox, and of the obliquity of the ecliptic, depending on the position of the lunar nodes, mathematicians in different parts of Europe have set themselves diligently to compute, from physical principles, the effects produced by the sun and moon, in the position of the terrestrial equator; and to examine whether these effects do really correspond with the observations. Two papers on this subject have already appeared in the Phil. Trans.; in which the authors have shown evident marks of skill and penetration. There is nevertheless one part of the subject that seems to have been passed over without a due degree of attention, as well by both those gentlemen, as by Sir Isaac Newton himself. This part, which on account of physical difficulties, is indeed somewhat slippery and perplexing, is the principal subject of this essay.

General Prop. Supposing a homogeneous sphere OABCD (fig. 1) revolving uniformly about its centre, to be acted on at the extremity A of the radius OA, in a direction AL perpendicular to the plane of the equator ABCD, and parallel to the axis of rotation P*p*, by a given force, tending to generate a new motion of rotation at right angles to the former: it is proposed to determine the change that will arise in the direction of the rotation, in consequence of the said force.



Let F denote the given force, by which the motion about the axis P*p* is disturbed, supposing f to represent the centrifugal force of a small particle of matter in the circumference of the equator, arising from the sphere's rotation; and let the whole number of such particles, or the content of the sphere, be denoted by c: let also the momentum of

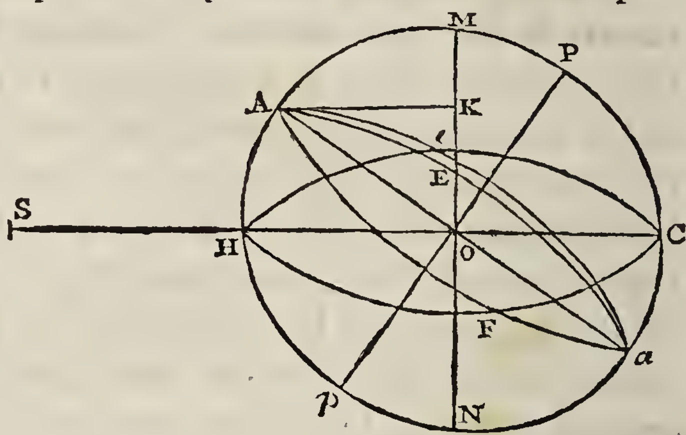
rotation of the whole sphere, or of all the particles, be supposed, in proportion to the momentum of an equal number of particles, revolving at the distance OA of the remotest point A, as n is to unity.

It is well known, that the centripetal force, by which any body is made to revolve in the circumference of a circle, is such as is sufficient to generate all the motion in the body, in a time equal to that in which the body describes an arch of the circumference equal in length to the radius. Therefore, if we here take the arch AR = OA, and assume m to express the time in which that arch would be uniformly described by the point A, the motion of a particle of matter at A (whose central force is represented by f) will be equal to that which might be uniformly generated by the force f, in the time m; and the motion of as many particles (revolving all at the same distance) as are expressed by cn (which by hypothesis is equal to the momentum of the whole body), will consequently be equal to the momentum that might be generated by the force $f \times cn$, in the same time m. Whence it appears, that the momentum of the whole body about its axe Pp, is in proportion to the momentum generated in a given particle of time m', by the given force F in the direction AL, as $ncf \times m$ is to $F \times m'$, or, as unity to $\frac{F}{ncf} \times \frac{m'}{m}$ (because the quantities of motion produced by unequal forces, in unequal times, are in the ratio of the forces and of the times conjunctly). Let therefore AL be taken in proportion to AM, as $\frac{F}{ncf} \times \frac{m'}{m}$ is to unity (supposing AM to be a tangent to the circle ABCD in A, and let the parallelogram AMNL be compleated; drawing also the diagonal AN; then, by the composition of forces, the angle NAM (whose tangent to the radius OA is expressed by $OA \times \frac{F}{ncf} \times \frac{m'}{m}$) will be the change of the direction of the rotation, at the end of the aforesaid time m'. But this angle being exceedingly small, the tangent may be taken to represent the measure of the angle itself; and if Z be assumed to represent the arch described by A, in the same time m' about the centre O, we shall also have $\frac{m'}{m} = \frac{Z}{AR} = \frac{Z}{AO}$, and consequently $OA \times \frac{F}{ncf} \times \frac{m'}{m} = Z \times \frac{F}{ncf}$. Whence it appears, that the angle expressing the change of the direction of the rotation, during any small particle of time, will be in proportion to the angle described about the axe of rotation in the same time, as $\frac{F}{ncf}$ is to unity. Q. E. I.

Though in the preceding proposition the body is supposed to be a perfect sphere, yet the solution holds equally true in every other species of figures, as is manifest from the investigation. It is true indeed, that the value of n will not be the same in these cases, even supposing those of c, f and F to remain unchanged; except in the spheroid only, where, as well as in the sphere, n will be $= \frac{2}{5}$; the momentum of any spheroid about its axis being two-fifths of the mo-

mentum of an equal quantity of matter placed in the circumference of the equator, as is very easy to demonstrate.

But to show now the use and application of the general proportion here derived, in determining the regress of the equinoctial points of the terrestrial spheroid, let AEaF (fig. 2) be the equator, and Pp the axis of the spheroid: also let HECF represent the plane of the ecliptic, S the place of the sun, and HAPNH the plane of the sun's declination, making right angles with the plane of the equator AEaF: then, if AK be supposed parallel, and OKM perpendicular, to OS, and there be assumed T and t to express the respective times of the annual and diurnal revolutions of the earth, it will appear (from the Principia, b. iii. prop. 25) that the force with which a particle of matter at A tends to recede from the line OM, in consequence of the sun's attraction, will be expressed by $\frac{3tt}{TT} \times \frac{AK}{OA} \times f$; f denoting the centrifugal force of the same particle arising from the diurnal rotation. Hence, by the resolution of forces, $\frac{3tt}{TT} \times \frac{AK}{OA} \times \frac{OK}{OA} \times f$ will be the effect of that particle, in a direction perpendicular to OA, to turn the earth about its centre O.



But it is demonstrated by Sir Isaac Newton, and by other authors, that the force of all the particles, or of all the matter in the whole spheroid APap, to turn it about its centre, is equal to $\frac{1}{5}$ of the force of a quantity of matter, placed at A, equal to the excess of the matter in the whole spheroid above that in the inscribed sphere whose axis is Pp. Now this excess (assuming the ratio of π to 1 to express that of the area of a circle to the square of the radius) will be truly represented by $\frac{4\pi}{3} \times OP \times (OA^2 - OP^2)$; and consequently the force of all the matter in the whole earth, by $\frac{3tt}{TT} \times \frac{AK}{OA} \times \frac{OK}{OA} \times \frac{4\pi}{15} \times OP \times (OA^2 - OP^2)$. Let therefore this quantity be now substituted for F, in the general formula $\frac{F}{ncf}$, writing also $\frac{4\pi}{3} \times OA^2 \times OP$, and $\frac{2}{5}$, instead of their equals c and n; by which means we have here $\frac{F}{ncf} = \frac{3tt}{2TT} \times \frac{OA^2 - OP^2}{OA^2} \times \frac{AK \times OK}{OA^2}$. Put the given quantity $\frac{3tt}{2TT} \times \frac{OA^2 - OP^2}{OA^2} = k$; and let the angle EA$e$ represent the horary alteration of the position of the terrestrial equator, arising from the force F here determined, and let the arch Ee be the corresponding regress of the equinoctial point E: then, in the triangle EAe (considered as spherical) it will be sin. e : sin. AE

(:: sin. EAe : sin. Ee) :: EAe : Ee = $\frac{\text{sin. AE} \times \text{EA}e}{\text{sin. E.}} = k \times \frac{\text{sin. AE}}{\text{sin. F}} \times \frac{\text{AK} \times \text{OK}}{\text{OA}^2} =$ $k \times \frac{\text{sin. AE} \times \text{cos. AH} \times \text{sin. AH}}{\text{sin. E.}}$. But in the triangle EHA, right-angled at A (where HA is supposed to represent the sun's declination, AE his right ascension, and HE his distance from the equinoctial point E*) we have (per spherics)

$$\text{sin. AE} : 1 \text{ (rad.)} :: \text{co-t. E} : \text{co-t. AH},$$
$$(\text{sin. AH})^2 : (\text{sin. EH})^2 :: (\text{sin. E})^2 : 1^2 (\text{rad})^2$$

Whence we get, sin. AE $\times$ co-t. AH $\times$ (sin. AH)^2 = (sin. EH)^2 $\times$ co-t. E $\times$ (sin. E)^2. But co-t. AH $\times$ sin. AH = co-s. AH $\times$ 1 (rad.), and co-t. E $\times$ sin. E = co-s. E $\times$ 1 (rad.): therefore sin. AE $\times$ co-s. AH $\times$ sin. AH = (sin. EH)^2 $\times$ co-s. E $\times$ sin. E; and, consequently, $k \times \frac{\text{sin. AE} \times \text{co-s. AH} \times \text{sin. AH}}{\text{sin. E}} = k \times$ co-s. E $\times$ (sin. EH)^2 = Ee.

Let now the sun's longitude EH be denoted by z, considered as a flowing quantity; then, (sin. z)^2 being $= \frac{1}{2} - \frac{1}{2}$ co-s. 2 z, we shall have $k \times$ co-s. E $\times$ (sin. EH)$^2 = \frac{1}{2} k \times$ co-s. E $\times$ (1 — co-s. 2 z). But the angle described about the axe of rotation Pp, in the time that the sun's longitude is augmented by the particle $\dot{z}$, will be $= \frac{T}{t} \times \dot{z}$. Therefore, by the general proposition, as $1 : \frac{1}{2} k \times$ co-s. $\times$ E (1 — co-s. 2 z) :: $\frac{T}{t} \times \dot{z} : \frac{1}{2} k \times \frac{T}{t} \times$ co-s. E $\times \dot{z} - \dot{z}$ co-s. 2 z, the true regress of regress of the equinoctial point E, during that time: whose fluent, $\frac{1}{2} k \times \frac{T}{t} \times$ co-s. E $\times$ (z — $\frac{1}{2}$ sin. 2 z), will consequently be the total regress of the point E, in the time that the sun, by his apparent motion, describes the arch HE or z; which, on the sun's arrival at the solstice, becomes barely $= \frac{1}{2} k \times \frac{T}{t} \times$ co-s. E $\times$ an arch of 90°; the quadruple of which, or $\frac{1}{2} k \times \frac{T}{t} \times$ co-s. E $\times$ 360° ($= \frac{3t}{4T} \times \frac{\text{OA}^2 - \text{OP}^2}{\text{OA}^2} \times$ co-s. E $\times$ 360°) is therefore the whole annual precession of the equinox caused by the sun. This, in numbers (taking $\frac{\text{OP}}{\text{OA}} = \frac{229}{230}$) comes out $\frac{3}{4 \times 366\frac{1}{4}} \times \frac{2}{230\frac{1}{2}} \times 0.917176 \times 360° = 21'' 6'''$.

M. Silvabelle, in his essay on this subject, inserted in vol. 48 of these Transactions,† makes the quantity of the annual precession of the equinox, caused by the sun, to be only the half of what is here determined. But he appears to have

* No error arises from considering the triangles EAe and AEH, as being formed on the surface of a sphere, though the earth itself is not accurately such. The angle EAa representing the effect of the solar force, is properly referred to the surface of a sphere; therefore after its measure is truly determined, the figure APap is itself taken as a sphere, in order to avoid the trouble of introducing a new scheme.—Orig.

† Page 436, Vol. 10, of these Abridgements.

fallen into a twofold mistake. First, in finding the momenta of rotation of the terrestrial spheroid, and of a very slender ring at its equator: which momenta he refers to an axis perpendicular to the plane of the sun's declination, instead of the proper axe of rotation, standing at right angles to the plane of the equator. The difference indeed thence arising, with respect to the spheriod (by reason of its near approach to a sphere) will be inconsiderable; but, in the ring, the case will be quite otherwise; its equinoctial points being made to recede just twice as fast as they ought to do. This may seem the more strange if regard be had to the conclusions relating to the nodes of a satellite, derived from this very assumption. But that these conclusions are true, is owing to a second, or subsequent mistake, at art. 27; where the measure of the sun's force is taken only the half of the true value; by means of which the motion of the equinoctial points of the ring is reduced to its proper quantity, and the motion of the equinoctial points of the terrestrial spheroid, to the half of what it ought to be.

M. Cha. Walmsley, in his Essay on the Precession of the Equinox, printed in this last volume (of the Abridgement, p. 17) has judiciously avoided all mistakes of this last kind, respecting the sun's force, by pursuing the method pointed out by Sir Isaac Newton; but in determining the effect of that force, has fallen into others, not less considerable than those above adverted to. In his 3d Lemma, the momentum of the whole earth, about its diameter, is computed on a supposition, that the momentum or force of each particle is proportional to its distance from the axis of motion, or barely as the quantity of motion in such particle, considered abstractly. No regard is therefore had to the lengths of the unequal levers, by which the particles are supposed to receive and communicate their motion: which doubtless ought to have been included in the consideration.

In the first proposition, he determines in a very ingenious and concise manner, the true annual motion of the nodes of a ring, or of a single satellite, at the earth's equator, revolving with the earth itself, about its centre, in the time of one sidereal day. This motion he finds to be $= \frac{3 \text{ co-s. } 23^\circ 29'}{4 \, rad.} \times \frac{1}{366\frac{1}{4}} \times 360^\circ$ Then in order to infer from this the motion of the equinoctial points of the earth itself, he first diminishes that quantity in the ratio of 2 to 5: because, as is demonstrated by Sir Isaac Newton in his 2d Lemma, the whole force of all the particles situated without the surface of a sphere, inscribed in the spheroid, to turn the body about its centre, will be only 2-5ths of the force of an equal number of particles uniformly disposed round the whole circumference of the equator, in the manner of a ring. The quantity $\frac{3 \text{ co-s. } 23^\circ 29'}{4 \, rad.} \times \frac{2}{5} \times \frac{1}{366\frac{1}{4}} \times 360^\circ$

thus arising, will therefore express the true motion of the equinoctial points of a ring, equal in quantity of matter to the excess of the whole earth above the inscribed sphere, when the force by which the ring tends to turn about its diameter is supposed equal to the force by which the earth itself tends to turn about the same diameter, in consequence of the sun's attraction. Thus far our author agrees with Sir Isaac Newton; but hence in deriving the motion of the equinoctial points of the earth itself, he differs from him; and in the corollary to his 3d Lemma assigns the reasons, why he thinks Sir Isaac Newton, in this particular, has wandered a little from the truth. Instead of diminishing the quantity above exhibited, as Sir Isaac has done, in the ratio of all the motion in the ring, to the motion in the whole earth, he diminishes it in the ratio of the motion of all the matter above the surface of the inscribed sphere, to the motion of the whole earth: which matter, though equal to that of the ring, has yet a different momentum, arising from the different situation of the particles in respect to the axis of motion.

But since the aforesaid quantity, from which the motion of the earth's equinox is derived, as well by this gentleman, as by Sir Isaac Newton, expresses truly the annual regress of the equinoctial points of the ring (and not of the hollow figure formed by the said matter, which is greater, in the ratio of 5 to 4) it seems at least, as reasonable to suppose that the said quantity, to obtain from thence the true regress of the equinoctial points of the earth, ought to be diminished in the former of the two ratios above specified, as that it should be diminished in the latter. But indeed both these ways are defective, even supposing the momenta to have been truly computed; the ratio that ought to be used here, being that of the momenta of the ring and earth about the proper axe of rotation of the two figures, standing at right-angles to the plane of the ring and of the equator. Now this ratio by a very easy computation, is found to be as $230^2—229^2$ to $\frac{2}{5}$ of 230^2; whence the quantity sought comes out $= \frac{3 \text{ co-s. } 23° 29'}{4 \, rad.} \times \frac{1}{366\frac{1}{4}} \times \frac{230^2—229^2}{230^2} \times 360° = 21'' \, 6'''$: which is the same that we before found it to be, and the double of what this author makes it.

What has been hitherto said, relates to that part of the motion only arising from the force of the sun. It will be but justice to observe here, that the effect of the moon, and the inequalities depending on the position of her nodes, are truly assigned by both the gentlemen above-named; the ratio of the diameters of the earth, and density of the moon being so assumed, as to give the maxima of those inequalities, such as the observations require; in consequence of which, and from the law of the increase and decrease (which is rightly determined by theory, though the absolute quantity is not) a true solution, in every other circumstance, is obtained.

LIV. *Remarks on the Heat of the Air in July* 1757, *in an Extract of a Letter from John Huxham, M.D., F.R.S. to William Watson, M.D., F.R.S. dated at Plymouth* 19*th of that Month. With additional Remarks by Dr. Watson.* p. 428.

'From the beginning of June last we have had a very dry season, generally 'very warm, and sometimes excessively hot. From the 7th to the 14th of this 'month the heat was violent; greater indeed than has been known here in the 'memory of man. I have talked with several persons, who have lived a considerable time in Jamaica, Gibraltar, and Minorca; and they severally assert, 'that they never felt such intense heat in any of these places. On the 11th, '12th, and 13th of this month, Fahrenheit's thermometer, in the shade, about '3 o'clock in the afternoon, was at 87; nay, on the 12th it was even above 88.

'Abundance of people have suffered very severely from these excessive heats: 'putrid, bilious, petechial, nervous fevers, are exceedingly common every where. 'Dysenteries, hæmorrhages, most profuse sweats, affect not only those in fevers, 'but a vast many others. The days and nights were so intolerably hot, that little or no sleep was to be gotten. The wind we had, like, the Campsin, 'actually blew hot, though strong.'

'On the 15th, about 7 at night, about Falmouth, Penryn, Truro, &c. a 'pretty smart shock of an earthquake was felt, attended with a hollow rumbling noise, throwing down pewter, china-ware, and such like. The tinners 'felt it 80 fathom under ground. No great damage however was done. The 'day before we had, about 11 o'clock before noon, a most violent hurricane, 'which lasted 5 or 6 minutes, attended with a heavy shower.'

Thus far Dr. Huxham. The following by Dr. Watson:

The heat of the air at London, during the period above-mentioned, was much greater than has been usually observed in these high latitudes; though it was never quite so severe here as at Plymouth. The annexed table exhibits the degrees of the heat, taken here upon the respective days, about 4 o'clock in the afternoon, by a Fahrenheit's thermometer. The instrument was placed in the shade; and the accuracy of the observer, who favoured me with his minutes, is not to be questioned. From hence it appears, that the air at London was, on several days hotter than it had been observed at Madeira for 10 years together: for, by Dr. Thomas Heberden's observations, mentioned in the Philosophical Transactions, the heat of the air at Madeira, during that period, was never but once at 80.

1757, July	5,	75°
	6,	78
	7,	75½
	8,	78
	10,	80¼
	11,	83¼
	12,	80¼
	13,	80
	14,	85
	15,	81
	16,	73

LV. Remarks on the Letter of Mr. John Ellis, F.R.S. to Philip Carteret Webb, Esq. F.R.S. printed in the Philosophical Transactions, Vol. 49, p. 806.* *By Mr. Philip Miller, F.R.S.* p. 430.

Mr. Ellis, in his letter to Mr. Webb, asserts, that the American toxicodendron is not the same with Kœmpfer's arbor vernicifera legitima. This assertion, says (Mr. Miller) makes it necessary to lay before the society the authorities on which I have grounded my belief that they are the same. But it may not be amiss first to take notice, that the shrub mentioned by the Abbé Sauvages is the same with that which the gardeners about London call the poison-ash. The title of it, mentioned by the Abbé Sauvages, was given by myself to that shrub, in a catalogue of trees and shrubs, which was printed in the year 1730; before which it had no generical title applied to it. And about the same time I sent several of the plants to Paris and Holland with that title, which I had raised a few years before from seeds, which were sent by Mr. Catesby from Carolina. And though this shrub had not been reduced to any genus before, yet it had been some years growing in the gardens of the Bishop of London at Fulham, at Mr. Reynardson's at Hillenden, Mr. Darby's at Hoxton, and in the Chelsea Garden, which were raised from seeds sent by Mr. Banister from Virginia; 2 of which were growing at Chelsea in the year 1722, when the care of that garden was intrusted to me.

The first intimation I had of the American shrub being the same with Dr. Kœmpfer's true varnish-tree, was from the late Dr. William Sherrard, in the year 1726, when that gentleman desired me to bring him a specimen of the American toxicodendron from the Chelsea garden; which I accordingly did: and then the Doctor, and Dr. Dillenius, compared it with a dried specimen in the collection of the former, which was gathered in Japan, and which he said he received from Dr. Kœmpfer some years before. It appeared to those 2 gentlemen, that they were the same; and their skill in the sciences of botany was never doubted. About a year after this, I carried a specimen of the American toxicodendron to an annual meeting of some botanists at Sir Hans Sloane's in Bloomsbury; where were present Mr Dale of Braintree, Mr. Joseph Miller, Mr. Rand, and some others; which was then compared with Dr. Kœmpfer's specimen, whose collection Sir Hans Sloane had purchased: and it was the opinion of every one present, that they were the same. Nor has any one doubted of their being so, who has compared the American shrub with Kœmpfer's figure and description of his true varnish-tree, except Mr. Ellis.

And now give me leave to examine his reasons for differing in opinion from every late botanist, who has mentioned this shrub. He says, that the midrib,

* Page 46 of this vol. of Abridgments.

which supports the lobe leaves, is quite smooth in the poison-ash, as is also the under side of the leaves; whereas Dr. Kœmpfer, in his description of the mid-rib of the true varnish-tree, calls it læviter lanuginoso; and in his description of the lobes or pinnæ he says, they are basi inequaliter rotunda; whereas those of the poison-ash come to a point at their footstalks nearly equal to that at the top. These characters, Mr. Ellis thinks, are sufficient to prove that they are different plants: and he blames Dr. Dillenius for having omitted these necessary characters in his description of it; and supposes this must have misled the accurate Linneus, who quotes his Synonyma. But as Dr. Linneus is possessed of Kœmpfer's book, he would little have deserved the appellation of accurate in this particular, had he not consulted the original, but trusted to a copy. But this I know he has done, and is as well assured that the plants in question are the same, as Mr. Ellis can be of the contrary. But here I must observe, that the branch, from which Dr. Kœmpfer's figure is taken, is produced from the lower part of a stem, which seems to have been cut down, and not from a flowering branch; and it is not improbable that his description may have been taken from the same branch: and if this be the case, it is easy to account for the minute differences mentioned by Mr. Ellis; for it would not be difficult to produce instances of hundreds of different trees and shrubs, whose lower and upper branches differ much more in the particulars mentioned by Mr. Ellis, than the figure and description given by Kœmpfer do from the American toxicodendron. I will only mention 2 of the most obvious: the first is the white poplar, whose shoots from the lower part of the stem, and the suckers from the root, are garnished with leaves very different in form and size from those on the upper branches, and are covered on both sides in the spring with a woolly down. The next is the willow with smooth leaves, which, if a standard, and the head lopped off, as is usual, the young shoots are garnished with leaves much broader, and of different forms from those on the older branches; and these have frequently a hairy down on their under surface, which does not appear on those of the older. So that a person, unacquainted with these differences in the same tree, would suppose they were different. And the American toxicodendron has varied in these particulars much more, in different seasons, than what Mr. Ellis has mentioned.

Mr. Ellis next says, that the toxicodendron mentioned by Mr. Catesby, in his Natural History of Carolina, is not the same with that which is now called by the gardeners posion-ash: but I am very positive of the contrary; for most of the plants in the nursery gardens about London were first raised from the seeds which were sent by Mr. Catesby from Carolina; part of which were sent to the late Dr. Sherard, as is mentioned by him in the Philosophical Transactions, N° 367; and another part came into my hands, from which I raised a

great many of the plants, which were distributed, and some of them are now growing in the Chelsea garden. And that this shrub grows naturally in Carolina, I can have no doubt, having received the seeds of it 2 or 3 times from the late Dr. Dale, who gathered them in the woods of that country.

In my paper above-mentioned I likewise observed, that the seeds which were sent to the Royal Society by Father D'Incarville, for those of the true varnish-tree, did not prove to be so; but the plants raised from them were taken to be referred to the spurious varnish-tree of Kœmpfer; which I believed to be the same, and own that it is yet my opinion, notwithstanding what Mr. Ellis has said to the contrary: for the number of lobes or pinnæ, on each leaf, with their manner of arrangement on the midrib, are the same. And here we must observe that the figure of this given by Kœmpfer is from a flowering branch; and every gardener or botanist must know, that the leaves which are situated immediately below the flowers, of most winged-leaved plants, have fewer lobes or pinnæ, than those on the lower branches: therefore I must suppose it to be the case in this plant; and from thence, with some other observations which I made on the seeds, I have asserted it to be the wild or spurious varnish-tree of Kœmpfer. But Mr. Ellis is of a contrary opinion, because the base of the lobes of those plants, which were raised from Father D'Incarville's seeds, are rounded and indented like 2 ears. In Dr. Kœmpfer's figure and description of the fasi-no-ki, the leaves are entire, and come to a point at their base.

Here I think Mr. Ellis is a little too hasty in giving his opinion, as he has not seen this plant in the state that the branch was, from which Kœmpfer's figure was taken. For as there are often such apparent differences between the leaves on the lower branches of trees, and those which are at their extremities, as that in the descriptive titles of the species Dr. Linneus frequently uses them to distinguish one from another; so in making the same allowance for the plant in question, I cannot help thinking that I am in the right, and must abide by my opinion, till the plants raised from Father D'Incarville's seeds have flowered, to convince me of the contrary.

However, I cannot help observing, that Mr. Ellis has given a title to this shrub before he had seen any of the characters, which are necessary to determine the genus. And I have pretty good reason to believe it should not be joined to the rhus; for the 3 seeds which I received from the Royal Society, were shaped like a wedge, being thicker on one edge than the other, and not unlike those of the beech-tree, as I noted in my catalogue when I sowed them; and by their structure seemed as if the 3 seeds had been inclosed in the same capsule. If it proves so, this will by no means agree with the characters of rhus: especially if the male flowers should grow on different plants from the fruit, which is what I suspect. Nor can I agree with Dr. Linneus in this particular of joining all

the species of toxicodendron to the genus of rhus, many of which have their male flowers growing on different plants from the fruit; and therefore would more properly come into his 22d class of dioecia, than his 5th of pentandria, into which he ranges the rhus. At the bottom of the characters of that genus he has added a note, to show the varnish-tree is so. But as there are several other species, which agree in this essential character of distinction; so, according to the Linnean system, they should be separated from the rhus, with another generical title.

Mr. Ellis observes, on the poetical description, which he says Kœmpfer has given of the leaves of the wild varnish-tree turning red in the autumn, that he had not found it to be the case of the tree growing in the stove at Busbridge. How it appeared in that situation, I know not: but the leaves of all those which are growing in the Chelsea-garden, and stand in the open air, do constantly change to a purple colour in the autumn, before they fall off from the shrub: but those of the true varnish-tree are much more remarkable for the deepness of their colour.

Mr. Ellis says, he had received a letter from Dr. Sibthorp, professor of botany at Oxford, in which the Doctor informs him, that there is no specimen of the true varnish-tree in the Sherardian collection at Oxford; but that there is one of fasi-no-ki, or spurious varnish-tree of Kœmpfer. How the Doctor could write so, I cannot conceive; for I am very sure there was no specimen of the latter in that collection while it remained in London, having myself often viewed that part of it: and sure I am Dr. Dillenius never added that synonym to the former: and I do believe the latter was no other way known in Europe, than by Kœmpfer's figure and description of it, excepting that specimen of Kœmpfer's now in the British Museum.

I find Mr. Ellis is inclinable to think, that the poison-ash, as it is called by the gardeners, is the same with the fasi-no-ki, or spurious varnish-tree of Kœmpfer. The difference between these shrubs does not consist in small and minute particulars, but the most obvious striking marks of distinction appear at first sight; for the poison-ash has rarely more than 3 or 4 pair of lobes to each leaf, terminated by an odd one; in which particular it agrees with the true varnish-tree of Kœmpfer; whereas in the figure which Kœmpfer has given of the spurious varnish-tree, the leaves of 7 or 8 pair of lobes terminated by an odd one: and this figure, as I before observed, is drawn from a flowering branch. Every one, who is the least acquainted with these things, knows, that the leaves immediately below the flowers are considerably less than those on the lower part of the branches; therefore this is a more essential note of distinction than those mentioned by Mr. Ellis.

I must also observe, that Mr. Ellis would suggest, that I supposed these 2

shrubs were only varieties of each other produced by culture; whereas it must appear to every one, who reads my paper, that my intention in mentioning the spurious varnish-tree was to show it was different from Kœmpfer's true varnish-tree, though Kœmpfer supposes otherwise.

In my paper I took notice, that one of the best kinds of varnish was collected from the anacardium in Japan; and recommended it to the inhabitants of the British islands in America, to make trial of the occidental anacardium, or cashew-nut tree, which abounds in those islands. This has occasioned Mr. Ellis to take great pains to show, that the eastern and western anacardium were different trees: a fact, which was well known to every botanist before; and of which I could not be ignorant, having been possessed of both sorts near 30 years. But as I was assured, from many repeated experiments, that the milky juice, with which every part of the cashew-tree abounds, would stain linen with as permanent a black as that of the oriental anacardium; so I just hinted, that it was worth the trial. Nor was my hint grounded on those experiments only, but on the informations I had received from persons of the best credit, who had resided long in the American islands, that people are very careful to keep their linen at a distance from those trees, well knowing that if a drop of the juice fell on it, they could never wash out the stain.

But Mr. Ellis, in order to prove that this tree has no such quality of staining, says, he has made some experiments on the caustic oil; with which the shell or cover of the cashew-nut abounds; and that he found it was not endued with any staining quality. But surely those experiments cannot be mentioned to prove that the milky juice of the tree has not this property: and Sir Hans Sloane, in his History of Jamaica, says, that the inhabitants of Jamaica stain their cottons with the bark of the cashew-tree.

LVI. An Answer to the preceding Remarks. By Mr. J. Ellis, F.R.S. p. 441.

My letter to Mr. Webb, which is printed in the 49th volume of the Philosophical Transactions,* was intended to show that Mr. Miller, in his reply to the Abbé Mazeas's letter, had brought no proofs to lessen the discovery, which he tells us the Abbé Sauvages† had made, in attempting to improve the art of painting or staining linens and cottons of a fine durable black colour, by making use of the juice of the Carolina pennated toxicodendron, instead of the common method of staining black with galls and a preparation of iron; which, he says, always turns to a rusty colour when washed.

Mr. Miller, instead of producing the proper proofs, to show that this method

* Page 46 of this vol.

† It should be observed, that the species of rhus used in Sauvages' experiments was the *rhus vernix*, Linn. The species used in those of the Abbé Mazeas was the *rhus toxicodendron*. Linn.

of staining cottons and linens of a black colour was known before, or quoting the authors in which he says it is mentioned, contents himself with telling the Society, that this American toxicodendron is the same plant with the true varnish-tree of Japan; and that calicoes are painted with the juice of this shrub. In my letter to Mr. Webb, I have endeavoured to show, that notwithstanding the authority of Dr. Dillenius, and the authors that have followed him, it does not appear, from Dr. Kœmpfer's description of this Japan plant, that it can be the same with our American one.

The design then of this paper, is to lay before this Society some further reasons, why these plants cannot be the same; and that even if they were the same, Mr. Miller has produced no authority to show that this juice was ever made use of for this purpose abroad; with some remarks on his reply to my letter, in which he obliges me to be more particular than I intended, in explaining some errors, which I find he has run into.

In my letter to Mr. Webb, I have pointed out the exact description which Kœmpfer has given us of the leaves of this plant, showing how much they differ from our American one: but now I shall mention some observations that escaped me before, and which, I think, will give us a clearer proof of this matter. Kœmpfer then informs us, that this Japan varnish-tree, or sitz-dsju, is a tree, not a shrub; and this author, it is well known, is remarkably exact in the description of his Japan plants, making the necessary distinctions between a shrub, an arborescent shrub, and a tree. He then goes on to explain the manner of its growth: and tells us, that it grows with long sappy shoots, very luxuriantly, to the height of a sallow or willow-tree, which we may reasonably allow to be from 20 to 30 feet; whereas this Carolina pennated toxicodendron, as Mr. Miller tells us in his Dictionary, 6th edit. in folio, is a shrub, and seldom rises above 5 feet high with us: and many people, who have been in North America, agree, that it is but a slow grower there, and is one of the shrubby underwoods of that country: so that, allowing it to grow even double the height it does here, it is still but a shrub, in comparison with the other. Further, while Dr. Dillenius was warm with this supposed discovery, of our having got the true Japan varnish-tree in America, attempts were made there, by intelligent persons under his direction, to procure this varnish after the manner of Kœmpfer; but without success, as I am assured by persons of that country now here, with whom the Doctor corresponded.

Let us now consult the growth of the Carolina and Virginia sumachs, or rhuses, in our nursery-gardens, and compare them with this little shrubby toxicodendron, and we shall find, that even in this cold climate nature keeps her regular proportionable pace in the growth of vegetables of the same country. Let us observe the growth of some of these rhuses, and we shall find that great

luxuriancy of the shoots, which Kœmpfer so justly describes in his varnish-tree. One of these American ones even seems to promise the same height as the Japan rhus; whereas this little shrubby toxicodendron still preserves the same dwarfish slow growing habit, that it has in its native country.

This leads me, in the next place, to show, that these two plants must be of different genuses; the one a rhus, and the other a toxicodendron: and if so, according to Mr. Miller, they ought to be properly distinguished, and not ranked together, as Dr. Linneus has done. In order to prove this, let us then examine Kœmpfer's description of the parts of the flower, and see whether it does not answer exactly to the genus of rhus; and whether the flowers are not male and female in themselves, that is, hermaphrodites, on the same tree. And yet Dr. Dillenius, and the authors that have copied after him, say, that his toxicodendron has the male blossoms on one plant, and the female on the other; whence it must evidently be another genus. It appears, however, that Dr. Dillenius was not altogether ignorant of this difference of genus in these two plants; but rather than his toxicodendron, which he had made agree exactly in the leaves, should not agree in the fructification, he makes the accurate Kœmpfer guilty of an unpardonable oversight, in not taking notice of the difference of the sexes of this varnish-tree in different plants; whereas we find, that nothing can be more minutely and judiciously described, than he has done both the male and female parts of the blossom, which change into the fruit on the same plant.

Mr. Miller remarks very justly, that the leaves of the same tree often vary much in shape, such as those of the poplar, sallow, &c. But in answer to this we may reasonably suppose, that Dr. Kœmpfer, who was on the spot, would not choose for his specimens leaves of the most uncommon sorts that were on the tree, and neglect the most common. This would be carrying the supposition further than can be allowed, unless we suppose this author had not the understanding even of a common gardener; for otherwise, I am persuaded, Sir Hans Sloane would not have thought his specimens worth purchasing.

I now come to that part of Mr. Miller's reply, relating to the China varnish-tree,* that was raised from seeds sent to the Royal Society by Father D'Incarville; where he still insists on it, that this is the same with the spurious varnish tree of Kœmpfer. His reasons are, that notwithstanding the indentation and roundness of the bottom of the lobe leaves of the China varnish-tree, and though the lobe leaves of the spurious Japan varnish-tree come to a point at the base, and are nowise indented, but quite even on the edges; yet he says, because they have an equal number of pinnæ, or lobe leaves, on the whole leaf of each tree, they must be the same.

* The true China varnish-tree is supposed to be the *rhus vernix* of Linneus.

In answer to this, I say their lobe-leaves are not equal; for I have examined both the specimens and drawings of Dr. Kœmpfer's spurious varnish-tree, and I don't find that the number of the pinnæ exceed 7 on a side: whereas I have a small specimen of a leaf by me, that was taken from the top of one of D'Incarville's China varnish-trees, which is above 8 feet high, and stands in an open exposure; and this leaf though but a foot long, has 12 lobe-leaves on a side, and each lobe indented at the base. See fig. 5, pl. 7, where this specimen is exactly delineated. At the same time I observed, that the leaves of the young shoots of another tree were a yard long, as they were this summer at the garden of the British Museum. Another thing is remarkable in the leaves of this China varnish-tree; and that is, the lobes of the leaves, as they approach to the end, grow smaller and smaller; whereas in the spurious Japan varnish-tree they are rather, if there is any difference, larger towards the end. I shall make this further remark, that though these indentations on the lobe-leaves may vary in number in this China varnish-tree; yet, as I observed before, since they are continued on even in the smaller leaves at the top of the branches of a tree 8 feet high in the open ground, it appears to me that this specific character, besides the form and insertion of the lobe-leaves, will ever distinguish it as a different species from the fasi-no-ki,* or spurious varnish-tree of Kœmpfer.

Mr. Miller now goes on to tell us, he is confirmed in his belief of their being the same, by making some observations on the seeds of this China varnish-tree; and therefore asserts that they are the same. It is natural to suppose he compared them with the accurate drawings of the seeds of Kœmpfer's fasi-no-ki, p. 794. that being the only place where the seeds of it are described. Mr. Miller goes on, and allows this China varnish-tree changes to a purple in the autumn; but not so deep as the true varnish-tree. I suppose he means by this true varnish-tree, the Carolina pennated toxicodendron; for Kœmpfer has not told us what colour the true varnish-tree of Japan changes to in autumn.

But this is no certain proof on either side of the question, only a corroborating circumstance of the species of a tree: nor should I have mentioned it, but for the manner in which Kœmpfer, with an imagination truly poetical, describes the autumnal beauty of his Fasi-no-ki, or spurious varnish-tree. "Rubore suo autumnati quâ viridantes sylvas suaviter interpolat, intuentium oculos e longinquo in se convertit." Even this description would make one suspect it is not the same with the China varnish-tree, which, I am informed, did not turn purplish in the garden of the British Museum till the first frost came on: whereas it is well known, that some of the rhus's and toxicodendrons, particularly the Carolina pennated one, change to a fine scarlet colour in the beginning

* The fasi-no-ki is the *rhus succedaneum*. Linn.

of a dry autumn, even before any frost appears. In the next paragraph I find Mr. Miller has entirely mistaken the meaning of one part of my letter to Mr. Webb; which I must recommend to him to read again, and he will find it exactly agrees with his own sentiments. There he will find my opinion is, that notwithstanding the change of soil and situation, this sitz-dsju, or true varnish-tree, and the fasi-no-ki, or spurious varnish-tree of Kœmpfer, are distinct species of rhus or toxicodendron, and will ever remain so.

Mr. Miller now desires me, since I have seen Dr. Kœmpfer's specimens in the British Museum, to declare whether I think I am mistaken. In answer to this, and to satisfy Mr. Miller as well as myself, I have been very lately at the museum, and have looked very carefully over Dr. Kœmpfer's specimens, and do sincerely think, as did other judges at the same time, that the sitz-dsju is not the same with the Carolina pennated toxicodendron, nor the fasi-no-ki the same with Father D'Incarville's China varnish-tree.

Mr. Miller informs us, that one of the best kinds of varnishes is collected from the anacardium in Japan. In answer to this, I must beg leave to show the society, that Dr. Kœmpfer does not so much as mention that this anacardium grows in Japan; but that the varnish which is collected from it, is brought to them from Siam: and I believe it will appear plainly from what follows, that there is not a plant of this kind in the kingdom of Japan; for Siam and Cambodia, especially the parts of those kingdoms where Kœmpfer informs us this * anacardium grows, lie in the latitudes of from 10 to 15 degrees north, which must be full as hot as our West Indies: so that it is not probable, that it would bear the cold of the winters in Japan; for Japan lies from the latitudes of 33 to above 40 degrees north, which is about the same parallel with our North American colonies.

Mr. Miller has only the bark of the cashew-tree left to support his argument of the dying property. This the Brasilian writers say, that the native Indians of Brasil used to dye their cotton-yarn with; but of what colour no mention is made. And whether this bark is used to give strength to this yarn, as we dye and tan our fishing-nets with oak-bark, or for ornament, is uncertain; for a great deal of this yarn was used in the making their net-hammocks, as well as their coarse garments. Mr Miller then introduces Sir Hans Sloane, in opposition to Dr. Browne, whose History of Jamaica I had quoted, to prove that the juice of the acajou was of the same nature and properties with that of the gum-arabic, and consequently not fit for varnish: whereas it plainly appears that Dr. Browne is right, and agrees exactly in opinion with Sir Hans.

* This is likewise called the Malacca bean, from its growing in great plenty on that coast, near the equinoctial line.—Orig.

He then makes Sir Hans say, that the inhabitants of Jamaica stain their cottons with the bark of the cashew-nut tree. By this one would naturally conclude, that Mr. Miller has been endeavouring to prove, in opposition to the Abbé Mazeas's letter, that the art of painting or staining cottons of a fine deep black colour, equal to that discovered by the Abbé Sauvages, as described in his experiments on the Carolina toxicodendron, was practised by the English 40 or 50 years ago in Jamaica. If this was the case it is something surprising, that notwithstanding our great intercourse with that island, the calico printers of England never got intelligence of this valuable secret. Further, if Mr. Miller will consult Piso and Margrave, writers of the best authority on the Brasilian plants, he will find their accounts of the acajou exactly correspond with that delivered by Dr. Browne, in his History of Jamaica, as well as Sir Hans Sloane's: for they say, that the juice of this tree is equal in virtue, and mechanical uses, to the best gum-arabic. And if he still doubts, I shall lastly recommend him to go to the British Museum, and there he may see a most elegant specimen of the cashew-gum, which will put this matter quite out of all doubt.

P. S. Since the foregoing paper was read, Professor Sibthorp was so kind to deliver me an exact drawing of the fasi-no-ki in the Sherardian collection at Oxford, taken by the Rev. Mr. William Borlase, F. R. S. the title and synonym of which are both in the hand-writing of Dr. Dillenius, as the professor assures me. See fig. 6, pl. 7.

LVII. On the Number of the People of England. By the Rev. Richard Forster, Rector of Great Shefford in Berkshire. p. 457.

In vol. 49 of the Transactions, there is another medium advanced to determine the amount of the people in England: and this is the number of houses which pay the window tax, and which "amount to about 690,000, besides cottages, that pay nothing." To this is added, that "though the number of cottages be not accurately known, it appears from the accounts given in, that they cannot amount to above 200,000."

Mr. F. thinks that no general public accounts have been given in, of the number of the houses taxed to the window lights; and he thinks that the number of the cottages which do not pay, far exceeds the number of houses that are rated. To prove this, he counted the numbers of both sorts in several parishes, the results of which are in the annexed table.

Great Shefford	90	17
Little Shefford	12	3
Welford	162	62
Chaddleworth	62	20
Bright-Walton	72	21
Catmore	10	1
Farmborough	34	5
Fawley	47	7
East Garston	99	41
	588	177

Here we see, that out of 588 houses, only 177 pay the window-tax. Now if we say with the philosopher ex pede Herculem, and suppose that 200,000

taxable houses stand in the country, we shall have the following proportion, 177 : 588 : : 200,000 : 664406, for the whole number of houses that stand in the country, commonly so called.

Again, Lamborn parish, in which is a market town, contains 445 houses, of which 229 pay the window-tax. Now if we suppose, in like manner, 200,000 taxable houses to stand in country towns, we must then say 229 : 445 : : 200,000 : 388646, the whole number of houses that stand in country towns. The remaining 290,000 houses must be placed in cities and flourishing towns; and must have Dr. Brakenridge's proportion assigned them; on this supposition, we must say 690,000 : 200,000 : : 290,000 : 84,058, for the number of cottages in great towns; which, if added to the houses that pay, makes the whole number in large towns to be 374,058. These 3 sums added together make the total amount of houses in the nation to be 1,427,110.

The two former of these numbers should be multiplied by 5, and the latter by 6. The reason of this difference is the great quantity of servants kept in large towns. By this way of proceeding it appears, that the whole number of people now alive in England, is somewhat more than 7 millions and a half.

The militia act levies 32,000 men on the whole kingdom; and in the west riding of Yorkshire 1 in 45, it is said completed their quota. Now if this proportion be applied to the whole nation, 32,000 × 45 will give 1,440,000 for the number of ballotters; and this multiplied by 5, will amount to 7,200,000 for the total of our people. But he does not build any thing on this computation, as many parts of the nation may have heavier quotas laid on them than the west riding.

But instead of speculating in these theoretical ways, Mr. F. thinks it would be much better to make an actual statement of the baptisms in every parish, taken at different periods, which he thinks a thing easy to be done; and accordingly annexes this account of 3 of them as taken by himself. This table stands in need of no remarks: it speaks, he thinks, loud enough of itself, that our people increase in a very rapid manner.

From	To	Lamborn.	Welford.	Shefford.	Total.
1614	1623 inclus.	327	67	59	463
1624	1633	401	62	64	527
1634	1643	391	119	86	596
1662	1671	441	146	93	680
1672	1681	380	132	108	620
1682	1691	451	201	112	764
1692	1701	366	134	88	588
1702	1711	387	137	84	608
1712	1721	422	171	97	690
1722	1731	483	156	106	745
1732	1741	578	205	128	911
1742	1751	566	253	137	956
1752	1756	349	120	64	533

LVIII. An Answer to the Foregoing Account of the Numbers and Increase of the People of England. By the Rev. William Brakenridge, D. D., F. R. S. p. 465.

Dr. B. states, that being resolved to depend only on the most sure, and general observations, he applied to a public office, where he thought he might possibly get at the number of houses. And he there found, that from the last survey that was made since the year 1750, there were 690,700 houses in England and Wales that paid the window-tax, and the 2 shilling duty on houses; besides cottages that paid nothing. By cottages are understood those which neither pay to church or poor, and are, by act of parliament in 1747, in consideration of the poverty of the people, declared to be exempted both from the tax and the 2 shillings duty; and these only remain not accurately known, to ascertain the whole number of houses. However, they are so far known, that from all the accounts that are hitherto given in, they do not appear to be so many as 300,000: and from what Dr. B. saw in the books of that office, he thinks they were not much above 200,000. And therefore, if there are not 300,000 cottages, there cannot be a million of houses in the whole in England and Wales; and the rated houses are to the cottages more than 2 to one; of both which, according to the returns made, there is now about one in 17; or 58,800 empty throughout the kingdom. But if we were to allow that there are a million of houses in the whole; which is more than the gentlemen in the above mentioned office believe, and then deduct those that are empty, there could not be above 941,200 inhabited houses; and consequently supposing 6 to a house, about 5,647,200 people, or near about 5 millions and a half; which at the utmost, is what he insists on to be the real number.

But now the gentleman who objects to these calculations, thinks that Dr. B. has made the number of houses too few, and that in the whole there are above 1,400,000 houses, of which he imagines there are more than 700,000 cottages; for he supposes them to be more than the rated houses; and thence he infers, that there are about 7 millions and a half of people in England and Wales. But Dr. B. is so far from thinking that he has under-rated them, that he suspects he has rather made them more than they are. However, this controversy will soon be determined he says, there being now orders given to all the officers concerned in the window-tax, to make an exact return of all the cottages, as well as the rated houses, in each of their several districts.

Dr. B. then examines the means and process by which Mr. F. computes, in making the number of persons in England to amount to about $7\frac{1}{2}$ millions; and these says Dr. B. are too imperfect and too small to authorize such a conclusion.

And as to what the gentleman mentions concerning the militia, he seems, says

Dr. B. to be much mistaken. For if the proportion be as he says, that one in 45 is levied, this directly proves the number of people in England and Wales to be about 5 millions and a half, according to my calculation; because the electors or balloters are the fencible men, or those able to carry arms. And if the whole levy be 32,000 then 45 multiplied by 32,000 will give 1,440,000 for all the fencible men in England. But Dr. Halley has clearly showed that the fencible men are one quarter of the whole people, children included; and therefore 4 times 1,440,000, or 5,760,000, will be the whole number of the people; which is nearly what he made them before.

LIX. On the Effects of Electricity in Paralytic Cases. By Benjamin Franklin, Esq. F. R. S. p. 481.

The following is what Mr. F. at present recollects, relating to the effects of electricity in paralytic cases, which have fallen under his observation. Some years since, when the newspapers mentioned great cures performed in Italy or Germany, by means of electricity, a number of paralytics were brought to him from different parts of Pennsylvania, and the neighbouring provinces to be electrised; which he did for them at their request. His method was to place the patient first in a chair, on an electric stool, and draw a number of large strong sparks from all parts of the affected limb or side. Then he fully charged two 6 gallon glass jars, each of which had about 3 square feet of surface coated; and he sent the united shock of these through the affected limb or limbs; repeating the stroke commonly 3 times each day. The first thing observed was an immediate greater sensible warmth in the lame limbs that had received the stroke, than in the others: and the next morning the patients usually related, that they had in the night felt a pricking sensation in the flesh of the paralytic limbs; and would sometimes show a number of small red spots, which they supposed were occasioned by those prickings. The limbs too were found more capable of voluntary motion, and seemed to receive strength. A man, for instance, who could not the first day lift the lame hand from off his knee, would the next day raise it 4 or 5 inches, the 3d day higher; and on the 5th day was able, but with a feeble languid motion, to take off his hat. These appearances gave great spirits to the patients, and made them hope a perfect cure; but he did not remember that he ever saw any amendment after the 5th day: which the patients perceiving, and finding the shocks pretty severe, they became discouraged, went home, and in a short time relapsed; so that he never knew any advantage from electricity in palsies, that was permanent. And how far the apparent temporary advantage might arise from the exercise in the patients journey, and coming daily to his house, or from the spirits given by the hopes of suc-

cess, enabling them to exert more strength in moving their limbs, he will not pretend to say.

Perhaps some permanent advantage might have been obtained, if the electric shocks had been accompanied with proper medicine and regimen, under the direction of a skilful physician. It may be too, that a few great strokes as given in his method, may not be so proper as many small ones; since, by the account from Scotland of a case, in which 200 shocks from a phial were given daily, it seems that a perfect cure has been made. As to any uncommon strength supposed to be in the machine used in that case, he imagines it could have no share in the effect produced; since the strength of the shock from charged glass, is in proportion to the quantity of surface of the glass coated; so that his shocks from those large jars must have been much greater than any that could be received from a phial held in the hand.

LX. Observations on the late Comet in Sept. and Oct. 1757; made at the Hague by Mr. D. Klinkenberg. Translated from the Low Dutch. p. 483.

Mr. K. observed this comet from Sept. 16th in the morning, till Oct. the 11th in the morning; and found its situations, according to his method, as follows:

1757,			Longit.			Latit.		
Sept. 16,	at 4 h. ante mer. The comet in	♋	10°	15′	with	10°	10′	North.
17	.. 3	♋	14	7		9	38	
18	.. 3¾	♋	18	10		8	57	
19	.. 4	♋	22	1		8	17	
22	.. 2¾	♌	3	46		6	15	
23	.. 4	♌	7	36		5	24	
25	.. 4¼	♌	14	50		4	6	
28	.. 4	♌	24	22		1	41	
Oct. 1	.. 4¾	♍	2	46		0	12	South
4	.. 4½	♍	9	45		1	30	
9	.. 4½	♍	20	20		2	40	
11	.. 5	♍	24	46		3	9	

But the last 2 observations will, in his opinion, differ the most; because when made he was in some doubt about the adjustment of his instruments; and the comet was then far advanced into the morning rays.

As his above-mentioned observations on the comet appeared too incorrect to undertake a calculation for ascertaining its path by the theory, he contented himself with effecting it by a construction. By this means he found on a figure, whose globular or spherical diameter was 13¼ Rhineland inches, as follows:

That the comet was in its perihelion the 21st of October, at 2 in the afternoon: the place of the perihelion 3° in Leo. The comet's distance in the perihelion from the sun was about 34 parts, of which 100 make the mean distance between the sun and the earth. The inclination of the comet's orbit with the ecliptic

13°; and the southern latitude of the perihelion also 13°: the ascending or north node ♌ $4^{\circ}\frac{1}{3}$ in Scorpio; and the comet's motion direct, or according to the order of the signs of the zodiac. On this supposition, he for some of the times of observations, estimated the apparent places of the comet, and found them as follows:

				Long.	Latit.	
Sept. 18,	at $3\frac{3}{4}$ ante merid.	In	♋	$18\frac{1}{12}$	and 9	deg. North.
19	.. 4		♋	22	.... $8\frac{2}{5}$	
22	.. $2\frac{3}{4}$		♌	$3\frac{5}{8}$	.... $6\frac{1}{4}$	
23	.. 4		♌	$7\frac{3}{5}$	.... $5\frac{1}{2}$	
25	.. $4\frac{1}{4}$		♌	$14\frac{2}{3}$	 4	
28	.. 4		♌	$24\frac{1}{3}$	.... $1\frac{3}{4}$	
Oct. 4	.. $4\frac{1}{2}$		♍	$9\frac{1}{3}$	 2	—— South.
9	.. $4\frac{3}{4}$		♍	$19\frac{2}{3}$	.... $3\frac{2}{5}$	
11	.. 5		♍	$23\frac{1}{8}$	.... $3\frac{4}{5}$	

The observations which he took to ground the measurement on, are those of the 16th and 23d of September, and of the 1st of October. It appears very evident, not only from this rough calculation, but every other circumstance of this comet, that it is not the same with that in the year 1682, nor none of those already calculated or brought upon a list, by Messieurs Halley and Struyk. It is somewhat remarkable, that the line of the nodes is almost at right angles with the longer axis of the ellipsis; which corresponds nearly with the comets of the years 1580, 1683, and 1686: but those had their perihelions northward of the ecliptic; whereas the perihelion of the last, which we have lately seen, was to the southward of the ecliptic.

LXI. On the Different Temperature of the Air at Edystone, from that observed at Plymouth, between the 7th and 14th of July 1757. By Mr. John Smeaton, F. R. S. p. 488.

Edystone is distant from Plymouth about 16 miles, and without the headlands of the sound about 11. The 7th and 8th were not remarkable at Edystone for heat or cold: the weather was very moderate, with a light breeze at east; which allowed them to work upon the rock both days, when the tide served.

About midnight, between the 8th and 9th, the wind being then fresh at east, it was remarkably cold for the season. The wind continued cold the 9th all day; and so continued till the 10th; when seeing no prospect of a sudden alteration of weather, Mr. S. returned to Plymouth in a sailing boat, wrapped up in his thick coat. As soon as they got within the headlands, he could perceive the wind to blow considerably warmer; but not so warm as to make his great coat uneasy. Having had a quick passage, in this manner he went home, to the great astonishment of the family to see him so wrapped up, when they were complaining of the excessive heat: and indeed, it was not long before he had reason to join in their opinion.

This heat he experienced till the 12th, when he again went off to sea, where he found the air very temperate, rather cool than warm; and so continued till the 14th.

It may not be amiss further to observe on this head, that once in returning from Edystone, having got within about 2 miles of the Ramhead, they were becalmed; and here they rolled about for at least 4 hours; and yet at the same time saw vessels not above a league from them, going out of Plymouth Sound with a fresh of wind, the direction of which was towards them, as they could observe from the trim of their sails; and as they themselves experienced, after they got into it by tacking and rowing.

Hence it appears, how different the temperature of the air may be in a small distance; and to what small spaces squalls of wind are sometimes confined.

LXII. Of the Earthquake felt in the Island of Sumatra, in the East Indies, in Nov. and Dec. 1756. *In a Letter from Mr. Perry, at Fort Marlborough, Feb.* 20, 1757. *Communicated by the Rev. William Stukeley, M. D., F.R.S.* p. 491.

The earthquake at Lisbon was certainly one of the most awful and tremendous calamities that has ever happened in the world. Its effects are extremely wonderful and amazing; and it seems to have been felt in all parts of the globe. On the 3d day of the same month the earthquake of Lisbon happened, Mr. P. himself felt at Manna * a violent shock; and from that time to the 3d of December following he felt no less than 12 different shocks. Since which we have had 2 very severe earthquakes, felt we believe throughout this island†. The walls of ‡ Cumberland-house were greatly damaged by them. Several houses, the houses of Laye|| and Manna, were all cracked by them; and the works at the sugar-plantation § received considerable damage. The ground opened near the qualloe¶ at Bencoolen, and up the river in several places; from which issued sulphureous earth, and great quantities of water, with a most intolerable stench. Poblo Point ** as much cracked at the same time; and some doosoons†† in-land at Manna were destroyed, and many people in them.

* Manna lies about 50 miles to the southward of Marlborough.—Orig.

† The island of Sumatra is between 7 and 8 hundred miles long, from north to south.—Orig.

‡ Cumberland house is a new well-built house for the governor of the place.—Orig.

|| Laye house or factory is about 30 miles to the northward of Marlborough, and Manna house or factory 50 miles to the southward.—Orig.

§ The sugar plantation is 5 or 6 miles from Marlborough.—Orig.

¶ The qualloe is the country word for a river's mouth.—Orig.

** Poblo Point lies about 3 leagues to the southward of Marlborough.—Orig.

†† Doosoons are villages.—Orig.

LXIII. *Concerning the Fall of Water under Bridges. By Mr. J. Robertson. F. R. S.* p. 492.

Some time before the year 1740, the problem about the fall of water, occasioned by the piers of bridges built across a river, was much spoken of at London, on account of the fall that it was supposed would be at the new bridge to be built at Westminster. In Mr. Hawksmore's and Mr. Labelye's pamphlets, the former published in 1736, and the latter in 1739, the result of Mr. Labelye's computations was given: but neither the investigation of the problem, nor any rules, were at that time exhibited to the public.

In the year 1742 was published Gardiner's edition of Vlacq's Tables; in which among the examples there prefixed to show some of the uses of those tables drawn up by the late William Jones, Esq. there are 2 examples, one showing how to compute the fall of water at London bridge, and the other applied to Westminster bridge: but that excellent mathematician's investigation of the rule, by which those examples were wrought, was not printed, though he communicated copies of it to several of his friends. Since that time, it seems as if the problem had in general been forgotten, as it has not made its appearance in any of the subsequent publications. As it is a problem somewhat curious, though not difficult, and its solution not generally known (having seen 4 different solutions, one of them very imperfect, extracted from the private books of an office in one of the departments of engineering in a neighbouring nation,) Mr. R. thought it might give some entertainment to the curious in these matters, if the whole process were published. In the following investigation, much the same with Mr. Jones's, as the demonstrations of the principles used appeared to be wanting, they are here attempted to be supplied.

PRINCIPLES.

1. A heavy body, that in the first second of time has fallen the height of a feet, has acquired such a velocity, that, moving uniformly with it, will in the next second of time move the length of $2a$ feet.

2. The spaces run through by falling bodies are proportional to one another as the squares of their last or acquired velocities.

These two principles are demonstrated by the writers on mechanics.

3. Water forced out of a larger channel through one or more smaller passages, will have the streams through those passages contracted in the ratio of 25 to 21. This is shown in the 36th prop. of the 2d book of Newton's Principia.

4. In any stream of water, the velocity is such, as would be acquired by the fall of a body from a height above the surface of that stream.

This is evident from the nature of motion.

5. The velocities of water through different passages of the same height, are reciprocally proportional to their breadths.—For, at some time the water must be delivered as fast as it comes; otherswise the bounds would be overflowed. At that time, the same quantity which in any time flows through a section in the open channel, is delivered in equal time through the narrower passages; or the momentum in the narrower passages must be equal to the momentum in the open channel; or the rectangle under the section of the narrower passages, by their mean velocity, must be equal to the rectangle under the section of the open channel by its mean velocity. Therefore the velocity in the open channel, is to the velocity in the narrower passages, as the section of those passages, is to the section of the open channel. But the heights in both sections being equal, the sections are directly as the breadths; consequently the velocities are reciprocally as the breadths.

6. In a running stream, the water above any obstacles put in it, will rise to such a height, that by its fall the stream may be discharged as fast as it comes.—For the same body of water, which flowed in the open channel, must pass through the passages made by the obstacles: and the narrower the passages, the swifter will be the velocity of the water: but the swifter the velocity of the water, the greater is the height from which it has descended: consequently the obstacles which contract the channel, cause the water to rise against them. But the rise will cease when the water can run off as fast as it comes: and this must happen when, by the fall between the obstacles, the water will acquire a velocity in a reciprocal proportion to that in the open channel, as the breadth of the open channel is to the breadth of the narrow passages.

7. The quantity of the fall, caused by an obstacle in a running stream, is measured by the difference between the heights fallen from to acquire the velocities in the narrow passages and open channel.—For just above the fall, the velocity of the stream is such, as would be acquired by a body falling from a height higher than the surface of the water: and at the fall, the velocity of the stream is such, as would be acquired by the fall of a body from a height more elevated than the top of the falling stream: consequently the real fall is less than this height. Now as the stream comes to the fall with a velocity belonging to a fall above its surface; consequently the height belonging to the velocity at the fall, must be diminished by the height belonging to the velocity with which the stream arrives at the fall.

Problem.—In a channel of running water, whose breadth is contracted by one or more obstacles; the breadth of the channel, the mean velocity of the whole stream, and the breadth of the water-way between the obstacles being given; to find the quantity of the fall occasioned by those obstacles.

Let b = breadth of the channel in feet.

c = breadth of the water-way between the obstacles.

v = mean velocity of the water in feet per sec.

Now $25 : 21 :: c : \frac{21}{25}c$ the water-way contracted; by princip. 3.

And $\frac{21}{25}c : b :: v : \frac{25b}{21c}v$ the veloc. per sec. in the water-way between the obstacles; by princip. 5.

Also $(2a)^2 : vv :: a : \frac{vv}{4a}$ the height fallen to acquire the vel. v; by 1 and 2.

And $(2a)^2 : (\frac{25b}{21c})^2 \times vv :: a : (\frac{25b}{21c})^2 \times \frac{vv}{4a}$ the height fallen to acquire the vel. $\frac{25b}{21c}v$; by 1 and 2.

Then $(\frac{25b}{21c})^2 \times \frac{vv}{4a} - \frac{vv}{4a}$ is the measure of the fall required; by 7.

Or $[(\frac{25b}{21c})^2 - 1] \times \frac{vv}{4a}$ is a rule, by which the fall is readily computed.

Here $a = 16{,}0899$ feet, and $4a = 64{,}3596$.

EXAMPLE I. *For London Bridge.*

By the observations made by Mr. Labelye in 1746,

The breadth of the Thames at London bridge is 926 feet.

The sum of the water-ways at the time of the greatest fall is 236 feet.

The mean velocity of the stream taken at its surface just above bridge is $3\frac{1}{6}$ feet per second.

Under almost all the arches there are great numbers of drip-shot piles, or piles driven into the bed of the water-way, to prevent it from being washed away by the fall. These drip-shot piles considerably contract the water-ways, at least $\frac{1}{6}$ of their measured breadth, or about $39\frac{1}{3}$ feet in the whole.

So that the water-way will be reduced to $196\frac{2}{3}$ feet.

Now $b = 926$; $c = 196\frac{2}{3}$; $v = 3\frac{1}{6}$; $4a = 64{,}3596$.

Then $\frac{25b}{21c} = \frac{23150}{4130} = 5{,}60532$.

And $5{,}60532^2 = 31{,}4196$; and $31{,}4196 - 1 = 30{,}4196 = (\frac{25b}{21c})^2 - 1$.

Also $vv = (\frac{19}{6})^2 = \frac{361}{36}$; and $\frac{vv}{4a} = \frac{361}{36 \times 64{,}3596} = 0{,}15581$.

Then $30{,}4196 \times 0{,}15581 = 4{,}739$ feet, the fall sought after.

By the most exact observations made about the year 1736, the measure of the fall was 4 feet 9 inches.

EXAMPLE II. *For Westminster Bridge.*

Though the breadth of the river at Westminster bridge is 1220 feet; yet, at the time of the greatest fall, there is water through only the 13 large arches, which amount to 820 feet: to which adding the breadth of the 12 intermediate piers, equal to 174 feet, gives 994 for the breadth of the river at that time: and

the velocity of the water just above bridge, from many experiments, is not greater than $2\frac{1}{4}$ feet per second.

Here $b = 994$; $c = 820$; $v = 2\frac{1}{4}$; $4a = 64{,}3596$.

Now $\frac{25\,b}{21\,c} = \frac{24850}{17220} = 1{,}443$.

And $1{,}443^2 = 2{,}082$; and $2{,}082 - 1 = 1{,}082 = (\frac{25\,b}{21\,c})^2 - 1$.

Also $vv = (\frac{9}{4})^2 = \frac{81}{16}$; and $\frac{vv}{4a} = \frac{81}{16 \times 64{,}3596} = 0{,}0786$.

Then $1{,}082 \times 0{,}0786 = 0{,}084$ feet, the fall sought. Which is about 1 inch; and is about half an inch more than the greatest fall observed by Mr. Labelye.

LXIV. Of the Earthquake in the West Parts of Cornwall, July 15, 1757. By the Rev. William Borlase, M. A., F. R. S. p. 499.

On Friday the 15th of July, 1757, a violent shock of an earthquake was felt in the western parts of Cornwall. The thermometer had been higher than usual, and the weather hot or calm, or both, for 8 days before; wind east and north-east. On the 14th in the morning, the wind shifting to the south-west, the weather calm and hazy, there was a shower. The afternoon hazy and fair, wind north-west. The barometer moderately high, but the mercury remarkably variable.

On the 15th in the morning, the wind fresh at north-west, the atmosphere hazy. Being on the sands, half a mile east of Penzance, at 10 A.M. near low water, Mr. B. perceived on the surface of the sands a very unusual inequality: for whereas there are seldom any unevennesses there, but what are made by the rippling of the water, he found the sands, for above 100 yards square, all full of little tubercles, each as large as a moderate mole-hill, and in the middle a black speck on the top, as if something had issued thence. Between these convexities were hollow basins of an equal diameter. From one of these hollows there issued a strong rush of water, about the thickness of a man's wrist, never observed there before nor since. About a quarter after 6, P.M. the sky dusky, the wind being at W.N.W., it fell quite calm. At half past 6, being then at Penzance, with some company, they were suddenly alarmed with a rumbling noise, as if a coach or waggon had passed near them over an uneven pavement; but the noise was as loud in the beginning and at the end as in the middle; which neither the sound of thunder or of carriages ever is. The sash-casements jarred: one gentleman thought his chair moved under him; and the gardener, then in the dwelling house (about 100 yards distant from them) felt the stone pavement of the room he was in move very sensibly.

The shock was not equally loud or violent. Its extent was from the isles of

Scilly eastward as far as Liskerd, and towards the north as far as Camelford. At Plymouth it was just sensible. In several places the houses shook, the windows rattled, the floors lifted, the chamber-bells rung, the pewter, &c. on the shelves were much agitated. Thus was this earthquake felt in towns, houses, and grounds adjacent; but still more particularly alarming in the mines, where there is less refuge, and consequently a greater dread from the tremors of the earth. In these it was heard and felt at all depths, to 70 fathoms or more, and seemed as below them.

LXV. On the Sleep of Plants; and of that Faculty which Linneus calls Vigiliæ Florum; with an Enumeration of several Plants which are subject to that Law. By Mr. Richard Pulteney of Leicester. p. 506.

Acosta and Prosper Alpinus, who both wrote near the conclusion of the 16th century, appear to be the first who recorded that nocturnal change in the leaves of plants, which has since been called somnus. It is an observation indeed as old as Pliny's time, that the leaves of trefoil assume an erect situation on the coming of storms. The same is observable of our wood-sorrel; and Linneus adds, of almost all plants with declinated stamina. In the trifolium pratense album C. B. or common white-flowered meadow-trefoil, it is so obvious, that the common people in Sweden remark, and prognosticate the coming of tempests and rain from it.

The examples of sleeping plants instanced by Alpinus are but few. That author says, it was common to several Egyptian species; but specifies only the Acaciæ, Abrus, Absus, Sesban, and the tamarind-tree. Cornutus some time afterwards remarked this property in the Pseudo-acacia Americana. From that time it has remained almost unnoticed, till Linneus, ever attentive to nature's works, discovered that the same affair was transacted in many other plants; and his observations have furnished us with numerous and obvious examples of it. Mr. Miller mentions it in the medicago arborea Linn. Sp. Pl. 778; and we may add to the list two other common plants not mentioned by Linneus: these are the phaseolus vulgaris, common kidney-bean; and the trifolium pratense purpureum majus, or clover-grass: in both which this nocturnal change is remarkably displayed. Doubtless the same property exists in numberless other species; and future observation will very probably confirm Dr. Hill's sentiment, that no plant or tree is wholly unaffected by it.

It is now more than 20 years since Linneus first attended to this quality in plants. In his Flora Lapponica, when speaking of the trifolium pratense album, as above-mentioned, he remarks, that the leaves of the mimosa, cassia, Bauhinia, Parkinsonia, Guilandina, and others in affinity with them, were subject to this change in the night-time: and he had then carried his observations so far, as to

find that heat and cold were not the cause of this quality; since they were alike influenced by it when placed in stoves, where the temperature of the air was always the same. The merit of reviving this subject is therefore due to the illustrious Swede; and the naturalist is greatly indebted to him for so far extending his observations on it.

The subject of the somnus plantarum cannot but be highly entertaining to the lovers of natural knowledge: and such, I apprehend, cannot be less entertained with that faculty which Linneus calls vigiliæ florum: of which we shall give a brief account.

Previous to our explanation of this affair it is proper to observe, that the flowers of most plants, after they are once opened, continue so night and day, until they drop off or die away. Several others, which shut in the night time, open in the morning either sooner or later, according to their respective situation in the sun or shade, or as they are influenced by the manifest changes of the atmosphere. There are however another class of flowers, which make the subject of these observations, which observe a more constant and uniform law in this particular. These open and shut duly and constantly at certain and determinate hours, exclusive of any manifest changes in the atmosphere; and this with so little variation in point of time, as to render the phenomenon well worth the observation of all whose taste leads them this way. This faculty in the flowers of plants is not altogether a new discovery; but we are indebted to the same hand for additional observations on this head also. It is so manifest in one of our common English plants, the tragopogon luteum, that our country-people long since called it John-go-to-bed-at-noon. Linneus's observations have extended to near 50 species, which are subject to this law. What we find principally on this subject, is in the Philosophia Botanica, p. 273. We will enumerate these plants, and mention the time when the flowers open and shut, that those who have opportunity and inclination may gratify themselves, and probably at the same time extend this branch of botanic knowledge still further.

It is proper to observe, that as these observations were made by Linneus in the academical garden at Upsal, whoever repeats them in this country will very probably find, that the difference of climate will occasion a variation in point of time: at least this will obtain in some species, as our own observations have taught us; in others the time has corresponded very exactly with the account he has given us. Whether this faculty has any connexion with the great article of fecundation in the economy of flowers, Mr. P. cannot determine: in the mean time it is not improbable. Future and repeated observations, and well-adapted experiments, will tend to illustrate this matter, and it may lead the way to a full explanation of the cause.

1. Anagallis flore phœniceo C. B. pin. 252. Raii Syn. p. 282. Anagallis ar-

vensis Lin. Spec. plant. p. 148. The male pimpernel. The flowers of this plant open about 8 o'clock in the morning, and never close till past noon. This plant is common in kitchen-gardens and in corn-fields; it flowers in June, and continues in flower 3 months.—2. The anagallis cærulea foliis binis ternisve ex adverso nascentibus C. B. pin. p. 252. Raii Hist. Plant. p. 1024. Anagallis monelli Sp. Plant. 148. Blue-flowered pimpernel with narrow leaves. The flowers of this plant observe nearly the same time in opening and shutting as the foregoing.—3. Convolvulus peregrinus cæruleus folio oblongo C. B. pin. 295. Convolvulus tricolor Sp. Plant. 158. Little blue convolvulus, or bind-weed. This opens its flowers between the hours of 5 and 6 in the morning, and shuts them in the afternoon.—4. Phalangium parvo flore ramosum C. B. pin. 29. Raii Hist. Pl. 1193. Branched spider-wort with a small flower. These open about 7 in the morning, and close between 3 and 4 afternoon.—5. Lilium rubrum asphodeli radice C. B. pin. 80. Hemerocallis fulvus Sp. Pl. 324. The day-lilly. The flowers open about 5 in the morning, and shut at 7 or 8 in the evening. —6. Plantago aquatica minor. Park. 1245. Raii Syn. 257. Alisma ranunculoides Sp. Pl. 343. Fl. Succ. 2, N° 325. The lesser water-plantain, during its flowering time, only opens its flowers each day about noon.—7. Caryophyllus sylvestris prolifer C. B. pin. 209. Raii Syn. 337. Dianthus prolifer Sp. Pl. 410. Proliferous pink. The flowers expand about 8 in the morning, and close again about 1 afternoon.—8. Spergula purpurea J. B. iii. 722. Raii Syn. p. 351. Arenaria rubra. Sp. Pl. 423. Purple spurrey. These expand between 9 and 10 in the morning, and close between 2 and 3 afternoon. This little plant is common among the corn in sandy soils, and flowers in June.—9. Portulaca latifolia sativa C. B. pin. 288. Portulaca oleracea Sp. Pl. p. 445. Common purslain, opens its flowers about 9 or 10 in the morning, and closes them again in about an hour's time.—10. Ficoides Africana, folio plantaginis undulato micis argenteis adsperso Boerh. Ludg. i. p. 291. Mesembryanthemum chrystallinum Sp. pl. 480. Diamond Ficoides. The flowers of this plant open at 9 or 10, and close at 3 or 4 afternoon.—11. Ficoides Africana folio tereti in villos radiatos abeunte. Tourn. Mesembryanthemum barbatum Sp. Pl. 482. The flowers of this species expand at 7 or 8 in the morning, and close about 2 afternoon.—12. Ficoides folio tereti Neapolitana flore candido Herm. Ludg. 252. Kali crassulæ minoris foliis C. B. pin. 289. Mesembryanthemum nodiflorum Sp. Pl. 480. The flowers of this plant open at 10 or 11 in the morning, and close at 3 afternoon. —13. Mesembryanthemum folio linguiformi latiore Dillen. Hort. Elth. Mesembryanthemum linguiforme Sp. Pl. 488. Ficoides with a tongue-shaped leaf. These open at 7 or 8 in the morning, and are closed about 3 afternoon.—14. Nymphæa alba J. B. iii. 770. Raii Syn. 368. Nymphæa alba Sp. Pl. 510. Fl. Suec. 2. N° 470. White water lily. This plant grows in rivers, ponds,

and ditches, and the flowers lie upon the surface of the water. At their time of expansion, which is about 7 in the morning, the stalk is erected, and the flowe more elevated above the surface. In this situation it continues till about 4 afternoon, when the flower sinks to the surface of the water, and closes again.—15. Papaver erraticum nudicaule flore flavo odorato Dillen. Hort. Elth. 302. Papaver nudicaule Sp. Pl. p. 507. Wild poppy with a naked stalk and a yellow sweet-smelling flower. The flower of this plant opens at 5 in the morning, and closes at 7 in the evening.—16. Alyssoides incanum, foliis sinuatis Tourn. Inst. 213. Alyssum sinuatum Sp. Pl. 651. Hoary madwort with sinuated leaves. The flowers of this plant expand between 6 and 8 in the morning, and close at 4 afternoon.—17. Abutilon repens alceæ foliis, flore helvolo Dillen. Hort. Elth. 5. Malva Caroliniana Sp. Pl. 688. Creeping Indian mallow with leaves like vervain mallow, and a flesh-coloured flower. These open at 9 or 10 in the morning, and close at 1 afternoon.—18. Tragopogon luteum Ger. 595. Raii Syn. 171. Tragopogon pratense Sp. Pl. 789. Yellow goats beard, or go-to-bed at-noon. The latter of these names was given to this plant long since, on account of this remarkable property. The flowers open in general about 3 or 4 o'clock, and close again about 9 or 10 in the morning. These flowers will perform their vigiliæ, if set in a phial of water, within doors for several mornings successively; and they are sometimes observed to be quite closed, from their utmost state of expansion, in less than a quarter of an hour. It flowers in June.—19. Tragopogon gramineis foliis, hirsutis. C. B. pin. 275. Raii Hist. Plant. 253. Rose-coloured goats beard. These open between 5 and 6 in the morning, and close about 11. Tragopogon hybridum Sp. Plant. 789. —20. Tragopogon, calycibus corolla brevioribus inermibus, foliis lyrato-sinuatis. Hort. Ups. 244. Sp. Pl. 790. Hall. Hort. Gotting. 2. p. 419. The flowers of this plant open at 6 or 7 in the morning, and shut between the hours of 12 and 4 afternoon.—21. Sonchus Tingitanus papaveris folio. Tourn. Raii Suppl. 137. Scorzonera Tingitana Sp. pl. 791. African sowthistle with a poppy leaf. This plant opens its flowers between 4 and 6 in the morning, and closes them in about 3 hours.—22. Sonchus repens, multis hieracium majus J. B. ii. 1017. Raii Syn. 163. Sonchus arvensis Sp. Pl. 793. Tree sowthistle. These flowers expand about 6 or 7, and close between 11 and 12 in the forenoon. This is common in cornfields, and flowers in June, July, and August.—23. Sonchus lævis Ger. Raii Syn. 162. Sonchus oleraceus Sp. Pl. 794. Smooth or unprickly sowthistle, hares lettuce. These open about 5 in the morning, and close again at 11 or 12.—24. Sonchus lævis laciniatus cæruleus C. B. pin. 124. Raii Hist. pl. 225. Sonchus alpinus Sp. Pl. 794. Blue-flowered mountain sowthistle. These open about 7, and close about noon.—25. Sonchus tricubitalis, folio cuspidato Morr. pin. Raii Syn. 163. Sonchus asper arborescens C. B. pin.

124. Sonchus palustris Sp. Pl. 793. The greatest marsh-tree sowthistle. It expands its flowers about 6 or 7, and closes about 2 afternoon.—26. Lactuca sativa C. B. pin. 122. Sp. Pl. 795. Garden lettuce, opens its flowers about 7, and closes them about 10 forenoon.—27. Dens leonis Ger. 228. Raii Syn. 170. Leontodon taraxacum Sp. Pl. 798. Dandelion. It expands at 5 or 6, and closes at 8 or 9 in the forenoon. This flowers early in the spring, and again in the autumn.—28. Dens leonis hirsutus leptocaulos, hieracium dictus. Raii Syn. 171. Leontodon hispidum Sp. Pl. 799. Rough dandelion, or dandelion hawkweed. This plant opens its flower about 4 in the morning, and keeps it expanded till 3 afternoon. In May.—29. Hieracium minus præmorsa radice. Park. 794. Raii Syn. 164. Leontodon autumnale. Sp. Pl. 799. Hawkweed with bitten roots, or yellow Devil's-bit. The flowers open about 7, and keep in an expanded state till about 3 afternoon. It flowers in July and August.—30. Pilosella repens Ger. 573. Raii Syn. 170. Hieracium pilosella Sp. Pl. 800. Common creeping mouse-ear. It opens about 8 in the morning, and closes about 2 afternoon. Very common on dry pastures, flowering in June and July. —31. Hieracium murorum folio pilosissimo C. B. pin. 129. Raii Syn. 168. Hieracium murorum Sp. Pl. 802. The flowers of this plant expand about 6 or 7, and close about 2 in the afternoon. Upon old walls, flowering in June and July. This is called in English French or golden lungwort.—32. Hieracium fruticosum angustifolium majus. C. B. pin. 129. Hieracium umbellatum Sp. Pl. 804. Narrow-leaved bushy hawkweed. The flowers of this species expand about 6 in the morning, and remain open till 5 afternoon.—33. Hieracium fruticosum latifolium hirsutum C. B. pin. 129. Raii Syn. p. 167. Hieracium sabaudum Sp. Pl. 804. Bushy hawkweed with broad rough leaves. These flowers are in their expanded state from about 7 in the morning till 1 or 2 afternoon. In woods, flowering in June and July.—34. Hieracium montanum cichorii folio. Raii. Syn. p. 166. Hieracium paludosum Sp. Pl. 638. Fl. Suec. 2. N° 702. Succory-leaved mountain hawkweed. The flowers expand about 6 in the morning, and close about 5 afternoon.—35. Hieracium hortense floribus atro-purpurascentibus C. B. pin. 128. Hieracium aurantiacum Sp. Pl. 801. Garden hawkweed with deep purple flowers, or sweet Indian mouse-ear. The flowers are in their expanded state from 6 or 7 in the morning till 3 or 4 afternoon.—36. Hieracium luteum glabrum, sive minus hirsutum. J. B. Raii Syn. 165. Crepis tectorum Sp. Pl. 807. Smooth succory hawkweed. The flowers of this plant expand about 4 in the morning, and close about noon.—37. Hieracium alpinum scorzoneræ folio. Tourn. Inst. 472.—Crepis alpina Sp. Pl. 806. Mountain hawkweed with a vipers-grass leaf. These open about 5 or 6, and close at 11 in the forenoon.—38. Hieracium dentis leonis folio, flore suave-rubente, C. B. pin. 127. Raii Hist. pl. 231. Crepis rubra Sp. Pl. 806. Hawkweed of Apulia with

a flesh-coloured flower. The flowers remain in their expanded state from 6 or 7 in the morning till 1 or 2 afternoon.—39. Hieracium echioides, capitulis cardui benedicti C. B. pin. 128. Raii Syn. 166. Picris echioides Sp. Pl. 792. Langue de bœuf. On banks about hedges, and about the borders of fields, flowering in August. These expand about 4 or 5 in the morning, and never close before noon: sometimes they remain open till 9 at night.—40. Hieracium alpinum latifolium hirsutie incanum flore magno. C. B. pin. 128. Raii Syn. p. 167. Hypochæris maculata Sp. Pl. 810. Broad-leaved Hungarian hawkweed. These flowers are in their vigilating state from 6 in the morning till 4 afternoon.—41. Hieracium ramosum, floribus amplis, calycibus valde hirsutis, foliis oblongis obtusis: dentibus majoribus inæqualibus incisis Raii Suppl. 144. 76. Hypochæris Achyrophorus Sp. Pl. 810. This plant opens its flowers about 7 or 8 in the morning, and closes them about 2 afternoon.—42. Hieracium minus dentis leonis folio, oblongo glabro C. B. pin. 127. Hypochœris glabra Sp. Pl. 811. These expand about 9 in the morning, and close about 12 or 1 o'clock.—43. Hieracium falcatum alterum Raii Hist. 256. Lapsana calycibus fructus undique patentibus, radiis subulatis, foliis lyratis Hort. Ups. 245. Sp. Pl. 812. The flowers open at 5 or 6, and close between the hours of 10 and 1.—44. Hedypnois annua Tourn. Inst. 478. Hyoseris hedypnois Sp. Pl. 809. The flowers open at 7 or 8, and close again at 2 afternoon.—45. Hieracium montanum alterum leptomacrocaulon Col. Raii Hist. 234. Lapsana chondrilloides Sp. Pl. 812. Mountain hawkweed with long slender stalks and small flowers. The flowers are in their expanded or vigilating state from 5 or 6 in the morning till about 10.—46. Cichoreum sylvestre Ger. em. 284. Raii Syn. 172. Cichorium Intybus Sp. pl. 813. Wild succory. On the borders of fields, flowering in August and September. The flowers open about 8 in the forenoon, and keep expanded till about 4 afternoon.—47. Calendula arvensis C. B. pin. 275. Raii Hist. 338. Calendula officinalis Sp. Pl. 921. Wild marigold. The flowers expand from 9 in the morning till 3 afternoon.—48. Calendula foliis dentatis Roy. Ludg. 177. Miller, p. 50, tab. 75, f. 1. Calendula pluvialis Sp. Pl. 921. Marigold with indented leaves. The flowers expand from 7 in the morning till 3 or 4 afternoon. Linneus observes of this plant, that if its flowers do not expand about their usual time in the morning, it will almost assuredly rain that day: with this restriction indeed that the plant is not affected by thunder-showers. Phil. Bot. 275.—49. Sonchus pedunculis squamatis, foliis lanceolatis indivisis sessilibus. Hort. Upsal. 244. Flor. Suec. 2, N° 690. Lactuca salicis folio, flore cæruleo. Amman. ruth. 211. Of this plant it is remarked, that whenever the flowers are in the expanded state in the night-time, the following day generally proves rainy.

LXVI. The Case of a Boy troubled with Convulsive Fits cured by the Discharge of Worms. By the Rev. Richard Oram, M. A. p. 518.

Joseph Postle, of Ingham in Norfolk, was subject to convulsive fits from his infancy, which were common and tolerable till he was about 7 years of age. At that time they began to attack him in all the varieties that can be conceived. Sometimes he was thrown on the ground: sometimes he was twirled round like a top by them: at others he would spring upwards to a considerable height, &c. and once he leaped over an iron bar, placed purposely before the fire to prevent his falling into it. He was much burned, but had been rendered so habitually stupid by his fits, that he never expressed the least sense of pain after this accident. His intellect was so much impaired, and almost destroyed, by the frequency and violence of his fits, that he scarcely seemed to be conscious of any thing, except that he was very voracious, and would frequently call for something to eat. There is no kind of filth which he would not eat or drink without distinction. He was much emaciated, and his body so distorted, that he was quite a cripple. His parents consulted a physician, who very judiciously considered his disorder as a worm case, and prescribed accordingly, but without success, being afraid to give too violent medicines to the boy. It was observed, that the disorder varied, and became worse at certain periods of the moon.

In these circumstances the boy continued to languish till he was about 11 years of age (July 1757), when he accidentally found a mixture of white lead* and oil, which had some time before been prepared for some purpose of painting, set by on a shelf, and placed, as it was thought, out of his reach. There was nearly ½ a pint of this mixture when he found it; and, as he did not leave much, it is thought he swallowed about ¼ of a pint of it. There was also some lamp black in the composition.

This began soon to operate, by vomiting and purging him for near 24 hours most violently. A large quantity of black matter was discharged; and an infinite number of worms, almost as small as threads, were voided. These operations were so intense, that his life was despaired of. But he not only survived them, but experienced a most wonderful change and improvement after them; for he daily got better, from the time of his drinking the mixture, both in body and mind. He became rather corpulent, his appetite moderate, and his body erect. His understanding too was equally benefited.

On the same Subject, in a Letter from Mr. John Gaze, of Walket in Norfolk. p. 521.

This letter confirms the former account with scarcely any variation.

* It is not improbable, that a considerable portion of whiting might be used instead of pure white lead, which is frequently done; and this supposition is favoured by the mixtures not proving fatal to the boy, as such a quantity of white lead in all probability would.—Orig.

LXVII. On the Extraordinary Heat of the Weather in July 1757, and its Effects. By John Huxham, M. D., F. R. S. p. 523.

By accounts it appears that the heat at London was not so great in the beginning of July 1757, as at Plymouth by 2 or 3 degrees of Fahrenheit's thermometer. We had again, after much rain at the close of the month, and in the beginning of August, excessive heat, viz. on the 8th, 9th, and 10th of August, which mounted the mercury in that thermometer to 85; nay, on the 9th to near 86. Dr. H. never before remembered the mercury in that thermometer to exceed 84.

The consequences of this extremely hot season were hæmorrhages from several parts of the body; the nose especially in men and children, and the uterus in women. Sudden and violent pains of the head and vertigo, profuse sweats, great debility and oppression of the spirits, affected many. Putrid fevers in great abundance; and a vast quantity of fluxes of the belly, both bilious and bloody, with which the fevers also were commonly attended. These fevers were always ushered in by severe pains of the head, back, and stomach; vomitings of green and sometimes of black bile, with vast oppression of the præcordia, continual anxiety, and want of sleep. These were soon succeeded by tremores tendinum, subsultus, delirium, or stupor. The pulse was commonly very quick, but seldom tense or strong; was sometimes heavy and undose. The blood oftentimes florid, but loose; sometimes livid, very rarely sizy: in some however, at the very attack, it was pretty dense and florid. The tongue was generally foul, brown, and sometimes blackish, and towards the crisis often dry. The urine was commonly high coloured, and in small quantity; frequently turbid, and towards the end deposed a great deal of lateritious sediment. A vast number were seized with this fever, during, and soon after, the excessive heats; though but few died in proportion. Long and great heats always very much exalt the acrimony of the bilious humours; of which we had this summer abundant instances. Bleeding early was generally beneficial; profuse, always hurtful, especially near the state of the fever.

LXVIII. On the Fossil Thigh-bone of a large Animal, dug up at Stonesfield, near Woodstock, in Oxfordshire. By Mr. Joshua Platt. p. 524.

This was the thigh-bone of a large animal, probably belonging to the same creature with the vertebræ formerly found in the same place by Mr. P. in a slate-stone pit at Stonesfield, near Woodstock. The bone, and stone, in which it was bedded, weighed no less than 200 lb.

The bone was 29 inches in length; its diameter, at the extremity of the 2 trochanters, was 8 inches; at the lower extremity the condyles formed a surface

of 6 inches. Both the extremities appeared to be a little rubbed by the fluctuating water, in which Mr. P. apprehended it lay some time before the great jumble obtained, which brought it to the aforesaid place; and whence he imagined it to have been part of a skeleton before the flood. For if it had been corroded by any menstruum in the earth, or during the great conflux of water before the draining of the earth, it must have suffered in other parts as well as at each end, but as the extremities only are injured, such a partial effect can be attributed to the motion of the water only, which caused it to rub and strike against the sand, &c.

The small trochanter was broken in lifting it out of the hamper, in which it was brought to Mr. P.; but not unhappily; since all the cancelli were by that means discovered to be filled with a sparry matter, that fixed the stone of the stratum, in which it lay. The outer coat or cortex was smooth, and of a dusky brown colour, resembling that of the stone, in which it was bedded. One-half of the bone was buried in the stone: yet enough of it was exposed to show that it was the thigh-bone of an animal of greater bulk than the largest ox. He had compared it with the recent thigh-bone of an elephant; but could observe little or no resemblance between them. If he might be allowed to assume the liberty, in which fossilists are often indulged, and to hazard a vague conjecture of his own, he would say it might probably have belonged to the hippopotamus, to the rhinoceros, or to some such large animal, of whose anatomy we have not yet a competent knowledge.

The slate-pit, in which this bone was found, was about $\frac{1}{4}$ of a mile north-west from Stonesfield, on the declivity of a rising ground, the upper stratum of which was a vegetable mould about 8 or 10 inches thick; under this was a bed of rubble, with a mixture of sand and clay, very coarse, about 6 feet deep, in which were a great number of anomiæ both plain and striated, and many small oblong oysters, which the workmen called the sickle-oyster, some of them being found crooked, and bearing some resemblance to that instrument; but all differing from the curvi-rostra of Moreton. Immediately under this stratum of rubble was a bed of soft grey stone, of no use; but containing the echini ovarii, with great mamillæ, the clypeati of different sizes, all well preserved; and also many anomiæ and pectines. This bed, which was about 7 or 8 feet in depth, lay immediately above the stratum of stone, in which the bone was found.

This stratum was never wrought by the workmen, being arenarious, and too soft for their use. It was about 4 or 5 feet thick, and formed a kind of roof to them, as they dug out the stone, of which the slates were formed; for they worked these pits in the same manner as they do the coal-pits, leaving pillars at proper distances to keep their roof from falling in. This last bed of slate-stone was about 5 feet in depth, and lower than this they never dug. So that the whole

depth of the pit amounted to about 24 or 25 feet. It was by working out the slate-stone, that this bone was discovered sticking to the roof of the pit, where the men were pursuing their work; and with a great deal of caution, and no less pains, they got it down entire, but attached to a large piece of stone.

There was no water in the works, but such as descended from the surface through perpendicular fissures; and the whole was spent in forming the stalactites and stalagmites, of which there was great variety, and constantly increasing of dimensions. One of the workmen had been so curious, as to mark the time of the growth of some of them for several years past.

LXIX. On the Usefulness of Inoculation of the Horned Cattle to prevent the Contagious Distemper among them. By Dan. Pet. Layard, M. D., F. R. S. p. 528.

In this paper Dr. L. endeavours to prove that the contagious distemper which raged among the horned cattle of this country and France in 1745, 1746, 1747, and 1748, and other years, bore a great analogy to the small-pox, and that the cattle never take it a second time; in both which opinions he differs from the Marquis de Courtivron, author of 2 memoirs on this subject, read before the Royal Academy of Sciences at Paris. Hence he proposes inoculation, as the means of lessening the mortality of this distemper.

As to the nature, rise, progress, and fatality of this distemper at Issurtille, says Dr. L. it appeared to be the same disease as raged in these kingdoms. All the symptoms agreed as described by Rammazini, Lancisi, the Marquis de Courtivron, and in Dr. L.'s Essay. A distempered beast gave rise to the 3 infections. The illness was everywhere the same in Italy, France, and Britain; and either terminated fatally on the 4th or 5th day, when a scouring prevented the salutary eruptions, or in some cases by abortion; and on the 7th or 9th favourably, when the pustules had regularly taken their course. Though the Marquis did not observe, that any particular medicines were of use, he says, that in general acids were beneficial, especially poor thin wines somewhat sour; and that the distempered beasts were all fond of these acids. The fatality was likewise the same, as will appear from the Marquis's tables. Of 192 head of cattle, 176 died. The mortality was chiefly among the fat cattle, or cows with calf, and young sucking or yearling calves; and of the surviving 16, only 2 calves out of 77 lived, and these 2, with 7 other beasts of the 16, escaped the infection, though constantly among the diseased.

The mortality was as considerable in these kingdoms. Whoever will compare the appearances, progress, and fatality of the small-pox, with what is remarked by authors of authority, as Rammazini and Lancisi, and other observers, relative to the contagious distemper among the horned cattle, will not be at a loss

one moment to determine, whether this disease be an eruptive fever, like the small-pox, or not.

Now if, as the Marquis has granted in both his memoirs, it be a general observation, that an eruption of pustules on some parts of the body, regularly thrown out, digested, and dried, was the means used by nature to effect the cure, and that in general the morbid matter did not affect the parotid, inguinal, or other glands, nor produce large carbuncles and abscesses, as the plague does: nay more, since it was observed by the Marquis, that the difference between the contagious distemper of 1745 and 1746, and of 1747 and 1748, was, that in the former the salutary eruptions appeared, but in the latter were, as he justly apprehended, checked by the excessive cold weather: and should it appear, that by inoculation the same regular eruptive fever had been produced, with every stage, and the same symptoms as arise in the small-pox; the nature of this distemper will then be ascertained. Dr. L. then proceeds to state the accounts he had received relative to the infection and inoculation of the cattle, and to offer some observations on the experiments made at Issurtille.

So long as the distemper had raged in Great Britain, not one attested proof had been brought of any beast having this disease regularly more than once. He made no doubt but these creatures might be liable to eruptions of different kinds; but as all sorts of eruptions, says Dr. Mead, are not the small-pox nor measles, so every pustule is not a sign of the plague. Through ignorance, or fraud, persons might have been deceived in purchasing cattle, and have lost them, as well in England as in the provinces of France mentioned by the Marquis; but until a second infection be proved, the general opinion must prevail in this case, as in the small-pox; for though many have insisted on the same thing with regard to the small-pox, yet a single instance, properly vouched and attested, had never been produced, either after recovery from the natural way, or from inoculation; unless what was frequently the case with nurses and others attending the small-pox, that is, pustules breaking out in their arms and face, be allowed as the signs of a second infection.

The farmers and graziers in Huntingdonshire, Cambridgeshire, Lincolnshire, Kent, and Yorkshire, whence Dr. L. had written testimonies, all agreed that they never knew of a beast having the contagious distemper more than once. In the first particularly, Mr. J. Mehew, the farmer mentioned in Dr. L.'s essay, had then among his stock at Godmanchester 8 cows, which had the contagious distemper the first time it appeared in Godmanchester in 1746. It returned in 1749, 1755, and 1756; the 2 last not so generally over the town as the 2 former years. All these 4 times Mr. Mehew suffered by the loss of his cattle; yet those 8 cows, which recovered in 1746, remained all the while the distemper was in the farm the 3 years it raged, were in the midst of the sick

5

cattle, lay with them in the same barns, eat of the same fodder, nay of such as the distempered beasts had left and slabbered on, drank after them, and constantly received their breath and steams, without ever being in the least affected. Was not this a convincing proof? If in general the cattle be susceptible of a 2d infection, how eame it, that not one of these 8 cows were affected?

In the years abovementioned the distemper spared no beast, but such as had recovered from that disease; and this was confirmed to Dr. L. by Mr. Mehew's father and brother, all the chief farmers of Godmanchester, and was the opinion of all the farmers and graziers in Huntingdonshire, who were so thoroughly convinced of there being no 2d infection, that they were always ready to give an advanced price for such cattle as had recovered from the contagious distemper.

The Rev. Mr. Scaife, assistant to the Rev. Dr. Greene, Dean of Salisbury, in his parish of Cottenham, Cambridgeshire, acquainted Dr. L. that the farmers in that neighbourhood lost, in 1746 and 1747, 1200 head of cattle; in 1751 470; and told him that Messrs. Ivett, Sayers, Moor, Dent, Lawson, chief farmers at Cottenham, Messrs. Taylor, Sumpter, and Matthews, of his own parish of Histon, and the farmers of Wivelingham alias Willingham, unanimously declared they never had one instance of a beast having the distemper twice. Mr. Thorpe, a farmer and grazier near Gainsborough in Lincolnshire, had had beasts recovered from the distemper, which had herded with cattle fallen ill afterwards, and never met with a single instance of a 2d infection. Mr. Loftie, an eminent surgeon at Canterbury, had inquired for Dr. L. of the farmers and graziers in that part of Kent, and about Romney-Marsh; and whence no belief of a 2d infection can be had. The Rev. Dr. Fountayne, Dean of York, wrote to him, that no beast had been known, in his neighbourhood, to have the distemper twice. And several persons from that county, and others, had told him the same thing.

If the above testimony of persons of character and veracity, together with the concurrent persuasion of farmers in general be allowed of, it must be determined, that there is no instance of a 2d infection. Supposing now it should appear that this distemper is regularly, as in the natural way, though in a milder manner, produced by inoculation, and that inoculation secures a beast also from a 2d infection; then undoubtedly inoculation will be recommendable. The very few trials made in England, and those not with the greatest exactness or propriety, will yet serve to put this matter out of all doubt. The Rev. Dean of York had 5 beasts inoculated, by means of a skein of eotton dipped in the matter, and passed through a hole like a seaton in the dew-lap. Of these 5, one cow near the time of calving died: the other 4 after going through the several stages of this contagious disease reeovered; 2 of which being cows young with calf, did not slip their calves. All 4 herded with distempered cattle a long while, and

never had the least symptom of a 2d infection. Mr. Bewley, a surgeon of reputation in Lincolnshire, inoculated 3 beasts 2 years old, for Mr. Wigglesworth of Manton, in the dew-lap, and with mucus from the nostrils. All 3 had the regular symptoms of the contagious distemper in a mild manner, recovered, and though they herded a 12 month after with 5 or 6 distempered beasts, they never were the least affected. Mr. Bewley also declared to Mr. Thorpe, that there never was one instance produced that he knew of, of a 2d infection.

Since it was plain, that notwithstanding neither well-digested pus was made use of, nor incisions made in the properest places, and it might be supposed few medicines were given; yet inoculation succeeded so as to bring on the distemper in a regular and mild manner, as appeared by the cows with calf not slipping their calves; one might fairly conclude, that in this contagious distemper, like the small-pox, the practice of inoculation was not only warrantable but much to be recommended.

But how came it then, that neither by application, digestion, nor inoculation, the distemper was not communicated in France? The Marquis says, that this distemper was not communicated but from one beast to another immediately. Dr. L. begged leave to say, that to his knowledge the distemper in February 1756 was carried from the farm yard, where he visited some distempered cattle, to 2 other farm yards, each at a considerable distance, without any communication of the cattle with each other, and merely by the means of servants going to and fro, or of dogs. The experiments made on 4 beasts, by tying over their heads part of distempered hides, or pieces of linen and woollen cloth or silk, which had received the breath and steams of dying cattle, serve to show, by the bullock's forcing off the cloth tied about him, that the putrid stench was disagreeable to him; but that neither his blood nor that of the other 3 beasts, was then in a state to receive the infection.

With regard to the pustules, which the Marquis relates were mixed with oats and bran, or dissolved in white wine; the distempered bile, which was mixed with milk; milk taken from diseased cows; water, in which part of a distempered hide had been steeped; and the precaution taken to force these mixtures into the paunch of calves by means of a funnel, whose end was covered with a piece of raw distempered skin, that the beast might both swallow and suck in the disease; all these experiments could have no other effect than what followed; which was, that the acrimony of the distempered bile created first a nausea, and then produced a violent scouring, which killed the beast, leaving marks of its irritation on the intestines.

The practice of inoculation was but lately followed, and even now but little known in the provinces of France. Its advantages had not long since been strangely disputed at Paris. In the case of inoculating cattle, instead of a slip of

raw hide taken from a beast just dead, or putting a pustule into the neck, they should either have passed in the dewlap cotton or silk dipped in well digested pus, or have inserted in proper incisions cotton-thread or silk soaked with pus, either on the shoulders or buttocks; the true way of inoculating in the English manner. Some persons have indeed thought, that to inoculate with the blood of the infected would answer the intention; but most of the modern practitioners chuse to depend on digested matter.

Several constitutions would not receive infection, though inoculated ever so judiciously. A Ranby, a Hawkins, a Middleton, and other inoculators, would tell us that the incisions had sometimes suppurated much, and pustules had appeared round the edges of the wound without any other particular marks of the disease; and yet the patient had never had the small-pox afterwards. The Marquis mentioned an instance somewhat of the same kind in his first Memoir, p. 147.

LXX. Trigonometry Abridged. By the Rev. Patrick Murdoch, A.M., F.R.S. p. 538.

The cases in trigonometry, that can properly be called different from one another, are no more than 4; which may be resolved by 3 general rules or theorems, expressed in the sines of arcs only; using the supplemental triangle as there is occasion.

Case 1. When of three given parts two stand opposite to each other, and the third stands opposite to the part required. Then,

Theorem 1. The sines of the sides are proportional to the sines of angles opposite to them.

Cases 2 *and* 3. When the three parts are of the same name. And, when two given parts include between them a given part of a different name, the part required standing opposite to this middle part. Then,

Theorem 2. Let s and s be the sines of two sides of a spherical triangle, d the sine of half the difference of the same sides, a the sine of half the included angle, b the sine of half the base; and writing unity for the radius, we have $\mathrm{s}sa^2 + d^2 - b^2 = 0$; in which a or b may be made the unknown quantity, as the case requires.

Note. 1. If this, or the preceding, be applied to a plane triangle, the sines of the sides become the sides themselves; the triangle being conceived to lie in the surface of a sphere greater than any that can be assigned.

Note 2. If the two sides be equal, d vanishing, the operation is shorter: as it also is when one or both sides are quadrants.

Note 3. By comparing this proposition with that of the Lord Napier, which

makes the 39th of Keill's Trigonometry, it appears, that if AC, AM, be two arcs, then sin. $\frac{1}{2}$ (AC + AM) × sin. $\frac{1}{2}$ (AC − AM) $= (b + d) \times (b - d) =$ (sin. $\frac{1}{2}$ AC + sin. $\frac{1}{2}$ AM) × (sin. $\frac{1}{2}$ AC − sin. $\frac{1}{2}$ AM. And in the solution of Case 2, the first of these products will be the most readily computed.

Case 4. When the part required stands opposite to a part which is also unknown: having from the data of Case 1 found a 4th part, let the sines of the given sides be S, s; those of the given angles Σ, σ; and the sines of half the unknown parts a and b; and we shall have, as before, $Ssa^2 + d^2 - b^2 = 0$; and if the equation of the supplements be $\Sigma\sigma\alpha^2 + \delta^2 - \beta^2 = 0$; then, because $\alpha^2 = 1 - b^2 = 1 - (Ssa^2 + d^2)$, and $\beta^2 = 1 - a^2$, substituting these values in the 2d equation, we get

Theorem 3. $\frac{1 - \Sigma\sigma \times (1 - d^2) - \delta^2}{1 - Ss\Sigma\sigma} = a^2$; in words thus:

Multiply the product of the sines of the two known angles by the square of the cosine of half the difference of the sides: add the square of the sine of half the difference of the angles; and divide the complement of this sum to unity, by the like complement of the product of the 4 sines of the sides and angles; and the square root of the quotient shall be the sine of half the unknown angle.

If we work by logarithms the operation will not be very troublesome; but the rule needs not be used, unless when a table of the trigonometrical analogies is wanting. To supply which, the foregoing theorems will be found sufficient, and of ready use; being either committed to memory, or noted down on the blank leaf of the trigonometrical tables.

These theorems Mr. Murdoch demonstrates.

LXXI. Two extraordinary Cases of Gall-stones. By James Johnstone, M.D. of Kidderminster.* p. 543.

The calculus voided in the 1st of these cases was of a pyriform shape, resembling the form of the gall-bladder. It was smooth and polished, except at

* For the following particulars respecting the author of the above paper, we are indebted to Dr. John Johnstone, an eminent physician at Birmingham, one of Dr. James Johnstone's sons. We regret that the limited space allotted to biographical notices would not admit of our inserting more than an abstract from Dr. John Johnstone's more extended and highly interesting account of the life and writings of his late father.

James Johnstone, M.D. who practised physic for half a century with great reputation at Worcester, and distinguished himself by several ingenious works, was the 4th son of J. J. Esq. of Galabank, an ancient branch of the Johnstones of Johnstone. He was born at Annan, April 14, 1730, and in that town imbibed the rudiments of his scholastic education, under the Rev. Dr. Robert Henry, afterwards celebrated for his History of Great Britain. He studied physic at Edinburgh under the elder Monro, and the Professors Whytt, Rutherford, St. Clair, and Plummer, and there was admitted doctor in

the base. In length it measured $1\frac{3}{10}$ of an inch. It floated on water, and weighed about 126 grs. In the other case a number of small biliary calculi were

that faculty in June 1750, publishing an inaugural dissertation "De Aeris Factitii Imperio, in Primis Corporis Humani Viis."

His family thinking him too young, being then but in his 20th year, to settle as a practical physician immediately after taking his degree, he went to Paris, and there perfected himself in anatomy and chemistry under Ferrein and Rouelle.

In 1751 Dr. Johnstone seated himself at Kidderminster; and here he was assisted by the good offices of Dr. M'Kenzie, who had retired from practice at Worcester, and by the advice of his former masters Dr. St. Clair and Dr. Whytt. With both these eminent physicians he kept up a regular correspondence, and from both he received the warmest tokens of friendship. At Kidderminster he soon became popular; even the first year of his practice, at the age of 21, he got near £100, and never afterwards had any occasion to apply to his father for money. It was here that he met with the case of gall-stones, inserted in the present vol. of these Trans.

From its low situation, and from the population being too much crowded in small habitations, malignant fevers and sore throats were often prevalent at Kidderminster, and had proved remarkably fatal; and Dr. Johnstone owed much of his reputation to his success in curing them. He gave mineral acids, Peruvian bark, and dulcified acids. He forbad bleeding. He discovered the power of mineral acids in a gaseous state to destroy putrid miasmata, and on his observations and experience he founded the method of cure of putrid fevers, which he published in 1758 in a book entitled "An historical Dissertation concerning the malignant Epidemical Fever of 1756, &c." London, printed 1758. On the discovery of the power of mineral acids in a state of gas to destroy contagions, Dr. John Johnstone has written a pamphlet* asserting and proving his father's prior claim to the discovery. This pamphlet was answered by Dr. Smyth, in his letter to Mr. Wilberforce. The reply of Dr. John Johnstone to Dr. James Carmichael Smyth, has very fairly stated and settled the point in dispute. For if the question is to be settled by dates, it is proved that Dr. James Johnstone, sen. promulgated his use of muriatic acid gas in 1758. That Guyton Morveau recommended and used muriatic acid gas for purifying the cathedral at Dijon in 1773. That Dr. James Johnstone, jun. in his Treatise on Malignant Angina, published at Worcester in 1779, recommended muriatic acid gas as the surest method to prevent the spread of contagion, and that Dr. James Carmichael Smyth professes not to have discovered the virtues of nitric acid gas till 1780, and did not publish what he professes to have discovered till 1795.

At Kidderminster, in a very early part of his practice, began Dr. Johnstone's acquaintance with the first lord Lyttelton, with whom he continued on terms of uninterrupted friendship until the death of that learned and pious nobleman. The account he has given of his last illness, under the denomination of "Case of George Lord Lyttelton" is an example of the vigour and purity of his style, and does equal honour to his head and to his heart.

In the 54th vol. of the Philosophical Transactions Dr. Johnstone published the first sketch of his opinions on the ganglions of the nerves, a subject which he afterwards pursued in the 57th and 60th vols. The publication of these papers procured their author the notice and friendship of the illustrious Haller; with whom Dr. Johnstone's correspondence began in 1764, and continued till 1775. It consists chiefly of physiological and critical observations on the doctrine of ganglions, to which he very candidly offers objections and admits of reply. In one letter, dated 25 May 1769, he says, after some prefatory observations on Dr. Johnstone's doctrine—"For any thing I know, there is but one objection (the ophthalmic ganglion), which lies entirely between nerves dedicated to

* Account of the Discovery of the Power of Mineral Acids in a State of Gas to destroy Contagion. London, 1803.

voided by a woman who had long laboured under the jaundice, from a small sore at the pit of the stomach, at the part where she had felt a pain and hardness about a quarter of a year before. For a more particular account of these cases the reader is referred to this author's collected works, entitled Medical Essays and Observations.

voluntary motion. I shall look for some opportunity of showing you my just regard, and am always yours, Haller." This objection will be found very satisfactorily answered, p. 17, 54, Medical Essays and Observations. These papers were collected and enlarged, and published at Salop in 1771, under the title of Essay on the Ganglions of the Nerves. They were again published in 1795, with many valuable physiological and pathological additions, and with several other separate tracts, in one volume, under the title of "Medieal Essays and Observations, with Disquisitions relating to the Nervous System."

The vol. of Medical Essays printed in 1795, contains all the traets published by Dr. Johnstone, except his inaugural Dissertation, his Treatise on the Fever of 1756, the Life of Dr. Gregory in the Manehester Memoirs, the Life of his dearest Friend and Companion, the Rev. Dr. Job Orton, published under the article Doddridge, in the Biographia Britannica, and 2 papers in the Memoirs of the Medieal Society of London on the Angina and Scarlet Fever of 1778, and on the Diseases of Needle-makers. He published besides separately a Dialogue against that most infamous of all traffics, the slave trade, and an Analysis of Walton-water near Tewkesbury, which he proved to be nearly the same in quality as the purging waters of Cheltenham. At the end of this analysis he again displayed the strong inventive tendency of his intelleetual powers, by assigning the uses of the lymphatic glands.

At Kidderminster Dr. Johnstone continued in a very wide sphere of practiee till August 1783, when he removed to Worcester, in eonsequence of the death of his eldest son Dr. James Johnstone, who was carried off by a pestilential fever caught in attending prisoners lying ill of the fever in the jail at Worcester. (See Howard on Lazarettos, p. 122.) Here Dr. J. continued to practise with an activity unabated by age and infirmities until the time of his death; which happened on the 28th of April 1802.

"In the catalogue of his private friends, he had the honour to recount the names of men eminently and deservedly distinguished for elegant taste, for elassical erudition, and for deep researches into all the various branches of philosophy, which are intimately or remotely conneeted with the study of medieine.

"As a praetieal physician, Dr. Johnstone was active and humane; quick and sagacious in observing and deciding, he conformed exaetly to the rule laid down by Hippocrates, and thus luminously expressed by Celsus, mederi oportere, et communia et propria intuentem. His knowledge was at onee comprehensive and accurate. His application both to books and to professional duties was intense, his memory was retentive, his penetration was keen, his judgment was correct, and his elocution was ready, copious, and energetic.

"In his moral carriage, he was firm and undeviating; but his vigorous and manly mind was perhaps on some oeeasions too little aceommodating to characters and to circumstances. In his temper he was cheerful, though sometimes hasty; in his conversation lively and instructive; in his affections warm and attached. In his domestic relations he was the best of fathers; his whole life was a saerifice to the advantage of his children. In fine, as a public or a private man, his charaeter has not often been surpassed; and although the memory of his personal services eannot be soon forgotten, yet has he erected a still more durable monument to his fame, in those various practieal improvements of the medical art, which rank his name among the benefaetors of mankind."

LXXII. A Remarkable Case of Cohesions of all the Intestines, &c. in a Man of about 34 Years of Age, who died in the Summer 1757, and afterwards fell under the Inspection of Mr. Nicholas Jenty. p. 550.

The subject was tall, and partly emaciated. Mr. J. found nothing externally but a wound in the left side, which seemed to have been degenerated into an ulcer. As he did not know the man when he was alive, and had him 2 days after his decease, he could not give an immediate account of the cause of his death. But in opening his abdomen he found the epiploon adhering close to the intestines, in such a manner that he could not part it without tearing it. It felt rough and dry. And as he was going to remove the intestines, to examine the mesentery, he found them so coherent with one another, that it was impossible to divide them without laceration. He then inflated the intestinal tube, for the inspection of this extraordinary phenomenon; but to his great surprize, all the external parts of the intestines appeared smooth; very few of the circumvolutions were seen, occasioned by the strong lateral cohesions of their sides with each other. The substance of the intestines was rough, and a great number of pimples, as large as the head of a pin, appeared in them, and were almost free from any moisture. It was proper to observe, that these pimples had been taken for glands by the late Dr. J. Douglas and others; whereas they were in reality nothing else but the orifices of the exhaling vessels obstructed, and not to be met with except in morbid cases.

After having made incisions in that part of the colon next to the rectum, he found the peritoneum, or external membrane which invests the intestines, and the viscera of the abdomen, to be of the thickness of a sixpence; and he fairly drew all the intestines from their external membrane without separating their cohesions; the peritonæum, or external membrane, afterwards appearing like another set of intestines. He found a fluid in the intestines; and he would not take upon him to say, how the peristaltic motion must have been performed. And afterwards he parted the stomach from its external tunic, as he had done the intestines. He found no obstruction in the mesenteric glands; but every evolution of the mesentery firmly cohered together. The liver also adhered closely to the diaphragm and its adjacent parts: and in the vesicula fellis he found the bile pretty thick, neither too green nor too yellow, but a tint between both. He met with nothing remarkable in the other parts of the abdomen. In opening the thorax he found the lungs closely adhering to the ribs laterally, and posteriorly and interiorly close to the pericardium. In making an incision to open the pericardium, he found it so closely adhering to the heart, that he could not avoid wounding that organ, and with much difficulty could part it from it. He met with no fluid in the pericardium. The heart was small; and in

the internal side the pores of the pericardium appeared so large, that one might have insinuated the head of a middling pin into them. They had been described by some anatomists, who had met with cases somewhat similar to this, but without such universal adhesions; and they had been supposed to have been glands. The same pores likewise appeared on the heart; which in his opinion were nothing but the extremities of the exhaling vessels. In removing the heart he found the dorsal, and other lymphatic glands above the lungs, quite large, indurated, and of a dark greyish colour. Nothing remarkable appeared in the lungs; only that the portion of the pleura, which invests the lungs, and is generally thin, was here thick and rough; and through a glass it appeared as if covered with grains of sand; and might in several places have been easily torn from the lungs.

The aorta was pretty large; and in that part of it which runs on the tenth dorsal vertebra, he found a cystis, as large as an olive, full of pus; and lower down, immediately before that vessel perforates the diaphragm, he found another something less, full of matter likewise; both which portions he had by him. That portion of the aorta, where the cistis appeared, was rather thicker than the other, and osseous. In opening the cranium, he found in that part of the cerebrum, which lay over the cerebellum, a table spoonful of pus, of a greenish colour; and examining it through a glass, there was an appearance of animalcula in it.

LXXIII. On the best form of Geographical Maps. By the Rev. Patrick Murdoch, M.A., F.R.S. p. 553.

When any portion of the earth's surface is projected on a plane, or transferred to it by any method of description, the real dimensions, and often the figure and position of countries, are altered and misrepresented. In the common projection of the two hemispheres, the meridians and parallels of latitude intersect at right angles, as on the globe, but the linear distances are every where diminished, excepting only at the extremity of the projection: at the centre they are but half their just quantity, and thence the superficial dimensions but a 4th part: and in less general maps this inconvenience will always in some degree attend the stereographic projection. The orthographic, by parallel lines, would be still less exact, those lines falling altogether oblique on the extreme parts of the hemisphere. It is useful however in describing the circum polar regions: and the rules of both projections, for their elegance, as well as for their uses in astronomy, ought to be retained and carefully studied. As to Wright's, or Mercator's nautical chart, it does not here fall under our consideration: it is perfect in its kind; and will always be reckoned among the chief inventions of the last age. If it has been misunderstood or misapplied by geographers, they only are

to blame. The particular methods of description proposed or used by geographers are so various, that we might on that very account suspect them to be faulty; but in most of their works we actually find these two blemishes, the linear distances visibly false, and the intersections of the circles oblique: so that a quadrilateral rectangular space shall often be represented by an oblique-angled rhomboid figure, whose diagonals are very far from equal; and yet by a strange contradiction you shall see a fixed scale of distances inserted in such a map.

The only maps Mr. M. remembers to have seen, in which the last of these blemishes is removed, and the other lessened, are some of P. Schenk's of Amsterdam, a map of the Russian empire, the Germania Critica of the famous professor Meyer, and a few more, several of which were drawn by Senex. In these the meridians are straight lines converging to a point; from which, as a centre, the parallels of latitude are described: and a rule has been published for the drawing of such maps, mentioned in the preface to the small Berlin Atlas. But as that rule appears to be only an easy and convenient approximation, it remains still to be inquired, what is the construction of a particular map, that shall exhibit the superficial and linear measures in their truest proportions? In order to which,

Let E*l*LP, fig. 1, pl. 8, be the quadrant of a meridian of a given sphere, its centre C, and its pole P; EL, E*l*, the latitudes of two places in that meridian, EM their middle latitude. Draw LN, *ln*, cosines of the latitudes, the sine of the middle latitude MF, and its cotangent MT. Then writing unity for the radius, if in CM we take $Cx = \frac{Nn}{Ll \times MF \times MT}$, and through x draw xR, xr, equal each to half the arc L*l*, and perpendicutar to CM; the conical surface generated by the line R*r*, while the figure revolves on the axis of the sphere, will be equal to the surface of the zone described in the same time by the arc L*l*; as will easily appear by comparing that conical surface with the zone, as measured by Archimedes. And lastly, if from the point *t*, in which R*r* produced meets the axis, we take the angle C*t*V in proportion to the longitude of the proposed map, as MF the sine of the middle latitude is to radius, and draw the parallels and meridians as in the figure, the whole space SOQV will be the proposed part of the conical surface expanded into a plane; in which the places may now be inserted according to their known longitudes and latitudes.

This construction is illustrated by a calculation in an example, having the breadth of the zone 50° lying between 10° and 60° north latitude; its longitude 110°, from 20° east of the Canaries to the centre of the western hemisphere; comprehending the western parts of Europe and Africa, the more known parts of North America, and the ocean that separates it from the old continent. And then it is remarked that a map drawn by this rule will have the following properties: 1. The intersections of the meridians and parallels will be rectangular. 2. The distances

north and south will be exact; and any meridian will serve as a scale. 3. The parallels through z and y, where the line Rr cuts the arc Ll, or any small distances of places that lie in those parallels, will be of their just quantity. At the extreme latitudes they will exceed, and in mean latitudes, from x towards z or y, they will fall short of it. But unless the zone is very broad, neither the excess nor the defect will be any where considerable. 4. The latitudes and the superficies of the map being exact, by the construction it follows, that the excesses and defects of distance, now mentioned, compensate each other; and are, in general, of the least quantity they can have in the map designed. 5. If a thread be extended on a plane, and fixed to it at its two extremities, and afterwards the plane be formed into a pyramidal or conical surface, it may be easily shown, that the thread will pass through the same points of the surface as before: and that, conversely, the shortest distance between two points in a conical surface is the right line which joins them, when that surface is expanded into a plane. Now, in the present case, the shortest distances on the conical surface will be, if not equal, always nearly equal, to the correspondent distances on the sphere; and therefore all rectilinear distances on the map, applied to the meridian as a scale, will, nearly at least, show the true distances of the places represented. 6. In maps, whose breadth exceeds not 10° or 15°, the rectilinear distances may be taken for sufficiently exact. But the above example is chosen of a greater breadth than can often be required, on purpose to show how high the errors can ever arise; and how they may, if needful, be nearly estimated and corrected, as follows: Write down, in a vacant space at the bottom of the map, a table of the errors of equidistant parallels, as from 5° to 5° of the whole latitude; and having taken the mean errors, and diminished them in the ratio of radius to the sine of the mean inclination of the line of distance to the meridian, you shall find the correction required: remembering only to distinguish the distance into its parts that lie within and without the sphere, and taking the difference of the correspondent errors, in defect and in excess.

7. The errors on the parallels increasing fast towards the north, and the line SQ having at last, nearly the same direction, it is not to be wondered that the errors in our example should amount to $\frac{1}{42}$. Greater still would happen, if we measured the distance from O to Q by a straight line joining those points; for that line on the conic surface, lying every where at a greater distance from the sphere than the points O and Q, must plainly be a very improper measure of the distance of their correspondent points on the sphere. And therefore, to prevent all errors of that kind, and confine the other errors in this part of our map to narrower bounds, it will be best to terminate it towards the pole by a straight line KI touching the parallel OQ in the middle point K, and on the east and west by lines, as HI, parallel to the meridian through K, and meeting the tangent at

the middle point of the parallel SV in H. By this means too we shall gain more space than we lose, while the map takes the usual rectangular form, and the spaces GHV remain for the title, and other inscriptions.

Another, and not the least considerable, property of our map is, that it may, without sensible error, be used as a sea-chart; the rumb lines on it being logarithmic spirals to their common pole *t*, as is partly represented in the figure: and the arithmetical solutions thence derived will be found as accurate as is necessary in the art of sailing.

If it be required to draw a map, in which the superficies of a given zone shall be equal to the zone of the sphere, while at the same time the projection from the centre is strictly geometrical; take C*x* to CM, as a geometrical mean between CM and N*n*, is to the like mean between the cosine of the middle latitude, and twice the tangent of the semidifference of latitudes; and project on the conic surface generated by *xt*. But here the degrees of latitude towards the middle will fall short of their just quantity, and at the extremities exceed it; which hurts the eye. Artists may use either rule; or, in most cases, they need only make C*x* to CM as the arc ML is to its tangent, and finish the map; either by a projection, or, as in the first method, by dividing that part of *xt* which is intercepted by the secants through L and *l*, into equal degrees of latitude. Mr. Mountaine justly observes, "that my rule does not admit of a zone containing N. and S. latitudes." But the remedy is, to extend the lesser latitudes to an equality with the greater; that the cone may be changed into a cylinder, and the rumbs into straight lines.

LXXIV. *A short Dissertation on Maps and Charts. By Mr. Wm. Mountaine, F. R. S.* p. 563.

Maps and charts are either curvilinear or rectilinear. Globular, or curvilinear, are either general or particular. General are the hemispheres, for the most part constructed stereographically. Particular contain only some part of the terraqueous globe; and of this sort there are sundry modes of construction, which for the most part are defective, so as not to be applied with accuracy and facility to the purposes intended, in determining the courses or bearings of places, their distances, or both.

Rectilinear were therefore very early adopted, on which the meridians were described parallel to each other, and the degrees of latitude and longitude everywhere equal; the rumbs were consequently right lines; and hereby it was thought that the courses or bearings of places would be more easily determined. But these were found also insufficient and erroneous, the meridians being parallel, which ought to converge: and no method or device used to accommodate that parallelism. Notwithstanding the great deficiency in this plane map or chart, it

was preferred, especially in nautical business; and has its uses at this day in topographic constructions, as in bays, harbours, and very narrow zones. However, the errors in this were sooner discovered than corrected, both by mathematicians and mariners, as by Martin Cortese, Petrus Nonius, Coigniet, and some say by Ptolemy himself.

The first step towards the improvement of this chart was made by Gerard Mercator, who published a map about the year 1550, in which the degrees of latitude were increased from the equator towards each pole; but on what principles this was constructed, he did not show.

About the year 1590, Mr. Edward Wright discovered the true principles on which such a chart should be constructed; and communicated the same to one Jodocus Hondius, an engraver, who, contrary to his engagement, published the same as his own invention: this occasioned Mr. Wright, in 1599, to show his method of construction, in his book, intitled, Correction of Errors in Navigation; in the preface of which may be seen his charge and proof against Hondius; and also how far Mercator has any right to share in the honour due for this great improvement in geography and navigation.

Blundeville, in his Exercises, p. 327, published anno 1594, gives a table of meridional parts answering to even degrees, from 1° to 80° of latitude, with the sketch of a chart constructed from it: but this table he acknowledges to have received from Mr. Wright.

About the year 1720, a globular chart was published, said to be constructed by Mr. Henry Wilson; the errors in which were obviated by Mr. Tho. Haselden, in a letter to Dr. Halley; who at the same time exhibited a new scale, by which distances on a given course may be measured, or laid off, at one extent of the compasses, on Wright's projection; and was intended to render the same as easy in practice as the plane chart. The above chart was published in opposition to Mr. Wright's, which that author charged with imperfections and errors, and that it represented places larger than they are on the globe. It is true, the surface is apparently enlarged; but the position of places, in respect to one another, are in nowise distorted; and it may be asserted, with the same parity of reason, that the lines of sines, tangents, and secants, are false, because the degrees of the circle, which are equal among themselves, are thus represented unequal. Yet if a map or chart was so constructed, as to show the situation and true extent of countries, &c. primâ facie (if I may be allowed the expression), and yet retain all the properties, uses, and simplicity, of Wright's construction, it would be a truly great improvement; but this seems to be impossible.

The method exhibited by the Rev. Mr. Murdoch, in the preceding paper, shows the situation of places, and seems better calculated for determining superficial and linear measures than any other. He illustrates his theory with examples

justly intended to point out the quantity of error that will happen in a large extent. For instance: between latitudes 10° and 60° N. and containing 110° difference of longitude, Mr. Murdoch computes the distance at 5594 miles; which, on the arc of a great circle, is found to be 5477, or by other methods 5462: so that the difference is only 117, or at most 132 miles in so great an extent, and to a high latitude; and the higher the latitude the greater the error is like to be, where ever middle latitude is concerned. His courses also agree very nearly with computations made from the tables of meridional parts. However, this method does not appear so simple, easy, and concise, in the practice of navigation, as Mr. Wright's construction, especially in determining the bearings or courses from place to place: nor will it admit of a zone containing both north and south latitude.

LXXV. On the Remarkable Effects of Blisters in Lessening the Quickness of the Pulse in Coughs, attended with Infarction of the Lungs and Fever. By Rob. Whytt, M. D., F. R. S. p. 569.

One of the most natural effects of blistering plasters, when applied to the human body, is to quicken the pulse, and increase the force of the circulation. This effect they produce, not only by means of the pain and inflammation they raise in the parts to which they are applied, but also because the finer particles of the cantharides, which enter the blood, render it more apt to stimulate the heart and vascular system.

The apprehension, that blisters must in every case accelerate the motion of the blood, seems to have been the reason, why some eminent physicians have been unwilling to use them in feverish and inflammatory disorders till after the force of the disease was a good deal abated, and the pulse beginning to sink. However, an attentive observation of the effects which follow the application of blisters in those diseases, will show, that instead of increasing, they often remarkably lessen the frequency of the pulse. This Dr. W. had occasion formerly to take notice of, in his Physiological Essays, p. 69, and now evinces more fully by the following cases.

1. A widow lady, aged about 50, was seized (Dec. 1755) with a bad cough, oppression about the stomach and breast, and a pain in her right side, though not very acute. Her pulse being quick, and skin hot, some blood was taken away, which was a good deal sizy: attenuating and expectorating medicines were also prescribed. But as her complaints did not yield to these remedies, Dr. W. was called on Dec. 26, after she had been ill about 10 days, at which time her pulse beat from 96 to 100 times in a minute, but was not fuller than natural. He ordered her to lose 7 or 8 oz. more of blood, which, like the former, was sizy; and next day, finding no abatement of her complaints, he advised a blister

to be applied in the evening, to that part of her right side which was pained. Next morning, when the blister was removed, the pain of her side was gone, and her pulse beat only 88 times in a minute, and in 2 days more it came down to 78. However, after the blistered part became dry, the pulse rose in one day's time to 96, and continued between that number and 90 for 4 days: after which he ordered a large blister to be put between her shoulders. When this plaster was taken off, her pulse beat under 90 times in a minute; and next day it fell to 76, and the day after to 72. The cough and other symptoms, which were relieved by the first blister, were quite cured by the second.

2. John Graham, bookbinder, in Edinburgh, aged 37, of a thin habit of body, formerly subject to coughs, and thought to be in danger of a phthisis pulmonalis, having exposed himself unwarily to cold in the night time, was, about the end of January 1756, seized with a bad cough and feverishness; for which he was blooded, and had a diaphoretic julep, a pectoral decoction, and a mixture with gum ammoniacum, and acetum scillitieum, given him by Mr. James Russel, surgeon-apothecary in this place. On the 12th of February, after he had been ill above a fortnight, Dr. W. was desired to visit him. He seemed to be a good deal emaciated; his eyes were hollow, and cheeks fallen in: he was almost constantly in a sweat; coughed frequently, and spit up a great quantity of tough phlegm, somewhat resembling pus: his pulse beat from 112 to 116 times in a minute. In this condition he ordered immediately a blister to be applied between his shoulders, which lessened in some degree his cough and spitting, as well as the frequency of his pulse; but the blistered part no sooner began to heal, than he became as ill as before, and continued in this bad way 9 or 10 days, gradually wasting, with continued sweats, and a great spitting of a thick mucus. During this time he used tinct. ros. and the mixture with gum. ammon. and acet. scill. without any sensible benefit, and had 6 oz. of blood taken away, which was very watery, and the crassamentum was of a lax texture. In this almost desperate condition, another blister larger than the former was put between his shoulders, which remarkably lessened his cough and spitting, and in 2 or 3 days reduced his pulse to 96 strokes in a minute. After this he continued to recover slowly, without the assistance of any other medicine, except the tinct. ros. and the mixture with gum. ammon. and acet. scill. and he then enjoyed good health.

3. Mrs. ———, aged upwards of 40, who had for several years been subject to a cough and spitting in the winter months, was in October 1756, seized with those complaints in a much greater degree than usual; to remove which she was blooded, and got some attenuating and pectoral medicines from Mr. John Balfour, surgeon-apothecary in Leith. Dr. W. was called on November 11th, after she had been ill several weeks, and found her in a very unpromising

condition. She had a frequent and severe cough, with great shortness of breath and a wheezing; her lungs seemed to be quite stuffed with phlegm, of which she spit a vast quantity every day, and of such an appearance, that Dr. W. was apprehensive it was, in part at least truly purulent. When she sat up in a chair, her pulse beat above 130 times in a minute. She had a considerable thirst, and her tongue was of a deep red colour, with a beginning aphthous crust on some parts of it. She was so weak and her pulse so feeble, that there was no place for further bleeding: a blister was therefore applied to her back, Nov. 11th, which somewhat lowered her pulse, and lessened the shortness of breathing and quantity of phlegm in her lungs. Nov. 16th, a second blister was laid to her side, which gave her still more sensible relief than the former, and reduced her pulse to 114 strokes in a minute. November 25th, a 3d blister was applied to her back; by which her cough and wheezing were rendered considerably easier, and the phlegm which she spit up, lost its purulent appearance, became thinner, more frothy, and was much less in quantity. Her pulse then beat only 104 times in a minute. After this her cough and spitting increasing again, she had on the 20th of Dec. a 4th blister applied to her back, which, like the former, did her great service. Her stomach being extremely delicate, he scarcely ordered any medicines for her all this time, except a cordial julep, with spir. volat. oleos. tinct. of rhubarb as a laxative, and a julep of aqu. rosar. acet. vin alb. and syr. bals. of which last she took 2 table spoonfuls 2ce or 3ce a day in $\frac{1}{4}$ of a pint of lintseed tea. After the 4th blister, she drank for some time a cupful of infusum amarum 2ce a day, and continued to recover slowly: and though during the remaining part of the winter she was as usually, a good deal troubled with a cough, yet in the spring she got free from it and recovered her ordinary health.

4. Christian M^c^ewen, aged 21, had laboured under a cough, thick spitting, pain of her breast, and pains in her sides affecting her breathing for about a 12 month: and after getting by proper remedies, in a good measure free from those complaints, her cough from taking a fresh cold, increased to a greater degree than ever, became hard and dry, and was attended with a constant difficulty of breathing, pain in her left side, and head-ach. After having been 7 or 8 days in this condition, she was admitted into the Royal Infirmary, January 9th, 1757. As her pulse was small, though very quick, viz. beating 130 in a minute, he thought it unnecessary to bleed her, as from former experience he did not doubt but that blistering alone would relieve her: he therefore ordered a large blister to be applied to her left side, where she complained of pain, and prescribed for her the following julep:

℞ Aq. menth simp. sp. Mind. ana ℥ iij. acet. scill. ℥ i. sacch. alb. ℥ ij. M. Cap. coch. ij. ter in die.

She was also desired to breathe frequently over the steam of hot water, and to drink lintseed tea.

January 10th. Her pulse beat only 112 times in a minute, and was somewhat fuller than on the 9th. The blister was not removed till late in the evening, and made a plentiful discharge. The cough having been so severe last night, as to keep her from sleep, he ordered her the following anodyne draught:

℞ Sp. Mind. ℥ss. acet. scill. ʒi. syr. papav. alb. ʒvi. M. cap. hor. somni.

Jan 11th. The cough easier last night; difficulty of breathing less; pulse 108 in a minute. Ordered the anodyne draught to be repeated, and the use of the julep, with acet. scill. to be continued. Jan. 12th. Pulse slower; cough and pain of the side easier; but still complained of a head-ache. Jan. 13th. Pulse 94 in a minute; cough continued easier in the night, but was troublesome in the day-time. Jan. 14th. Every way better; pulse only 80 in a minute. As her cough was still bound, he ordered her, besides the medicines above-mentioned, a pectoral decoction of rad. alth. &c. Jan. 15th. Cough and other complaints in a great measure removed: pulse 65 in a minute.

From this time her cough gave her little trouble; but on the 18th, she complained of a pain in the epigastrium, with sickness at stomach, want of appetite, and a giddiness in her head, which were considerably relieved by a vomit, infus. amar. and stomachic purges; and were almost wholly cured by the return of her menses, on the 5th of Feb. after an interval of 8 weeks.

5. A girl 21 months old, who had (Dec. 1756) a great load of the small-pox, and not of a good kind, with a cough and obstructed breathing, was, on the 7th day from the eruption, blistered on the back; by which the pulse was lessened from 200 to 156 strokes in a minute. Next day her legs were also blistered, and the pulse thereby fell to 136. But the child's lungs being much oppressed, and her throat being so full of pustules that she could scarcely swallow any thing, she died towards the end of the 9th day.

Dr. W. could have added several other cases of the remarkable effects of blisters in lessening the quickness of the pulse in coughs attended with fever, pain in the side, and pituitous infarction of the lungs: but those above seemed sufficient to put this matter out of doubt, as well as to remove any prejudice that might remain against the free use of so efficacious a remedy.

In a true peripneumony, especially where the inflammation is great, repeated bleeding is the principal remedy, and blisters early applied are not so proper. But when the peripneumony is of a mixed kind; when the lungs are not so much inflamed as loaded with a pituitous matter; when bleeding gives but little relief; when the pulse, though quick is small; when the patient is little able to bear evacuations, and the disease has continued for a considerable time; in all

these cases blistering will produce remarkably good effects, and far from increasing, will generally lessen the frequency of the pulse and fever, more speedily than any other remedy.

On the other hand, when the fever and frequency of the pulse proceed from a true inflammation of the lungs, from large obstructions tending to suppuration, or from an open ulcer in them, blisters will be of less use, nay sometimes will do harm except in the last case, where they, as well as issues and setons are often beneficial, though seldom able to complete a cure. But as in pituitous infarctions of the lungs, with cough and fever, repeated blisters applied to the back and sides are far preferable to issues or setons, so these last seem most proper in an open ulcer of the lungs. The former make a greater and more sudden derivation, and are therefore adapted to acute cases; the latter act more slowly, but for a much longer time, and are therefore best suited to chronic diseases. Further, while blisters evacuate chiefly the serous humours, issues and setons generally discharge true purulent matter, and on this account may be of greater service in internal ulcers.

In what manner blisters may lessen the fever and frequency of the pulse attending internal inflammations, Dr. W. had endeavoured to explain in his Physiological Essays, p. 69; and only adds here, that in the cases above recited, where the quick pulse and feverishness proceeded more from a pituitous infarction than a true inflammation of the lungs, blisters, by relieving this organ in some measure of the load of humours oppressing it, would render the circulation through its vessels freer, and consequently lessen the quickness of the pulse, and other feverish symptoms.

It might not, however, be improper briefly to point out the reason, why blisters which have been observed to be remarkably efficacious, even when early applied in pleurisies,* are less so in true peripneumonies. This difference he imagined might be accounted for from there being no immediate communication between the pulmonary vessels and those of the sides and back, to which the blisters are applied; whereas the pleura and intercostal muscles are furnished with blood-vessels from the intercostal arteries, which also supply the teguments of the thorax: so that while a greater flow of serous humours, and also indeed of red blood, is derived into the vessels of the external parts to which the vesicatories are applied, the force of the fluids in the vessels of the inflamed pleura, or intercostal muscles must be considerably lessened. Further, as the intercostal muscles and pleura are, as well as the teguments of the thorax, supplied with nerves from the true intercostals, blisters applied to the back and sides may perhaps, on this account, also have a greater effect in relieving inflammations there

* Dr. Pringle's Observations on the Diseases of the Army, part iii, chap. 2.

than in the lungs, which have nerves from the 8th pair, and from the intercostals improperly so called.

LXXVI. A remarkable Instance of Four Rough Stones, discovered in a Human Urinary Bladder, contrary to the received Opinion; and Successfully Extracted by the Lateral Method of Cutting for the Stone. By Mr. Joseph Warner, F. R. S. p. 579.

[Reprinted in this author's work, entitled Cases in Surgery, to which the chirurgical practitioner is referred.]

LXXVII. Observations on the Limax non cochleata Purpur ferens, the naked Snail producing Purple. By J. A. Peyssonel, M. D., F. R. S. Translated from the French.* p. 585.

Among the fish met with in the seas of the Antilles of America, this, here described, appears precious from the beautiful purple colour it produces, in the same manner that the cuttle-fish produces its ink, if means could be found to procure this liquor in a sufficient quantity to render it an article of commerce. These fishes are soft, viscous, without shells, scales, or bones; are of the nature of the polypi, and such other kinds, without feet, fins, or any thing to supply their places. Their motion is vermicular; and like the slugs they wreath themselves up, and when touched make themselves quite round. They fill up certain membranes of the body with water. Their local motion, antennæ, which they lengthen and contract; and a great many other properties which they have in common with snails, slugs, and turbinated shell-fish, made Dr. P. call them naked snails: and though they have not the most essential qualities of snails, he thought he might give them the name, as they have no particular appellation in this country. Some call them piss-a-beds, some sea-cats, and others a less modest name, tapecon, taken from Pliny.

This fish is commonly 4 inches long, and 2 thick; of a greenish colour, spotted with black, each of which forms a circle. The under part is like that of snails, flat, with kinds of mamillæ, or rugosities, which are adhesive; by means of which they advance in a vermicular motion; and when touched become round, by retracting their neck and head; and afterwards protrude them considerably, according to their motion and progression, crawling upon rocks to seek their food. The head of this animal has a flatness, or is inclinable to a square or parallelogram. On each side there are membranes of skins, which form kinds of ears; and under them others, which at times fill with water, and are then transparent. Under this thick skin there is a cranium, of a kind of

* The animal here described seems to be a species of aplysia.

coriaceous or cartilaginous matter; and in the cranium we find the brain, which is a white substance, and very firm. At the basis of the head its oval wide mouth is placed, being above two lines long, which often discovers a white hard edge, with which it crops the fucuses and other sea-plants for his nourishment. About half an inch from the ears there are 2 horns, or antennæ, like those of some testaceous animals which serve them for eyes; and these antennæ extend and contract at will, turning to either side also. The œsophagus begins at the upper and inner part of the mouth, which is a delicate long tube; near which there is another thick one, made nearly like the colon, which leads to a bag or the first stomach, which may be likened to the craw of a fowl: it is always filled with fucus mixed with sand. Sometimes this stomach is double, or at least lengthens itself considerably, and the aliment parts it, as it were, into 2 portions. After this craw or stomach we find another, which performs the same office as the gizzard of fowls. The membranes are thick, and are set with 12 stones or horny pieces, of a bright yellow colour, and as transparent as fine yellow amber, ending in points like a diamond, so that the great side or basis is set into the membrane of the gizzard as a diamond in its socket: others differ in size, having different figures, that in acting all together they may be able to break and grind the herbs the animal feeds on, as well by the strength of the muscle or gizzard which puts them into action, as by the situation of these stones assisted by grains of sand found in it, turning the whole by this trituration into a liquor. Afterwards, what was thus triturated by the power of the gizzard, passes into a 3d belly or stomach, which is covered by a purple body, resembling the parenchyma of the liver, and nearly of the same consistence: then this belly turns into a long tube, which surrounds this parenchyma, and is covered in like manner by a very fine membrane: it is full of a white liquor, like chyle, and goes to discharge itself into another reservoir, at the side of which is a yellowish gland, like a pancreas. From these 2 bodies or glands, one of which may be called hepatic, and the other pancreatic, two conduits pass out; that of the pancreas is white, the other of a blackish purple: the first conducts its chyle, condensed into a reservoir or bladder, which may be resembled to the receptaculum chyli of Pecquet, and thence passes to the fecal matter: the other conducts to a body made like the mesentery, but which is always found out of the common capacity or cavity, in which all the viscera are contained. This common capacity is very large, beginning at the head and ending at the tail of the fish: it is sometimes filled with a yellowish water, and is formed by the fleshy body of the animal; which is only a membrane composed of fibres every way interwoven together, open at the top, where the organs are situated, which contain the purple juice.

There is a hollow on the back of the animal, where the canal filled with a

reddish juice passes out, carrying it to a fringed body like a mesentery; and it is there the purple juice is brought to perfection; and afterwards goes to a long sac lying under a kind of horny plate, not like the bone of the cuttle-fish, but like the bone of the sepia or little cuttle-fish, which we call le couteau. This bone or horny substance is transparent, and is of a triangular figure, or approaching the form of a bivalve shell. On the right side it is fastened by a strong cartilaginous muscle, which binds it to the body of the animal; and on the left it is open and detached, and easy to be pulled up: then it is easy to see underneath both the mesenteric body, and the tube or reservoir of the purple juice. This bone or horny plate is covered by a loose membrane, which is by no means attached to it, but capable of being filled and inflated with water or wind. The whole is covered with two membranes, which are continuations of the flesh of the fish's body: the membranes are loose, and larger than are necessary to the bone: they are wrinkled or rumpled over one another to cover the whole, and to defend the bone and viscera from all kinds of pressure; but they are ready to stretch from each other, and leave the parts destined for the purple juice uncovered. They begin a little under the neck, and extend in the female animal to the tail, which is flat; and in the male they do not go so low, but end at some distance from the tail. The females are oviparous; for eggs are found in the grand cavity, at the side of the pancreatic body.

It has already been said, that when the animal is touched he makes himself round, and throws out his purple juice, as the cuttle-fish does his ink. This juice is of a beautiful deep colour: it tinges linen, and the tincture is difficult to get out. It remains at present to try if we can collect a sufficient quantity of this juice, and to find a means of preserving the tincture; which would then be certainly of great value. When the fish is boiled, or put into spirits it shrinks up, and loses two thirds of its size; because all the water which is in the interstices of the fibres is dissipated, and the dry fibres contract: which clearly appears from dissecting them.

LXXVIII. New Observations on the Worms that form Sponges. By J. A. Peyssonel, M. D., F.R.S. From the French.* p. 590.

The existence of the nests of corallines and lithophyta, and the mechanism of their polypi, made Dr. P. conjecture that it was the same with respect to sponges; that animals nested in the interstices of their fibres, gave them their origin and growth: but he had not yet seen nor discovered the insects, nor observed their work. Sponges appeared only as skeletons: but at length he dis-

* It is necessary to observe here, that Dr. Peyssonel's notion of the formation of sponges by worms is totally erroneous, and that sponges constitute a peculiar genus of Zoophytes.

covered these worms, which form sponges in the 4 following species: 1. Spongia Americana tubo similis; the tube-like sponge of Plumier. 2. Spongia Americana longissima funiculo similis; the cord-like sponge of Plumier. 3. Spongia Americana capitata et digitata; the fingered sponge of Plumier. 4. Spongia Americana favo similis; the honey-comb sponge of Plumier.

These 4 kinds differ only in form: they have the same qualities, are made by the same kinds of worm, and what may be said of the one agrees exactly with all the rest; for the same observations were made on them all.

When a fresh sponge is squeezed, the mucilage from the interstices of the fibres comes out frothy, by the mixture of the windings of its fibres: it always issues forth with sand, or little parcels of shells crushed by the sea. These fibres which consist of the twisted doubles of the sponge, form as it were a labyrinth filled with worms, which are easily crushed, and their juice is confused with the mucilage; but having carefully torn the sponges, and their gross fibres, Dr. P. discovered the living worms. These species of sponge commonly grow on sandy bottoms. At their origins is perceived as it were a nodule of sand or other matter, almost petrified, round which the worms begin to work, and round which they retire, as to their last seat or refuge; where he had seen them play, exercise themselves, and retire, by examining them with the microscope; and he had even made observations without its assistance.

The worms he found in these kinds of sponges are about one-third of a line thick, and 2 or 3 lines in length. They are so transparent, that one may discern their viscera through their coverings and substance: the blood may be seen to circulate, and all their parts to act. They have a conic figure, with a small black head furnished with two pincers: the other extremity is almost square, and much larger than the head. On the back may be seen 2 white streaks or fillets, as if they contained the chyle: these 2 canals are parallel to each other from the head to the other extremity, where they come together. In the middle where the belly and viscera ought to be placed, a blackish matter is perceivable, which has a kind of circulation: sometimes it fills all the body of the worm, sometimes it gathers towards the head, or at the other end, and sometimes it follows the motion of the animal. This vermicular motion or progression begins at the posterior extremity, and ends at the head, which is pushed, and consequently advances forward. He kept these worms alive out of the sponge, quite detached from it more than an hour; having examined them thoroughly with a middling magnifier; for a great magnifier would be the grave of the insect.

He was surprized, after having finished his observations, when he put them near a piece of the fresh sponge, where the nests were moist, and from which he had pulled them, to see them enter into them and disappear, being lost in

the windings of the tubes. He thought to have found them again; but it was a difficult task to search for them. He crushed them, or they were themselves mashed in the tubes, which he pressed, and of which he had consequently spoiled the texture; but he could not find them; and this happened several times. These worms have no particular lodge: they crawl indifferently in the tubular labyrinth. So that without offence to Pliny and other naturalists, he does not see that it is in their power to dilate and contract the bodies of the sponges; which always remain in the same state of magnitude, without being any way sensible to the touch, or any other motion of the sea, nor to any other accident whatever, being an inanimate body; for the animal sensitive life, or whatever you will have it, belongs only to the worms, that form these bodies, and which are their dwelling places; and which, by the juice they deposit, make the sponge increase or grow, as bees, wasps, and especially the wood-lice of America, increase their nests or cells. In short, the blood or humours, which the ancients have observed, is no other than the mucilage or juice of the substance of these worms.

LXXIX. Of an Experiment, by which it appears that Salt of Steel does not enter the Lacteal Vessels. By Edward Wright, M.D. p. 594.

Though iron is universally allowed to be one of the most powerful medicines now in use, yet many physicians observing that the fæces of patients who used it, either in a metallic or saline form, were tinged of a black colour, have been led to think, that in a metallic state it could not be reduced into particles fine enough to be received by the lacteal vessels; and if taken in a saline form, that it underwent a precipitation in the intestines, by which, being reduced to an earth or calx, it was in like manner rendered incapable of making its way into the blood. But the accurate experiments with which Signor Menghini has favoured the public in the Memoirs of the Bononian Academy, sufficiently prove, that the ore and filings of iron, finely levigated, enter the blood in considerable quantity; as does also the crocus, calx, or earthy part of the metal, though in less proportion than the 2 former, which were found to act with a violent stimulus on the vessels, and to have dissolved and broken the crasis of the blood of different animals, that had used them for some weeks in large doses mixed with their ordinary food. Though it must be allowed that these experiments are very curious, yet the subject seems to require a further inquiry, viz. whether iron is capable of entering the blood in a state of solution, or under a saline form: for, from the violent stimulus, as well as from the dissolution of the blood, and other symptoms brought on by the use of the ore and filings, these substances, not being properly dissolved, appear to have acted in a manner so grossly mechanical, that whatever Signor Menghini might think, very little was to be concluded

from them, with regard to the action of iron on the human body, in such cases as indicate its use, and where a rational physician would think proper to prescribe it as a medicine.

Having read Signor Menghini's memoir, Dr. W. recollected, that in the year 1753 he had, with the assistance of 2 friends, made the following experiment, in order to discover whether iron in a saline form is capable of entering the lacteals. An oz. and a half of salt of steel dissolved in a sufficient quantity of water, filtrated and mixed with about 1 lb. of bread and milk, were forced down the throat of a dog that had been kept fasting for 36 hours. An hour after he had swallowed this mixture, having secured him in a supine posture, as is usual in such experiments, they opened the abdomen, and observed the lacteal vessels, like white threads, running along the mesentery in a very beautiful manner. On slitting open part of the small guts, they there found a good deal of the mixture, which appeared frothy, but without any black colour, or the least sign of the salt being precipitated; and it struck a deep inky colour with infusion of galls. Though the white colour of the lacteals convinced them that they were full of chyle, yet as it would have been impossible to have collected a sufficient quantity of it from them, they found it necessary to open the thorax, and tie the thoracic duct a little above the receptacle, which, from the ligature, soon became turgid, the animal being alive and warm, and the chyle still continuing its course towards the thoracic duct. Having cut open the receptacle, they easily collected a sufficient quantity of chyle, and immediately mixed with it, drop by drop, infusion of galls; a very simple and easy method, by which an incredibly small quantity of salt of steel may be discovered in most liquors: but not the smallest change of colour was observed, though they were rubbed together for some time, and allowed to stand several hours. Now had there been a single atom, so to speak, of the salt in so small a portion of chyle, as that used in this experiment, which was, as near as he could guess, somewhat less than $\frac{1}{2}$ oz., it is not to be imagined that it could have failed to discover itself by this method of trial; for on adding $\frac{1}{4}$ gr. of the salt, this mixture instantly became of a bright purple: and he had found by other experiments that the smallest quantity of salt of steel shows itself as readily in the chyle by galls as in any other liquor of the same consistence. This experiment, with another observation he had made, viz. that neither the blood nor urine of patients, during the use of salt of steel, in the least change colour with galls, renders it more than probable that this salt does not enter the blood.

As the salt was found to have undergone no change in the small guts, it appeared that it is not prevented from entering the lacteals by its being decomposed or precipitated, as has been imagined; but on the contrary, that what rendered it incapable of being received by these vessels was its astringency: for the lac-

teals seem to be endowed with that admirable faculty of admitting such particles of pure chyle as they happen to be in contact with, and of accommodating their diameters to them, at the same time that by their natural irritability, and power of constriction, they obstinately exclude such as are astringent; which, were they to enter the lacteals, would either produce dangerous obstructions in these vessels, or if they got into the blood, would occasion polypous concretions in the larger vessels, or coagulations incapable of being transmitted through the minute vessels of the lungs; the effects of which would be either sudden death, or at least inflammations and suppurations from obstructions in the pulmonary vessels; inconveniences which nature, by precluding astringents from entering the lacteals, has carefully and wisely avoided.

Salt of steel, taken internally, must retain its astringency until it be precipitated: which can scarcely ever fail to happen in the great guts, from the putrid fæces they contain, which are always observed to be tinged of a black colour from the metallic basis of the salt, part of which, as it has little or no astringency, may doubtless enter the blood, as Signor Menghini observed of the crocus, which is the same substance; and we know, from the experiments of Lister and Musgrave, that particles, much grosser than those of the white chyle, provided they be not astringent, or very acrid, are conveyed by the lacteals. But the metallic basis being separated from its acid, and thus reduced to a mere calx or earth, can scarcely be supposed to have any medicinal quality whatever, or at least to have any share in the virtues justly attributed to salt of steel.

As this salt is not only astringent, and consequently a strengthener, but at the same time acts with a gentle stimulus, all its virtues (which are known to be very great in diseases, where the fluids are either viscid, cold, and phlegmatic, or dissolved and watery, from a laxity of the solids) may be accounted for from its immediate effects on the stomach and primæ viæ, and on the system of the solids in general by consent; which it would be needless to illustrate by similar examples, because well known to every one the least versed in medical studies. Dr. W., from the obvious qualities of this medicine, and from what had been observed above, deduces the following corollaries: 1. That salt of steel has no deobstruent or aperient virtue by any immediate action that it can possibly have on the blood, or other animal fluids, as some have imagined; but that on the contrary it owes this quality to its not entering the blood, which it would otherwise coagulate, and to its action on the solids alone. 2. That in diseases proceeding from a laxity of the solids, great care ought to be taken to restore and invigorate the primæ viæ; since a medicine (and this we may presume not the only one) whose immediate action is confined to those parts, is yet found by experience to produce so salutary effects in such diseases. 3. That as this salt does not enter the blood, and consequently cannot be in danger of too much

stimulating or constricting the vessels, on which it only acts by consent, it may, in small doses, be successfully used in many cases, where it has been imagined to be hurtful, particularly in consumptions of the lungs, so frequent and fatal in this island; which are commonly attended with too great a laxity of the primæ viæ, and of the solids in general, though they seem more immediately to proceed from a laxity and weakness of the pulmonary vessels; in which circumstances it must be of the utmost consequence to restore the tone of those principal organs of chylification, the primæ viæ; as good chyle not only corrects the acrimony of the blood, which in the advanced stages of consumptions so much prevails, but likewise saves a great deal of labour, which the lungs (already too much oppressed) must otherwise undergo from a crude and ill-concocted chyle. Agreeably to this we find, in the Essays Physical and Literary of Edinburgh, two well-vouched histories of patients far gone in consumptions, with the usual symptoms of pain in the breast, cough, gross spitting of fetid matter, difficulty of breathing, hectic fits, and morning sweats, perfectly cured in a few weeks, by the use of the Hartfell-spa near Moffat; which, contrary to what is observed in most natural chalybeate waters, contains a fixed vitriol of iron.

LXXX. On the Antiquity of Glass in Windows. By the Rev. John Nixon, M.A., F.R.S. p. 601.

Mr. N. having formerly laid before the R.S. a few observations on some of the curiosities found at Herculaneum, among other articles he just mentioned a piece of a plate of white glass; and now he inquires into the uses to which such plates might be applied in the early age, to which this fragment undoubtedly belongs.

And first it is obvious to imagine that such plates might serve for specula, or looking-glasses. And indeed that specula were anciently made, not only of metals, and some stones, as the phengites, &c. but also of glass, may, he thinks, be collected from Pliny, who having mentioned the city of Sidon as formerly famous for glass-houses, adds immediately afterwards, siquidem etiam specula excogitaverat. But then it is to be observed, that before the application of quick-silver in the constructing of these glasses (which Mr. N. thinks is of no great antiquity), the reflection of images by such specula must have been effected by their being besmeared behind, or tinged through with some dark colour, especially black, which would obstruct the refraction of the rays of light. On these hypotheses (supposing the tincture to be given after fusion) the lamina before us may be allowed to be capable of answering the purpose here assigned.

It may further be suggested, that plates of this kind might be intended to be wrought into lenses, or convex glasses, either for burning or magnifying objects placed in their focus. But this designation cannot be supported by proper vouchers from antiquity. On the contrary, we are informed that the ancients

used either specula of metal, or balls of glass for the former of these purposes; as it is well known, that glass was not applied to the latter in optical uses till the beginning of the 13th century. However, we may with greater probability propose another use for which the ancients might employ such plates of glass, as are now under consideration, viz. the adorning the walls of their apartments by way of wainscot. This he takes to be the meaning of the vitreæ cameræ mentioned by Pliny, who intimates, that this fashion took its rise from glass being used by M. Scaurus for embellishing the scene of that magnificent theatre, which he erected for exhibiting shows to the Roman people in his ædileship. And we may collect from the same author (what is further confirmed by his contemporary Seneca), that this kind of ornament had been admitted, in his name, into chambers in houses, baths, &c. Whether the plates used for this purpose were stained with various colours (as mentioned above), or had tints of divers kinds applied to the back part of them, he pretends not to determine: but in either way they would have a very agreeable effect.

The last destination, which the obvious congruity of the thing itself, countenanced by the practice of many ages past, as well as of the present time, would induce one to ascribe to such plates of glass, is that of windows for houses, baths, porticos, &c. But he was sensible that whoever should be hardy enough to advance such an hypothesis, would be censured as an innovator, in opposing the general opinion of the connoisseurs in antiquity. These gentlemen are almost unanimous in asserting, that whenever we meet with mention made of specularia in ancient writers (especially those of, or near to, the age to which we must refer this fragment), we are to understand by that term nothing but fences made of laminæ, either of a certain stone called from its transparent quality, lapis specularis, brought first from Hispania Citerior, and afterwards found in Cyprus, Cappadocia, Sicily, and Africa; or of another stone of the same nature, viz. the phengites. These, though expressly distinguished from each other by Pliny, are yet reckoned by some moderns as one and the same thing; and thought to have been nothing but a kind of white transparent talc, of which according to Mons. Valois, there is found a great quantity in Moscovy at this day. Now that this lapis specularis, or phengites, was really used for windows by the ancient Romans in their houses, &c. cannot be denied: since, according to the opinion of the learned in antiquity, this usage is mentioned by Seneca, among other improvements in luxury introduced in his time. But whether it was so used exclusive of other materials (particularly glass), may he thinks admit a doubt. Salmasius is of opinion that nothing can be determined on this point from the word specular itself, which seems to be a generical term, equally applicable to windows of all kinds, whether consisting of the lapis specularis, or any other transparent substance. And as (according to this learned writer) there

is nothing in the term specular itself which hinders it from being extended to windows made of other materials besides those above mentioned; so others imagine, that there are some intimations in ancient authors which require that it should actually be so extended. Thus Mr. Castells, the ingenious illustrator of the villas of the ancients, thinks 'that if this had not been the case, Palladius would not have given directions to his husbandman to make specularia in the olearium, or store-room, where the olives were preserved. For it appears, says this author, from Pliny's describing a temple built of the lapis specularis, or phengites, as the greatest rarity in his time, and the mention Plutarch makes of a room in Domitian's palace lined with it, that it was not common enough for husbandmen to purchase;' viz. in such quantities as were required for the purposes mentioned above.

Mr. N. does not pretend to decide on the weight of this argument of Mr. Castells; but only observes, that if any one should be induced by it to think, that the use of glass for windows may be of much greater antiquity than is commonly allowed, or even as old as the fragment which occasions these remarks, he may find other probable reasons to corroborate his opinion. As, first, that there seems to have been a natural and obvious transition from the practice of using glass plates for the ornamenting the walls of apartments to that of introducing light into those apartments, as we find the lapis specularis was in fact employed at the same time for both those purposes; and consequently it seems reasonable to suppose, that the latter of these applications could not be long in point of time after the former. But it appears from the authorities produced above, that the former of these usages did actually subsist in the age of Pliny; and therefore before the destruction of Herculaneum, where he lost his life. Whence we may draw no improbable conclusion, that the latter destination of plates of glass, viz. for window-fences, did likewise precede the same event.

Mr. N. adds further, that this presumptive argument in favour of the antiquity of windows made of plates of glass, receives an additional force from the close relation which must be allowed to subsist between them, and those composed of the lapis specularis. The former must be considered as an improvement on the other, as they answered all the purposes of convenience, and at the same time were more beautiful; and being the manufacture of Italy, might probably be purchased at a less expence. On all which accounts it seems reasonable to conclude, that one of these inventions would naturally be introductory to the other: and consequently, that as window-lights of the lapis specularis began to be used within the memory of Seneca, who died under Nero, about Anno Christi 68, the original of those of glass may have fair pretensions to a place within the period assigned in the foregoing paragraph, viz. some years before the destruction of Herculaneum, in whose ruins the plate before us was buried.

Mr. N. concludes, that all the evidence here produced to prove the usage of glass windows to have been coeval with the fragment he is now considering, is of the conjectural kind only: for he had not been able to trace it up by any positive authority higher than about 200 years short of the epocha last mentioned, viz. to the latter end of the 3d century, when it is expressly mentioned by Lactantius in these words:—Manifestius est, mentem esse, quæ per oculos ea, quæ sunt opposita, transpiciat, quasi per fenestras lucente vitro aut speculari lapide obductas. De Opificio Dei, cap. 5.

LXXXI. Of an Extraordinary Case of the Efficacy of the Bark in the Delirium of a Fever. By Nicholas Munckley, M. D., F. R. S. p. 609.

In this paper it is related that a patient who had been for several days ill of a fever (partaking of the nature of an irregular intermittent), and who from the reduced frequency of pulse and other circumstances appeared to be much better, was seized with a delirium, in which it was difficult for the attendants to keep him in bed. He laughed, played antic tricks, and used gestures the most opposite to his common demeanour when well; a state of mind having more the appearance of mania than of the delirium of a fever. Dr. M. prescribed the bark, which put a stop to the delirium, and effected a cure.

LXXXII. Of an Earthquake felt at Lingfield in Surrey, and Edenbridge in Kent, Jan. 24, 1758. By James Burrow, Esq. R. S. V. P.* p. 614.

This slight, and rather partial shock, happened between 1 and 2 o'clock in the morning of the day mentioned. It was felt in the parishes of Worthe, and East-Grinsted in Sussex; Lingfield in Surrey; and Edenbridge in Kent; and other adjacent places; which alarmed several of the inhabitants very much; but no damage ensued. The beds and windows, &c. were sensibly shaken, and at last it went off with a noise like a small gust of wind.

LXXXIII. On the Case of the first Joint of the Thumb torn off, with the Flexor Tendon in its whole Extent torn out.† By Robert Home, Surgeon at Kingston on Hull. p. 617.

Jan. 2d, 1758, Wm. Taylor, 17 years of age, an apprentice to a white-smith

* Sir James Burrow, an eminent lawyer, and master of the Crown-office, died in 1782, at a very advanced age, apparently upwards of 80. In 1773 acting as president of the R. S. till the time of the anniversary election that year, and the Society addressing the king at that time, he received the honour of knighthood on the occasion. Sir James published 4 vols. of Reports; and one of Decisions in the Courts of King's Bench. He wrote also an Essay on Punctuation, and some Anecdotes of Oliver Cromwell and family.

† Mr. Home, in the prefatory observations to this communication states, that Marchetis has a case of the same kind, and that there are several others collected together by Mons. Morand in the 2d vol. of the Memoirs of the Royal Academy of Surgery at Paris.

in this place, in endeavouring to make his escape from one who was going to correct him, opened the door of a cellar, and threw himself into it; but in his hurry so entangled his right thumb with the latch, that the whole weight of his body was suspended by it, until it gave way, and was torn off at the first articulation; the flexor tendon being at the same time pulled out in its whole length, having broke when it became muscular. Mr. H. was immediately sent for, found little or no hæmorrhage, and the bone of the 2d phalanx safe, and covered with its cartilage, but protruding considerably, occasioned by part of the skin belonging to it being irregularly torn off with the first joint.

He was doubtful whether he should not be obliged at last to make a circular incision, and saw the bone even with the skin; but thought it proper to give him a chance for the use of the whole phalanx. He complained only for the first day of a pretty sharp pain in the course of the tendon; to which compresses, wrung out of warm brandy were applied: but his arm was never swelled; there was no ecchymosis; nor had he so much fever, as to require bleeding even once. The cure proceeded happily, no symptoms arising from the extracted tendon. At the third dressing the bone was covered; and no other application but dry lint was necessary during the whole time. No exfoliation happened; yet it was 12 weeks before it was entirely cicatrized, owing to the loss of skin: and he seemed to enjoy the use of the stump as completely, as if that tendon had not been lost.

LXXXIV. On the late Discoveries of Antiquities at Herculaneum, and of an Earthquake there. By Camillo Paderni, F.R.S. Dated Portici, Feb. 1, 1758. p. 619.

Having been working continually at Herculaneum, Pompeii, and Stabiæ, since his communication of Dec. 16, 1756, the most remarkable discoveries made there are the following:

February 1757, was found a small and most beautiful figure of a naked Venus in bronze, the height of which is 6 Neapolitan inches. She has silver eyes, bracelets of gold on her arms, and chains of the same metal above her feet; and appears in the attitude of loosening one of her sandals. The base is of bronze inlaid with foliage of silver, on one side of which is placed a dolphin.

In July was found an inscription, about 12 Neapolitan palms in length, as follows:

IMP , CAESAR , VESPASIANVS , AVG , PONTIF , MAX
TRIB , POT , VII · IMP · XVII · P · P · COS · VII · DESIGN · VIII
TEMPLVM , MATRIS , DEVM , TERRAE , MOTV , CONLAPSVM , RESTITVIT

After having found a great number of volumes of papirus in Herculaneum;

many pugillaries, styles, and stands with ink in them as formerly mentioned; at length, in the month of August, on opening a small box, was also found the instrument with which they used to write their manuscripts. It is made of wood, of an oblong form, but petrified, and broken into 2 pieces. There is no slit in it, that being unnecessary, as the ancients did not join their letters in the manner we do, but wrote them separate.

In September were discovered 8 marble busts, in the form of terms. One of these represents Vitellius, another Archimedes; and both are of the finest workmanship. The following characters in a black tint, are still legible on the latter, namely, ΑΡΧΙΜΑΕ which is all the inscription that now remains. In October was dug up a curious bust of a young person, who has a helmet on his head adorned with a civic crown, and cheek-pieces fastened under his chin. Also another very fine bust of a philosopher with a beard, and short thick hair, having a slight drapery on his left shoulder. Likewise two female busts; one unknown, in a veil; the other Minerva, with a helmet; both of middling workmanship. In November we met with two busts of philosophers of excellent workmanship, and, as may be easily perceived, of the same artist; but unfortunately, like many others, without names. In January was found a small but most beautiful eagle, in bronze. It has silver eyes, perches on a prafericulum, and holds a fawn between its talons. In the same month was discovered at Stabiæ, a term 6 palms high, on which is a head of Plato, in the finest preservation, and performed in a very masterly manner. Also divers vases, instruments for sacrificing, scales, balances, weights, and other implements for domestic uses, all in bronze.

Having finished the examination and arrangement of the scales, balances, and weights, which were very numerous in the museum; it was remarkable that many of the former, with all the weights, exactly answer those now in use at Naples.

The whole day and night of the 24th of last month it seemed as if Mount Vesuvius would again have swallowed up this country. On that day it suffered 2 internal fractures, which entirely changed its appearance within the crater, destroying the little mountain that had been forming within it for some years, and was risen above the sides; and throwing up by violent explosions, immense quantities of stones, lava, ashes, and fire. At night the flames burst out with greater vehemence, the explosions were more frequent and horrible, and our houses shook continually. Many fled to Naples, and the boldest persons trembled. But the mountain having vented itself that night and the succeeding day, is since become calm, and throws out only a few ashes.

LXXXV. A Further Attempt to Facilitate the Resolution of Isoperimetrical Problems. By Mr. Thomas Simpson, F. R. S. p. 623.

About 3 years before, Mr. S. laid before the R. S. the investigation of a general rule for the solution of isoperimetrical problems of that kind, wherein one only of the 2 indeterminate quantities enters along with the fluxions, into the equations expressing the conditions of the problem. Under which kind are included the determination of the greatest figures under given bounds, lines of the swiftest descent, solids of the least resistance, with innumerable other cases. But though cases of this sort do indeed most frequently occur, and have therefore been chiefly attended to by mathematicians, others may nevertheless be proposed, such as actually arise in inquiries into nature, where both the flowing quantities, together with their fluxions are jointly concerned. The investigation of a rule for the solution of these, is what Mr. S. attempts in this paper by means of the following

GENERAL PROPOSITION.

Let Q, R, S, T, &c. fig. 2, pl. 8, represent any variable quantities, expressed in terms of x and y, with given coefficients, and let q, r, s, t, &c. denote as many other quantities, expressed in terms of $\dot{x}$ and $\dot{y}$: it is proposed to find an equation for the relation of x and y, so that the fluent of $\text{Q}q + \text{R}r + \text{S}s + \text{T}t$, &c. corresponding to a given value of x or y, may be a maximum or minimum.

Let AE, AF, and AG, denote any 3 values of the quantity x, having indefinitely small equi-differences EF, FG; and let EL, FM, and GN, perpendicular to AG, be the respective values of y, corresponding to them; and, supposing $\text{EF} = \text{FG} = \dot{x}$, to be denoted by e, let $c\text{M}$ and $d\text{N}$, the successive values of $\dot{y}$, be represented by u and w. Also, supposing $\text{P}'p'$ and $\text{P}''p''$ to be ordinates at the middle points $\text{P}'\text{P}''$, between E, F, and F, G, let the former $\text{P}'p'$ be denoted α, and the latter $\text{P}''p''$ by β; putting $\text{AP}' = a$ and $\text{AP}'' = b$. Then, if a and α, the mean values of x and y, between the ordinates EL and FM, be supposed to be substituted for x and y, in the given quantity $\text{Q}q + \text{R}r + \text{S}s + \text{T}t$, &c. and if, instead of $\dot{x}$ and $\dot{y}$, their equals e and u be also substituted, and the said given quantity, after such substitution, be denoted by $\text{Q}'q' + \text{R}'r' + \text{S}'s' + \text{T}'t'$, &c. it is then evident, that this quantity $\text{Q}'q' + \text{R}'r' + \text{S}'s' + \text{T}'t'$, &c. will express so much of the whole required fluent, as is comprehended between the ordinates EL and FM, or as answers to an increase of EF in the value of x. And thus, if b and β be conceived to be written for x and y, e for $\dot{x}$, and w for $\dot{y}$, and the quantity resulting be denoted by $\text{Q}''q'' + \text{R}''r'' + \text{S}''s'' + \text{T}''t''$ &c. this quantity will, in like manner, express the part of the required fluent corresponding to the interval FG. Whence that part answering to the interval EG will consequently be equal to $\text{Q}'q' + \text{R}'r'$ &c. $+ \text{Q}''q'' + \text{R}''r''$ &c. But it is manifest, that the whole required fluent cannot be a maximum or minimum, unless this part, supposing the bounding ordinates EL, GN

to remain the same, is also a maximum or a minimum. Hence, in order to determine the fluxion of this expression $Q'q' + R'r'$ &c. $Q''q'' + R''r''$ &c. which must of consequence be equal to nothing, let the fluxions of Q' and q', taking α and u as variable, be denoted by $\bar{Q}\dot{\alpha}$ and $\bar{q}\dot{u}$; also let $\bar{R}\dot{\alpha}$ and $\bar{r}\dot{u}$ denote the respective fluxions of R' and r'; and let, in like manner, the fluxions of Q'', q'', R'', r'', &c. be represented by $\bar{\bar{Q}}\dot{\beta}$, $\bar{\bar{q}}\dot{w}$, $\bar{\bar{R}}\dot{\beta}$, $\bar{\bar{r}}\dot{w}$, &c. respectively. Then, by the common rule for finding the fluxion of a rectangle, the fluxion of our whole expression $Q'q' + R'r'$ &c. $+ Q''q'' + R''r''$ &c. will be given equal to $Q'\bar{q}\dot{u} + q'\bar{Q}\dot{\alpha} + R'\bar{r}\dot{u} + r'\bar{R}\dot{\alpha}$ &c. $+ Q''\bar{\bar{q}}\dot{w} + q''\bar{\bar{Q}}\dot{\beta} + R''\bar{\bar{r}}\dot{w} + r''\bar{\bar{R}}\dot{\beta}$ &c. $= 0$.

But $u + w$ being $=$ GN — EL, and $\beta - a = \frac{1}{2}$ (GN — EL) constant quantity, we therefore have $\dot{w} = -\dot{u}$, and $\dot{\beta} = \dot{\alpha}$: also u being $= 2\,rp' = 2\,\alpha - 2$, EL thence will $\dot{u} = 2\,\dot{\alpha}$: which values being substituted above our equation, after the whole is divided by $\dot{\alpha}$, will become

$2\,Q'\bar{q} + q'\bar{Q} + 2\,R'\bar{r} + r'\bar{R}$, &c. $- 2\,Q''\bar{\bar{q}} + q''\bar{\bar{Q}} - 2\,R''\bar{\bar{r}} + r'\bar{\bar{R}}$ &c. $= 0$;

or, $Q''\bar{\bar{q}} - Q'\bar{q} + R''\bar{\bar{r}} - R'\bar{r}$ &c. $= \frac{1}{2}(q'\bar{Q} + q''\bar{\bar{Q}}) + \frac{1}{2}(r'\bar{R} + r''\bar{\bar{R}},)$ &c.

But $Q''\bar{\bar{q}} - Q'\bar{q}$, the excess of $Q''\bar{\bar{q}}$ above $Q'\bar{q}$, is the increment or fluxion (answering to the increment or fluxion $\dot{x}$) arising by substituting b for a, β for α, and w for u. And, with regard to the quantities on the other side of the equation it is plain, seeing the difference of $q'\bar{Q}$ and $q''\bar{\bar{Q}}$ is indefinitely little in comparison of their sum, that $q'\bar{q}$ may be substituted instead of $\frac{1}{2}(q'\bar{Q} + q''\bar{\bar{Q}},)$ &c. which being done, our equation will stand thus:

$$\text{Flux. } R'\bar{q} + R'\bar{r} \text{ &c.} = q'\bar{Q} + r'\bar{R} \text{ &c.}$$

But $q'\bar{Q} + r'\bar{R}$ &c. represents (by the preceding notation) the fluxion of $q'Q' + r'R'$ &c. or of $Qq + Rr$ &c. arising by substituting α for y, making α alone variable, and casting off $\dot{\alpha}$. If therefore that fluxion be denoted by $\dot{v}$, we shall have flux. $Q'\bar{q} + R'\bar{r}$ &c. $= \dot{v}$, and consequently $Q'\bar{q} + R'\bar{r}$ &c. $= v$. But $Q'\bar{q} + R'\bar{r}$ &c. (by the same notation) appears to be the fluxion of $Q'q' + R'r'$ &c. or of $Qq + Rr$ &c. arising by substituting u for $\dot{y}$, making u alone variable, and casting off $\dot{u}$. Whence the following

GENERAL RULE.

Take the fluxion of the given expression (whose fluent is required to be a maximum or minimum) making $\dot{y}$ alone variable; and, having divided by $\ddot{y}$, let the quotient be denoted by v: then take again the fluxion of the same expresssion, making y alone variable, which divide by $\dot{y}$; and then this last quotient will be $= \dot{v}$

When $\dot{y}$ is not found in the quantity given, v will then be $= 0$; and consequently, the expression for $\dot{v}$, equal to nothing also. But if y be absent, then will $\dot{v} = 0$, and consequently the value of $v =$ a constant quantity. It is also easy to comprehend that, instead of $\dot{y}$ and y, $\dot{x}$ and x may be made successively variable. Also, should the case to be resolved be confined to other restrictions,

besides that of the maximum or minimum, such as having a certain number of other fluents, at the same time, equal to given quantities, still the same method of solution may be applied, and that with equal advantage, if from the particular expressions exhibiting all the several conditions one general expression composed of them all, with unknown, but determinate coefficients be made use of.

In order to render this matter quite clear, let A, B, C, D, &c. be supposed to represent any quantities expressed in terms of x, y, and their fluxions, and let it be required to determine the relation of x and y, so that the fluent of $\text{A}\dot{x}$ shall be a maximum, or minimum, when the cotemporary fluents of $\text{B}\dot{x}$, $\text{C}\dot{x}$, $\text{D}\dot{x}$, &c. are all of them equal to given quantities.

It is evident in the first place, that the fluent of $\text{A}\dot{x} + b\,\text{B}\dot{x} + c\,\text{C}\dot{x} + d\,\text{D}\dot{x}$ &c. (b, c, d, &c. being any constant quantities whatever) must be a maximum or minimum in the proposed circumstance: and, if the relation of x and y be determined (by the rule,) so as to answer this single condition, under all possible values of b, c, d, &c. it will also appear evident, that such relation will likewise answer and include all the other conditions propounded. For, there being in the general expression, thus derived, as many unknown quantities b, c, d, &c. to be determined, as there are equations, by making the fluents of $\text{B}\dot{x}$, $\text{C}\dot{x}$, $\text{D}\dot{x}$, &c. equal to the values given; those quantities may be so assigned or conceived to be such, as to answer all the conditions of the said equations. And then, to see clearly that the fluent of the first expression, $\text{A}\dot{x}$, cannot be greater than arises from hence (other things remaining the same) let there be supposed some other different relation of x and y, by which the conditions of all the other fluents of $\text{B}\dot{x}$, $\text{C}\dot{x}$, $\text{D}\dot{x}$, &c. can be fulfilled; and let, if possible, this new relation give a greater fluent of $\text{A}\dot{x}$ than the relation above assigned. Then, because the fluents $b\,\text{B}\dot{x}$, $c\,\text{C}\dot{x}$, $d\,\text{D}\dot{x}$, &c. are given, and the same in both cases, it follows according to this supposition, that this new relation must give a greater fluent of $\text{A}\dot{x} + b\,\text{B}\dot{x} + c\,\text{C}\dot{x} + d\,\text{D}\dot{x}$ &c. (under all possible values of b, c, d, &c.) than the former relation gives: which is impossible; because whatever values are assigned to b, c, d, &c. that fluent will, it is demonstrated, be the greatest possible, when the relation of x and y is that above determined by the general rule.

To exemplify, now, by a particular case, the method of operation above pointed out, let there be proposed the fluxionary quantity $\frac{x^n y^m \dot{y}^p}{\dot{x}^{p-1}}$; where the relation of x and y is so required, that the fluent, corresponding to given values of x and y, shall be a maximum or minimum. Here, by taking the fluxion, making $\dot{y}$ alone variable (according to the rule) and dividing by $\ddot{y}$, we shall have $\frac{p x^n y^m \dot{y}^{p-1}}{\dot{x}^{p-1}} = v$. And, by taking the fluxion a second time, making y alone variable, and dividing by $\dot{y}$, will be had $\frac{m x^n y^{m-1} \dot{y}^p}{\dot{x}^{p-1}} = \dot{v}$. Now from these equa-

tions to exterminate v, let the latter be divided by the former; so shall $\frac{m\dot{y}}{py} = \frac{\dot{v}}{v}$; and therefore $ay^{\frac{m}{p}} = v$ (a being a constant quantity.) Hence $y^{\frac{m}{p}}\dot{y} = (\frac{a}{p})^{\frac{1}{p-1}} \times \dot{x}x^{-\frac{n}{p-1}}$; and consequently $\frac{p}{m+p} \times y^{\frac{m+p}{p}} = (\frac{a}{p})^{\frac{1}{p-1}} \times \frac{p-1}{p-n-1} \times x^{\frac{p-n-1}{p}}$.

Let there now be proposed the two fluxions $x^n y^m \dot{x}$ and $x^p y^q \dot{y}$, the fluent of the former being required to be a maximum or minimum, and that of the latter, at the same time, equal to a given quantity. Then the latter, with the general coefficient b prefixed, being joined to the former, we shall here have $x^n y_m \dot{x} + bx^p y^q \dot{y}$. Hence, by proceeding as before, $bx^p y^q = v$, and $mx^u y^{m-1}\dot{x} + qbx^p y^{q-1}\dot{y} = \dot{v}$. From the former of which equations, by taking the fluxions on both sides, will be had $pbx^{p-1}y^q\dot{x} + qbx^p y^{q-1}\dot{y}$ $(= \dot{v}) = mx^n y^{m-1}\dot{x} + qbx^p y^{q-1}\dot{y}$. Whence $pbx^{p-1}y^q = mx^n y^{m-1}$; and therefore $pby^{q-m+1} = mx^{n-p+1}$. And in the same manner proper equations, to express the relation of x and y, may be derived in any other case, and under any number of limitations.

LXXXVI. On the Alga Marina Latifolia; the Sea Alga with Broad Leaves. By J. A. Peyssonel, M. D., F. R. S. Dated Aug. 4, 1758. p. 631. From the French.

Having cast anchor at Verdun, the road at the entrance of the river of Bourdeaux, Dr. P. was fishing with a kind of drag-net on a bank of sand, which was very fine and muddy. He collected a number of sea-plants, and among them the great broad-leaved alga, which he did not know; and as the root or pedicle of this plant appeared to be very particular, he observed it with attention. The following is its description, and the detail of his observations.

From a pedicle, which is sometimes flat, and sometimes round (for they vary in these plants, and might be about 3 lines in diameter, and an inch high, of a blackish colour, and coriaceous substance, approaching to the nature of the bodies of lithophyta), a single flat leaf arises, about an inch or an inch and half broad, thick in its middle to about 3 lines, ending at the sides in a kind of edge, like a two-edged sabre, almost like the common alga, formed of longitudinal fibres, interlaced with other very delicate ones, and the whole filled with a thick juice, like the parenchyma of succulent plants, such as the sedum, aloes, and the like, of a clear yellowish green, and transparent. This first leaf is always single, and serves instead of a trunk or stem to the whole plant. When it rises to about a foot high, more or less, it throws out at the sides other leaves formed of a continuation of the longitudinal fibres; and these 2d leaves are of the same thickness and substance with the first: they are 2 or 3 feet long, and the whole plant is 5 or 6, or more, for one can hardly tell the length; and is not capable

of supporting itself, but is sustained by the strength of the waters, in which it floats.

The substance of the plant is not so solid as that of the common alga, which is capable of drying as it fades, and of being kept; whereas the leaves of this great alga shrink and wither in the air, become of a blackish colour, and very friable, or indeed soon fall into putrefaction. But what we find particular in this plant is its root or foot: first, this pedicle extends in ribs, like what we call the thighs of certain trees: these thighs are in right lines. They are about 3 or 4 lines high towards the pedicle, and, ending, are lost. They flourish and spread at the bottom, forming an elliptical bladder, like an egg, flattened above and below, and rounded at the sides, being entirely empty: it is rough without, and very smooth within. This egg, or oval bladder, is exactly round at the ends of the great diameter, but varies a little in the less diameter, and forms itself like the body of a fiddle. The under part is a little flattened; and there is a hole, which is very considerable, in the centre of the two diameters. This hole is about an inch wide, and is quite round; it gives passage to the root, or pivot; the edges appear to turn a little inward; and it is by this hole that the egg fills with sea-water. The whole substance of this bladder or egg is of a coriaceous matter, firm and transparent, and of a clear green; nor can there be any fibres, either longitudinal or transverse, observed on it.

The vault at the top, surmounted by the thighs, is as it were granulated; but at the rounding of the egg it produces a kind of mammæ, or little elevations, very round and cylindrical, entirely full, of the same nature and substance with the egg. In examining the under part of the egg were found a 2d rank of these mamellæ, somewhat longer than the first, and at equal distances from each other, in a circular line: then a 3d yet longer: then a 4th, which at the extremities were bifurcated; and at last a 5th rank, which divided into 3, and sometimes into 5 branches: these last, placed round the hole, were wreathed inwards, and several were joined together, and only formed a small body; and in wreathing themselves thus they close and embrace the pivot mentioned below. None of these mamellæ have any apparent opening; their substance is compact, of the same nature with the bladder or egg, that produces them. Below the trunk and thighs the plant protrudes a pivot, of a like substance with that of the bladder. This pivot, which is large at its origin, proceeding thus from the trunk and thighs, forms something like the knot of the sea-tree: it descends perpendicularly to the trunk, diminishing as it lengthens, and as it grows round; and then divides into a number of mamellæ, branched and wreathed inwards so firmly, as not to be retracted; of a coriaceous nature, blackish, forming a bunch like what we call the rose of Jericho. This bunch, or wreathed rose, incloses a heap of gravel, as if petrified or hardened, and ends on a level with the hole of the egg, ex-

actly as high as the last rank of mamellæ, which wreath upon, embrace, and sustain it, leaving always an empty space to let the sea-water pass in, which should fill the inside of the egg or bladder, and even to let in little fishes and shells. He found in one little living muscles, as they always are attached to some solid body by their beards. Now by what means could they enter into this egg? probably they had their beginning there, by the seminal matter of muscles carried in by the sea-water. He also found some small star-fish, whose rays might be about 4 or 5 lines long.

LXXXVII. Of the Distilling Water Fresh from Sea-water by Wood-ashes. By Capt. William Chapman. p. 635.

Some time in September 1757, when Capt. C. had been 10 days at sea, by an accident (off the north cape of Finland) they lost the greatest part of their water. They had a hard gale of wind at S. W. which continued 3 weeks, and drove them into 73° lat. During this time he was very uneasy, as knowing if their passage should hold out long, they must be reduced to great straits; for they had no rains, but frequent fogs, which yielded water in very small quantities. He now blamed himself for not having a still along with him, as he had often thought no ship should be without one. But it was now too late; and there was a necessity to contrive some means for their preservation.

He was not a stranger to Appleby's method: he had also a pamphlet written by Dr. Butler, intitled, An easy Method of procuring Fresh Water at Sea. And he imagined, that soap might supply the place of capital lees, mentioned by him. He now set himself at work, to contrive a still; and ordered an old pitch-pot, that held about 10 quarts, to be made clean: the carpenter, by his direction, fitted to it a cover of fir-deal, about 2 inches thick, very close; so that it was easily made tight by luting it with paste. They had a hole through the cover, in which was fixed a wooden pipe nearly perpendicular. This he should call the still-head; it was bored with an augre of $1\frac{1}{2}$ inch diameter, to within 3 inches of the top or extremity, where it was left solid. They made a hole in this, towards the upper part of its cavity, with a proper angle, to receive a long wooden pipe, which they fixed in it, to descend to the tub in which the worm should be placed. Here again he was at a loss, for they had no lead pipe, nor any sheet lead, on board. He thought, if he could contrive a straight pipe to go through a large cask of cold water, it might answer the end of a worm. They then cut a pewter dish, and made a pipe 2 feet long: and at 3 or 4 trials, for they did not let a little discourage them, they made it quite tight. They bored a hole through a cask, with a proper descent, in which they fixed the pewter pipe, and made both holes in the cask tight, and filled it with sea-water: the pipe stuck without the cask 3 inches on each side. Having now got his apparatus

in readiness, he put 7 quarts of sea-water, and 1 oz. of soap into the pot, and set it on the fire. The cover was kept from rising by a prop of wood to the bow. They fixed on the head, and into it the long wooden pipe abovementioned, which was wide enough to receive the end of the pewter one into its cavity. They easily made the joint tight.

It need not be mentioned with what anxiety he waited for success: but he was soon relieved; for, as soon as the pot boiled, the water began to run; and in 28 minutes he got a quart of fresh water. He tried it with an hydrometer he had on board, and found it as light as river-water; but it had a rank oily taste, which he imagined was given it by the soap. This taste diminished considerably in 2 or 3 days, but not so much as to make it quite palatable. Their sheep and fowls drank this water very greedily without any ill effects. They constantly kept their still at work, and got a gallon of water every 2 hours, which, if there had been a necessity to drink it, would have been sufficient for the ship's crew.

He now thought of trying to get water more palatable; and often perused the pamphlet above-mentioned, especially the quotation from Sir R. Hawkins's voyage, who "with 4 billets distilled a hogshead of water wholesome and nourishing." He concluded he had delivered this account under a veil, lest his method should be discovered: for it is plain, that by 4 billets he could not mean the fuel, as they would scarcely warm a hogshead of water. When, ruminating on this, it came into his head, that he burnt his 4 billets to ashes, and with the mixture of those ashes with sea-water he distilled a hogshead of fresh water wholesome and nourishing. Pleased with this discovery, he cut a billet small, and burnt it to ashes: and after cleaning the pot, he put into it a spoonful of those ashes, with the usual quantity of sea-water. The result answered his expectations: the water came off bright and transparent, with an agreeable pungent taste, which at first he thought was occasioned by the ashes, but afterwards he was convinced it received it from the resin or turpentine in the pot, or pipes annexed to it. He was now relieved from his fears of being distressed through want of water; yet thought it necessary to advise his people not to be too free in the use of this, while they had any of their old stock remaining; and told them, he would make the experiment first himself; which he did, by drinking a few glasses every day without any ill effect whatever. This water was equally light with the other, and lathered very well with soap. They had expended their old stock of water before they reached England; but had reserved a good quantity of that which they distilled. After his arrival at Shields, he invited several of his acquaintance on board to taste the water: they drank several glasses, and thought it nothing inferior to spring water. He made them a bowl of punch of it, which was highly commended.

He had not the convenience of a still, or he should have repeated the experi-

ment for the conviction of some of his friends: for as to himself, he was firmly persuaded, that wood-ashes mixed with sea-water would yield, when distilled, as good fresh water as could be wished for. And he thought, if every ship bound a long voyage was to take a small still with Dr. Hales's improvements, they need never want fresh water. Wood-ashes might easily be made, while there was any wood in the ship, and the extraordinary expence of fuel would be trifling, if they contrived so that the still should stand on the fire along with the ship's boiler.*

LXXXVIII. A Lunar Eclipse observed at Matritus,† *July* 30, 1757. *By the Jusuit Father John Wendlingen.* p. 640. *From the Latin.*

The eclipse began at $9^h\ 47^m\ 34^s$, and ended at $12^h\ 52^m\ 15^s$, true time.

Another lunar eclipse was observed by the same, Jan. 24, 1758. It began at $4^h\ 7^m\ 42^s$, and the total immersion was at $5^h\ 13^m\ 10^s$.

LXXXIX. Observations on a slight Earthquake, though very particular, which may lead to the Knowledge of the Cause of Great and Violent ones, that ravage whole Countries, and overturn Cities. By J. A. Peyssonel, M. D., F. R. S. p. 645. *From the French.*

Dr. P. went to make observations on the natural history of the sea; and when he arrived at a place called the Cauldrons of Lance Caraibe, near Lancebertrand, a part of the island of Grande Terre Guadaloupe, in which place the coast runs N.E. and S.W. the sea being much agitated that day flowed from the N.W. There the coast is furnished with hollow rocks, and vaults underneath, with chinks and crevices: and the sea, pushed into these deep caverns by the force and agitation of the waves, compresses the air, which, recovering its spring, forces the water back in the form of the most magnificent fountains; which cease, and begin again at every great pressure. This phenomenon is common to many places in this island. The explanation of it is easy: but the following is what he particularly observed.

As he walked within about 40 paces from the brink of the sea, where the waves broke, he perceived, in one place, the plants were much agitated by some cause, that was not yet apparent. He drew near, and discovered a hole about 6 feet deep, and half a foot diameter; and stopping to consider it, he perceived the earth tremble under his feet. This increased his attention; and he heard a dull kind of noise underground, like that which precedes common earthquakes.

* The addition of wood-ashes, or any other alkaline substance, is quite unnecessary; sweet water being obtainable from sea-water by simple distillation in clean vessels; provided the degree of heat be duly regulated, and care be taken to stop the distillation when the brine becomes too much concentrated. See note at p. 549, vol. i. of these Abridgements.

† Neither the latitude nor longitude of this place is given.

It was followed by a quivering of the earth; and after this a wind issued out of the hole, which agitated the plants round about. He watched to see whether the motion extended to any distance; but was sensible it did not reach above 3 or 4 paces from the hole, and that no motion was perceived farther off.

He further observed, that this phenomenon never happens till after the 7th wave rolls in; for it is a common thing in this country to find the sea appear calm for some time, and then produce 7 waves, which break on the coast one after another: the first is not very considerable: the 2d is somewhat stronger; and thus they go on increasing to the 7th, after which the sea grows calm again, and retires. This phenomenon of the 7 waves is observed by navigators with great attention, especially at low water, to be the better able to go in or come out the very time that the sea grows quiet. These 7 waves successively fill the caverns, which are all along the coast; and when the 7th comes to open itself, the air at the bottom of the caverns being greatly compressed, acted by its elasticity, and immediately made those fountains and gushings abovementioned; and the water continuing in the caverns, up to the very place of the hole, began to produce that dull noise, caused the emotion or earthquake, and finished with the violent wind forced up through the hole; after which the water retired into the sea, and having no further impelling cause, on account of the waves, rendered every thing quiet again.

XC. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society, by the Company of Apothecaries for the Year 1757, *Pursuant to the Direction of Sir Hans Sloane, Bart. By John Wilmer, M.D.* p. 648.

[This is the 36th annual presentation of this kind, completing to the number of 1800 different plants.]

XCI. An Historical Memoir on a Genus of Plants called Lichen, by Micheli, Haller, and Linneus; and comprehended by Dillenius under the terms Usnea, Coralloides, and Lichenoides, tending principally to Illustrate their several Uses. Communicated by Wm. Watson, M. D., F. R. S. p. 652.

The whole class of mosses was but very little noticed by the revivers of botany in the 16th century: they indeed took some pains to distinguish the particular species mentioned by the ancients, but disregarded almost all the rest. Modern botanists however suppose, that they were but little successful in general in their application of the ancient names to plants: nor is a failure in such attempts to be wondered at, considering the too great conciseness, and frequent obscurity of their descriptions. In the class of mosses, as in many others, the accounts transmitted to us are little more than a scene of uncertainty and confusion. It

is to the moderns we are indebted for the discovery of the far greater number of the plants of this class. In this branch of botany our own countrymen Mr. Ray, Buddle, Dale, Doody, Petiver, and Dr. Morison, Sherard, Richardson, and others, have distinguished themselves: and among foreigners M. Vaillant, Sig. Micheli, and Dr. Haller: but, beyond all Dr. Dillenius has made the most ample discoveries and improvements, of which his elaborate history will ever remain a standing proof.

The word lichen occurs in the writings of Dioscorides and Pliny; and though it may be doubtful, there is yet good reason to apprehend, that Dioscorides meant to describe under that name the very plant, or at least one of the same genus, to which the commentators agreed to affix his description. Since then the name has been variously applied by different authors; on which account it is necessary to premise, that the lichen sive hepatica off, or liverwort of the shops, does not fall under this generical term, as it is now formed by the 3 above-named authors. They comprehend under the term lichen, and Dillenius under those of usnea, coralloides, and lichenoides, the hairy tree moss or usnea of the shops; the muscus pulmonarius, tree lungwort, or oak lungs; the lichen terrestris cinereus, or ash-coloured ground liverwort; the coralline mosses; the cup mosses; horned mosses; the orchel, or Canary-weed; the muscus islandicus of Bartholine; and a multitude of others found on trees, walls, rocks, and stones, in all parts of the world, and in many parts thereof in very great abundance.

Caspar Bauhine, in his Pinax, John Bauhine, and our countrymen Gerard and Parkinson, and their contemporaries, as they wrote before the time that generical characters in botany were in use, included these lichens among the other herbaceous mosses, under the general name of muscus; adding to the name in general some epithet descriptive of its form, place of growth, or supposed virtue. Mr. Ray, both in his History of Plants, and in the supplement, as he was usually averse to the forming of new names, has interspersed them among other mosses, under the character of musci steriles seu aspermi, retaining the synonyms of the two Bauhines, Gerard, and Parkinson, to the general species.

Dr. Morison seems to have been the first, who separated them entirely from the herbaceous mosses; and, from the analogy he supposed they had with the fungus tribe, formed them into a genus, under the name of musco-fungus. He enumerates 50 species and upwards under this term in the Historia Oxoniensis, and has divided them into 5 orders, according to their different appearances, as follows:

1. Musco-fungi e terra prominentes, latiores. 5.—2. Musco-fungi pixidati. 11.—3. Musco-fungi corniculati. 26.—4. Musco-fungi crustæ modo adnascentes. 37.—5. Musco-fungi corticibus arborum dependentes. 53.—Table the 7th of his 15th section exhibits several good figures of some of these lichens.

Tournefort was the first who adapted the generical term lichen to them; but it was in consequence of his joining them to the lichen of the shops. He has however excluded the coralline-mosses, and forms them into a genus, by the name of coralloides; to which he has connected some plants, properly of the fungus tribe. In this distinction he is followed by Dr. Boerhaave in his Index alter Plantarum.

Dr. Dillenius first called them lichenoides in the catalogue of plants growing about Giessen, choosing to retain the word lichen to the liverwort of the shops. Under this name however, in this work, he does not comprehend the usneæ, or hairy-tree mosses, but refers them to the confervæ, adding the epithet arborea to each species, to distinguish them from the water kinds. He enumerates upwards of 60 species of lichenoides, but has applied few or no synonyms to them. Under the same generic term he has introduced them into the 3d edition of Ray's Synopsis of British Plants, taking in the usneæ, and recounting upwards of 90 species, all found spontaneously growing in England. Many of these are doubtless only varieties. They are in this work very naturally divided into several orders and subdivisions, for the greater ease of distinguishing them, as follows:

- Lichenoides
  - caulifera
    - 1. Capillacea et non tubulosa scutellata.
    - 2. Coralliformia tuberculosa plerumque.
      - a. Solida et non tubulosa.
      - b. Tubulosa.
    - 3. Pyxidata.
    - 4. Fungiformia.
  - cauliculis destituta.
    - 1. Mere crustacea.
    - 2. Crusta foliosa scutellata seu foliis scutellatis arcte adnascentibus.
      - a. Substantiæ gelatinosæ.
      - b. Substantiæ durioris.
    - 3. Foliis magis liberis nec tam arcte adnascentibus.
      - a. Scutellatis et tuberculatis.
      - b. Peltatis.

M. Vaillant, in the Botanicon Parisiense, retains Tournefort's names. Many of these lichens, as well as other mosses, are accurately represented in the elegant tables which adorn that work. Dr. Haller tells us he learnt to distinguish almost all the mosses solely by the help of these tables, so well are they expressed. The lovers of botanic science are greatly indebted to Boerhaave for his publication of that work.

Micheli, after Tournefort, adopts the term lichen, and comprehends all the species under it, except one or two, which he calls lichenoides. This author, however does not take into this genus the liverwort of the materia medica; he describes the species of that genus under the name of marchantiæ. Near 20 of the plates in his Nova Plantarum Genera are taken up in representing various species of this genus. In this work they are divided into 38 orders or subdivisions; a circumstance very necessary indeed, considering how greatly he has multiplied the number of the species. It is to be regretted, that so indefatigable an author, one whose genius particularly led him to scrutinize the minuter subjects

of the science, should have been so solicitous to increase the number of species under all his genera: an error this, which tends to great confusion and embarassment, and must retard the progress and real improvement of the botanic science.

Dr. Haller retains Micheli's term, and enumerates 160 kinds in his Enumeratio Stirpium Helvetiæ; he divides them into 7 orders, according to the following titles: 1. Lichenes corniculati et pixidati.—2. Lichenes coralloidei.—3. Lichenes fruticosi alii.—4. Lichenes pulmonarii.—5. Lichenes crustacei scutis floralibus ornati.—6. Lichenes scutellis ornati.—7. Lichenes crustacei non scutati. The extensive number of the species, and the difficulty of distinguishing them with a tolerable degree of certainty, has deterred Dr. Haller from adding so full and complete a list of synonyms to the plants of this genus as he has elsewhere done in that splendid work. Plate the 2d exhibits several elegant sorts of these lichens.

Linneus, and the followers of his method, who seem to have established their generical character from Micheli's discoveries, retain also his generical title. Micheli's passion for the multiplication of species is no where more conspicuous than in the plants of this genus, which he has most enormously augmented to the number of 298 species. The Swedish professor cannot be charged with this foible: it is one of the excellencies of his writings, that they inculcate the reverse. He has so far retrenched this genus, that in his general enumeration of plants he recounts only 80 species belonging to it. They are in this work divided into 8 orders, according to the difference of appearance which they form by their facies externa, little or no regard being had to what are usually called the parts of fructification. 1. Lichenes leprosi tuberculati.—2. Lichenes leprosi scutellati.—3. Lichenes imbricati.—4. Lichenes foliacei.—5. Lichenes coriacei.—6. Lichenes scyphiferi.—7. Lichenes fructiculosi.—8. Lichenes filamentosi.

Dr. Dillenius, in his work, intitled, Historia Muscorum, has divided this Michelian genus into 3, under the names of usnea, coralloides, and lichenoides. Under the word usnea he comprehends the hairy-tree mosses, among which are the usnea of the shops, and the true usnea of the Arabians. Of these he describes 16 species. Under coralloides he describes 39 species, among which are the cup mosses, and many others, disposed according to the following scheme:

Ordo 1. Fungiformia, non tubulosa, nec ramosa, 5.

Ordo 2. Scyphiformia, tubulosa, simplicia et prolifera.

Series 1. Scyphis perfectioribus. 13. Cup mosses.

Series 2. Scyphis imperfectis. 20. Horned mosses.

Ordo 3. Ramosa fruticuli specie summitatibus acutis multifariam divisis.

Series 1. Species tubulosæ. 30. Tubulous coralline mosses.

Series 2. Species solidæ. 39. Solid coralline mosses: among which is the orchel.

The genus of lichenoides contains 135 species, disposed according to the following scheme:

Ordo 1. Species aphyllæ mere crustaceæ. { 1. Tuberculosæ. 8.
2. Scutellatæ. 18.

Ordo 2. Species foliosæ. { 1. Gelatinosæ tuberculosæ et scutellatæ. 35.
2. Aridiores et exsuccæ, scutellatæ. 100.
3. Aridiores peltatæ et clypeatæ. 121.

These plants are not only largely described, and accompanied with the most perfect assemblage of synonyms, but every species is accurately figured, and many of them in various views, and at different ages of their growth; by which this laborious work, notwithstanding it is conversant on the minutest, and consequently the most abstruse parts of botany, may yet be justly esteemed, without any exaggeration, one of the most complete works extant of the kind.

Dr. Hill, in his History of Plants, has disposed them into 5 genera, under the following names: 1. Usnea, comprehending the hairy tree-mosses; 2. Platysma, flat-branched tree-mosses, the lungwort, and others; 3. Cladonia, containing the orchel and coralline-mosses; 4. Pyxidium, the cup-mosses; 5. Placodium, the crustaceous mosses. The plants of this extensive genus are very different in their form, manner of growing, and general appearance: on which account those authors who preserve them under the same name, saw the propriety and necessity of arranging them into different orders and subdivisions, that the species might be distinguished with greater facility. On the same principle Dr. Dillenius and Dr. Hill have formed them into several genera.

So far as the parts of fructification are distinguished in these plants, they appear in different forms on different species; on some, in the form of tubercles; on others, in the form of little concave dishes, called scutellæ; on others, of oblong flat shields or pelts. All these are conceived by Micheli and Linneus to be receptacles of male flowers. The female flowers and seeds are suspected by the same authors to be dispersed in the form of farina or dust on the same plants, and in some instances on separate ones. Dillenius has not dared to determine any thing positive with regard to the real parts of fructification in these lichens: time will hereafter, it is to be hoped, throw more light on the subject.

In order to convey a more distinct idea of the several plants of this genus, which enter into economical or medical uses in the various parts of the world, we shall distribute them into several orders, according to the custom of former writers: and as it is not consistent with our plan to describe each of these species, we shall refer to the page of the more modern authors, where they may be found.

1. Lichenes Filamentosi.

Such as consist of mere solid filaments, of a firm and solid but flexible tex-

ture, having the appearance of fructification in the form of scutellæ, or flat round bodies growing from the sides or extremities of these filaments.

This order or division comprehends the hairy tree-mosses, or usnea of Dillenius and Hill; several of the species of the 5th order of lichens of Micheli; and the lichenes filamentosi of Linneus.

Dr. Dillenius describes 16 species under the term usnea, several of which are found in England, though some of them, as the common usnea of the shops, but very sparingly, and none of them in any considerable plenty. The thick woods in many other parts of Europe, and the rest of the globe, afford them in great plenty. They hang from the branches of various kinds of trees, like large tufts of hair, to a considerable length: some species grow several feet long. The rocks on the tops of high mountains afford several kinds. They are of various colours; some whitish, ash-coloured, others grey or blackish, and 2 or 3 species have a yellow or orange hue.

The commentators in general agreed in making the bryon* of Dioscorides one of these hairy tree-mosses, which they called usnea. No wonder therefore that at the restoration of letters it became a matter of controversy, which of them was the usnea of the ancients. Dioscorides recommends his as an astringent; and tells us, that 'the best grew upon the cedar; but that from whatever tree it was gathered, the whitest and most fragrant was preferable to the black.' The several usneæ would doubtless in different countries be found upon different trees. In Italy, that of the larch-tree was the most odoriferous; and on that account Matthiolus preferred † it to all others. That kind, which at length obtained a place in the shops as the usnea of the ancients, was a species commonly found in our countries on old oaks and other trees, and is called by Dillenius ‡ stringy tree-moss, or usnea of the shops. Many excellent virtues have been ascribed to it, on a supposition of its being the true usnea; but it does not appear to have deserved them: and the present practice, at least in England, has quite expunged it, and that perhaps very justly. Dr. Dillenius is evidently of opinion however, that this common usnea, though it obtained a place in the shops as such, is not the bryon of Dioscorides and Pliny, or the phascon of Theophrastus, since he has applied these names from those fathers of botany to another species, which he calls the beard § usnea. Nor does either of these species appear to be

* Lib i. c. 20. See this subject largely discussed in Bodæus à Stapel Comment. in Theoph. p. 156 et seq.—Orig.

† Opera omnia à C. B. ed. 1598. p. 64.—Orig.

‡ Usnea vulgaris loris longis implexis Hist. Musc. p. 56. Lichen plicatus Lin. Sp. Pl. 1154. Muscus arboreus: usnea officin. C. B. Raii Syn. iii. p. 64.—Orig.

§ Usnea barbata loris tenuibus fibrosis Hist. Musc. p. 63. Lichen barbatus Lin. Sp. Pl. 1155. Quercus excrementum villosum C. B. p. 422. Bauhin took this to be the true usnea Arabum.—Orig.

the true usnea of the Arabians, whatever title they may seem to have to it, either from their colour or smell. Bellonius, as he is quoted by Dr. Dillenius, tells us, 'that the true usnea, or bryon, as he calls it, is sold at Constantinople under the name of usnech; and tells us we are deceived in believing ours to be the true usnea.' Dillenius has therefore described another species,* which he received from the East Indies, from Madegascar, and St. Helen's, as the usnea Arabum. This plant the Indians call saliaga; and Camelli assures us that while fresh it has a very fragrant musk-smell. He adds, that he had himself experienced what Serapio says of it; viz. that a vinous infusion of it restrains fluxes, stops vomiting, strengthens the stomach, and induces sleep. The common usnea of the shops was said to be the basis of that fine perfumed powder, which the French called corps de cypre gris, and which formerly made a great article of trade at Montpelier.

We may here observe by the bye, that the usnea cranii humani, which through the influence of superstition formerly obtained a place in the catalogues of the materia medica, does not belong to this division of the lichens. The writers of those times distinguished two kinds of usnea humana, under the names of crustacea and villosa. Any of the crustaceous lichens, but more properly the common grey-blue pitted lichenoides of Dillenius, was used for the former of these; and as Dale tells us, was held in most esteem. The villosa was a species of the genus of hypnum. Indeed it does not appear that they were in those days very curious in determining the exact kind; and doubtless any moss which happened to grow upon a human skull was sufficient for the purposes designed.

2. Lichenes Fruticulosi.

Such as consist of a tough flexible matter, formed into ramifications, in some species almost simple, in others resembling small shrubs: in some of the species the branches are quite solid, in others tubular.

This order comprehends the third of Dillenius's genus of coralloides; the whole cladonia of Hill; the 2d, and several species of the 3d order of Haller's lichens; several species of the 5th, and the whole 6th, order of Micheli: and the lichenes fruticulosi of Linneus. The plants of this genus grow principally on the ground on heaths, forests, and mountainous barren places; except the orcelle, or Canary-weed, which is found upon the rocks on the sea-coast. To this division belongs the horned moss.† It is found with us in rocky barren ground, and on

* Usnea ceratoides candicans glabra et odorata Hist. Musc. p. 71. Muscus arboreus candicans et odorifer Camelli Raii Hist. iii. Append. p. 3.—Orig.

† Coralloides corniculis longioribus et rarioribus. Dillen. Hist. Musc. p. 103. Muscus corniculatus Ger. p. 1372. Park. 1308. Raii Hist. i. p. 112. iii. p. 28. Lichenoides tubulosum cinereum minus crustaceum minusque ramosum Raii Syn. 3, p. 67.—Orig.

old walls not uncommon. It was formerly in great credit as a pectoral: but is now quite in disrepute.

The common branched coralline-moss* is one of the most useful plants of all the tribe of lichens. It is pretty frequent with us on our heaths, forests, and mountains. The northern regions afford it in abundance; and there it is peculiarly and singularly useful. It is indeed the very support and foundation of all the Lapland economy, and without which the inhabitants could not sustain their rein-deer in the winter time. Linneus tells us† that Lapland affords no vegetables in such plenty as this, and other of the lichens. Plains of several miles extent are totally covered over with it, as if with snow; and where no other plant will even take root, this will thrive and be luxuriant. These dreary and inclement wastes, these terræ damnatæ, as a foreigner would readily call them; these are the Lapland fields and fertile pastures. On this lichen the rein-deer, those sources of all their wealth, feed in the winter time, when it is in its most flourishing condition, and no other vegetable is to be had: with this too they will even become fat. The riches of the Laplanders consist in their number of these cattle: they are cloathed with their skins, fed with her flesh, and from their milk they make both butter and cheese. Nature, by the inclemency of their seasons, has almost denied them the cultivation of their earth: they neither sow nor reap; but live a perpetual migratory life, tending their flocks of rein-deer, on which their whole care is centred and employed. The milk of the rein-deer is very remarkably fat and rich: it tastes indeed like cow's milk, with which some butter, and a small quantity of fat or suet, has been intimately united. Dr. Haller‡ suspects that this richness of the milk is owing to the animal's feeding on this moss. Most of the plants of this family are of an astringent quality, which indeed they manifest to the taste. This astringency of their food will doubtless contribute much to that effect. The reindeer are not the only animals that will feed on the coralline moss. The Novaccolæ § gather vast quantities of it to fodder their oxen with in the winter. They take the opportunity of raking it together in the rainy seasons when it is tough; for in dry weather it easily crumbles into powder. This they moisten with a little water in the winter season when they use it, and find it excellent fodder.

Another of the most remarkable and useful plants of this division is the

* Coralloides montanum fruticuli specie ubique candicans Hist. Musc. p. 107. Lichen rangiferinus Lin. Sp. Pl. 1153. Muscus corallinus. Tab. Ger. em.—Orig.

† Flor. Lappon. p. 332.—Orig.

‡ Enum. Stirp. Helv. p. 69. N° 38.—Orig.

§ The Novaccolæ are a people originally sprung from the Finlanders: they fixed themselves in Lapland not long since, and traffic with the old inhabitants.—Orig.

is the orchel,* or argol, as it is commonly called. This enters more into economical uses among us than any other of the whole genus. How considerable an article it forms in the dyeing trade, in which its uses are various and extensive, is very well known. Its tinging property has been known from ancient times; and some of our most celebrated botanic writers are of opinion, that it was used as a dye even in the days of Theophrastus. That father of botany mentions a fucus, which he says grew on the rocks about the island of Crete; and that they dyed woollen garments of a purple, or rather a red colour, with it. It grows on the rocks by the sea-coast in many parts of the Archipelago, and in the Canary Islands; whence we generally import it, as well as from the Cape Verde, which afford it in plenty. The demand for orchel is so great, that M. Hellot,† of the Royal Acad. of Sciences, informs us, they gather yearly on an average, from the isle of Teneriffe 500 quintals, which amounts to 25 ton weight; from the Canary Islands 400 quintals, from Forteventura 300, from Lancerota 300, the same from Gomera, and from Ferro 800.

The way of manufacturing the orchel for the uses of dyeing, was for a considerable time a secret in few hands; but it is now done in London, and other parts of Europe, to great perfection. Mr. Ray, from Imperatus, gives a brief account of the process.‡ Micheli has since delivered a more exact detail of it. His at least seems to be the method § which the dyers at Florence used. From both these accounts, urine and pot-ash appear to be the principal ingredients used in extracting its colour. Many other plants of this genus contain the same tophaceous matter as the orchel; and on trial have been found to strike a good colour.

3. Lichenes Pyxidati.

Such as consist of a firm tough flexible matter, formed into simple tubular stalks, whose tops are expanded into the form of little cups.

This division contains the cup-mosses of authors; the 2d order of coralloides of Dillenius; great part of the first order of lichens in Haller; the 7th, 8th, 9th, and 10th order in Micheli; and the lichenes schyphiferi of Linneus. Dr. Hill has constituted a genus entirely of these cup-mosses, under the name of pyxidium. They are common with us on heaths, and other dry and barren places. Some of them are proliferous, even to the 3d degree, and form a very

* Coralloides corniculatum fasciculare tinctorium fuci teretis facie Dillen. Hist. Musc. p. 120. Cladonia tophacea Hill. Hist. Pl. p. 93. Fucus capillaris tinctorius Raii Hist. i. p. 74. Lichen (Rocella) fruticulosus solidus aphyllus subramosus tuberculis alternis Lin. Sp. Pl. 1154.—Orig.

† L'Art de la Teinture des Lains et des Etoffes de Lain, Paris 1750, p. 543.—Orig.

‡ Raii Hist. Pl. i. p. 74.—Orig.

§ Nova Plant. Gener. p. 78.—Orig.

beautiful appearance. Some have tubercles on the edges of the cups, of a beautiful scarlet colour. The cup-moss* was a long time in great and established use for coughs, and especially for the whooping cough in children; for which it was long accounted a specific. To this end it was given in various forms. Gerard and Parkinson recommend the powder to be taken for several days together. Dr. Willis was particularly one of its patrons. He has given us † several forms for its exhibition, as that of the powder, a decoction, and a syrup from it. The present practice has quite exploded it, and very justly perhaps, as in any degree specific in the above disorder.

4. LICHENES CRUSTACEI.

Such as consist of a dry and friable matter, more or less thick, formed into flat crusts, very closely adhering to whatever they grow upon.

Some of the species of this division consist of an exceedingly fine thin crustaceous, or rather as Micheli calls it, farinaceous matter, the fructifications appearing in the form of tubercles. Others consist of a thicker scabrous crust, having the fructifications in the form of little cups, called scutellæ. This division contains the first order of the lichenoides of Dillenius; the 5th, 6th, and 7th orders of Haller's lichens; the lichenes leprosi and crustacei of Linneus; and several of the placodium of Hill. The species are numerous, and most of them very common on rocks, stones, old walls, the bark of trees, old pales, &c. which are commonly covered over with them, in undisturbed places. They form a very agreeable variety, and some of them have a very elegant appearance.

Dr. Dillenius describes a species of this order, which he found upon the tops of the mountains in Caernarvonshire in Wales; and which the inhabitants told him they used as a red dye, and found it preferable to the cork, or arcel, which they call kenkerig. He has entitled it in English, the white tartareous scarlet-dying lichenoides.‡ He is of opinion that this is the moss which Martin mentions, in his account of the Western Islands of Scotland, under the name of corkir: with which the inhabitants of the island of Sky dye a scarlet colour. They prepare it by drying, powdering it, and then steeping it for 3 weeks in urine. Linneus queries whether this moss be not the same as his lichen calcareus;§ a species so peculiar to limestone rocks, that wherever that stone occurs among others, it may be distinguished at the first view by this moss growing upon it. This is a singularity which Dr. Dillenius has not mentioned in his moss: on the other hand, Linneus does not mention any tinging property in his.

* Coralloides schyphiforme tuberculis fuscis Hist. Musc. 79. Lichenoides tubulosum pyxidatum cinereum. Raii Syn. iii. p. 68. Pyxidium margine leviter serrato. Hill. Hist. Plant. p. 94.—Orig.

† Willis Pharm. Rational, sect. i. cap. 6, de tussi puerorum convulsiva.—Orig.

‡ Lichenoides tartareum tinctorium candidum tuberculis atris. Hist. Musc. p. 128.—Orig.

§ Lichen (calcareus) leprosus candidus tuberculis atris Spec. Plant. 1140.—Orig.

With regard to these crustaceous mosses in general, it is worthy our regard, that in the economy of nature they answer singular and important uses. To an unobserving eye, no class of vegetables may appear more insignificant, or less adapted to advantageous purposes in the creation than these. This vulgar estimation of things is frequently erroneous; and it is certainly so in the instance before us. These minute and seemingly insignificant mosses serve, under some circumstances, to valuable purposes. No sooner is a rock left bare by the sea, but these lichens lay the foundation for its future fertility. Their seeds, which are presently brought thither by the winds, soon cover it all over. These corrupting, presently afford a soil sufficient to nourish other smaller mosses; which in their turn form one deep enough for larger plants and trees; and thus the rock becomes a fertile island.*

5. Lichenes Foliacei Scutellati.

Such as consist of a more lax and flexible matter, formed into a foliaceous appearance, having the parts of fructification in the form of scutellæ.

Some of the plants of this division are interspersed with the former in some of the systems of botanic authors. In general this division contains the whole first series of the 2d order of lichenoides in Dillenius; the first division of the 2d series, and the latter part of the 2d division, of the same: it comprehends the lichenes imbricati and umbilicati of Linneus; and many of the placodium of Hill. The plants of this order are many of them not less common in England than the foregoing, on rocks, stones, old pales, trees, &c. Some adhere very closely to what they grow upon, and seem to be only foliaceous about the edges: others adhere but loosely, and are much expanded and divaricated, so as to form something like ramifications.

It was remarked, from Linneus's observation, that one of the crustaceous lichens was scarcely ever found growing but upon limestone rocks. On the contrary, the same author has observed of a foliose lichen belonging to this order, that it will thrive on all kinds of rocks except limestone rocks. This species† Dillenius calls the common grey-blue pitted lichenoides. It is very common with us upon trees, old wooden pales, &c. as well as upon rocks and stones. It is the usnea cranii humani of the old materia medica. Linneus adds, that it will dye a purplish colour.

Hither likewise must be referred the cork or arcel,‡ which is used by the

* Vide Econom. Natur. in Amæn. Acad. vol. ii. p. 17.—Orig.

† Lichenoides vulgatissimum cinereo-glaucum lacunosum et cirrhosum Hist. Musc. p. 88. Lichenoides crusta foliosa superne cinereo-glauca, inferne nigra et cirrhosa scutellis nigricantibus. R. Syn. p. 72.—Orig.

‡ Lichenoides saxatile tinctorium foliis pilosis purpureis Raii Syn. p. 74, No. 70. Hist. Musc. p. 185. Lichen petræus purpureus Derbiensis Park. Theat. p. 1315. Lichen omphalodes Lin. Spec. Pl. 1143.—Orig.

Scotch and others to dye a purple or scarlet colour. The preparation of it is by powdering, and making it into a mass with urine. Parkinson tells us * the poor people in Derbyshire scrape it from the rocks, and make the same use of it. Mr. Ray † adds to this account, that the Welsh, who call it kenkerig, have long been acquainted with this property, and have it in common use. The colour from this moss is but very dull; but if the same methods were taken to improve it as have been with the orchel, it would doubtless be rendered much better, and more durable. Linneus relates ‡ that there is an immense quantity of this moss about the rocks of the isle of Aland in the Baltic; where the good women themselves make a yellow dye with it from a simple decoction of the plant, without the addition of any saline article. He adds, that those who would heighten the colour, add a small quantity of roucou § to the decoction.

6. Lichenes erecti ramosi plani.

Such as consist of a firm tough matter, disposed into flat and thin ramifications growing erect, and bearing their scutellæ on the edges, surfaces, and at the extremities.

This division comprehends the flat branched tree-mosses of authors; many of the 4th order of Haller's lichens; the first part of the 2d division of series the 2d in Dillenius; and the platisma of Hill. The plants of this division grow upon old trees, especially in thick and unfrequented woods; some of them on rocks: they are many of them extremely common in England on all kinds of trees. As they were some of the most obvious, so they were some of the first lichens noticed by the old writers, by whom they were called lichenes arborum. The mosses of this order were substituted instead of the usnea in the composition of the pulvis cyprius. The very species, which was most frequently used for this purpose, was the channel-leaved lichenoides of Dillenius, ‖ on account of its being easily reduced into a fine powder, of a good white colour. Yet others are doubtless as well adapted to the same purposes: and if it was of importance enough to employ them to any purposes of the like nature in our own country, they might be procured in sufficient plenty.

One of the plants of this order is applicable to the same uses as the Canary-weed, and is reckoned not much inferior to it: and as it is found in the same places, it is very often packed up with it in considerable quantities. Dillenius

* Park. Theat. Botan. p. 1315.—Orig. † Raii Hist. Plant. p. 116.—Orig.

‡ Flor. Lappon. p. 343. V.—Orig. § Otherwise called arnotto.—Orig.

‖ Lichenoides coralliforme rostratum et canaliculatum. Hist. Musc. 170. Lichenoides arboreum ramosum angustioribus cinereo-virescentibus ramulis. Raii Syn. 75. Lichen calicaris Lin. Spec. Plant. 1146.—Orig.

calls it the flat dyers lichenoides with longer and sharper horns. * It is truly and properly a plant of the lichen genus, though the older writers of the last century called it a fucus. They were led into this mistake by its having flat ramifications, and from its growing on the rocks by the sea-side. It is found in the East Indies upon trees, and is frequent on the coasts of the Mediterranean, as well as about the Canary Islands.

7. LICHENES PELTATI.

Such as consist of a tough or coriaceous matter, disposed into a foliaceous appearance; on the edges of which, in general, the parts of fructification are placed, in the form of flattish oblong bodies, in these mosses called shields or pelts.

This division contains the 3d series of the 2d order of Dillenius's lichenoides; the lichenes coriacei of Linneus; and several of the placodium of Hill.

That celebrated and well-known plant, the ash-coloured ground liverwort † of Ray belongs to this order. It is very common all over England on dry and barren ground; and indeed almost all Europe, and America too, seems to afford it in sufficient plenty, as we find it observed by almost all the botanic writers since Ray, who was one of the first that described it. The earliest account we have of its use for the bite of a mad dog is in the Phil. Trans. vol. 20, p. 49; (or vol. iv, p. 232 of these Abridgments) from Mr. Dampier, in whose family it had been a secret for a number of years. It was communicated first to Sir Hans Sloane, as a kind of fungus, or Jew's-ear; and at the request of Dr. Mead was some years afterwards received into the London Dispensatory. Scarcely any of the boasted specifics of former ages ever acquired so great reputation as this plant has done in modern times, for its prevalence against the bite of a mad dog; and the patronage of the late learned Dr. Mead made it sufficiently known throughout all the world. Happy would it be indeed if it fully deserved the high encomiums which have been bestowed on it. A great and eminent physician ‡ has doubted its efficacy at all in such cases; and it is well known that Boerhaave even laughed at it. Dr. Mead had certainly a high opinion of it: he tells us it never failed, through the course of 30 years experience, where it was duly given before the hydrophobia came on.§ Later instances have showed that it is not infallible, and Dr. Van Swieten's supposition is but too likely to prove

* Lichenoides fuciforme tinctorium corniculis longioribus et acutioribus, Hist. Musc. 168. Platysma corniculatum, Hill Hist. Plant. 90. Lichen fuciformis, Lin. Sp. Pl. 1147.—Orig.

† Lichenoides digitatum cinereum lactucæ foliis sinuosis, Dillen. Hist. Musc. 200. Platysma sinuosum scutellis ovato-rotundis, Hill Hist. Pl. 89. Lichen caninus, Lin. Sp. Pl. 1149.—Orig.

‡ Dr. Van Swieten. See Comment. in Boerh. Aphor. § 1147.—Orig.

§ Mechanical Account of Poisons, ed. 4th, p. 156.—Orig.

true. It must be confessed that Dr. Mead's exhibition of it seems too much complicated with other means to leave room for judging fully of its real efficacy; and it may really be questioned whether bleeding, pepper, and cold bathing, have not had more to do in the case than the lichen.

The muscus pulmonarius officinarum,* tree-lungwort, or oak-lungs, belongs to this order. It is found about old oaks, and on rocks and stones overgrown with moss, in many of our thick woods in England; but not in any great plenty. Few perhaps of the antiquated simples were in more repute in their day than this plant. It was celebrated for ages, on account of its supposed prevalence in pulmonary complaints of almost all kinds; and yet on inquiry into the original of its use in such cases, it would probably appear that it arose more from a fancied resemblance they found in the plant to the lungs themselves, than from any real and well-grounded proofs of its efficacy. As a gentle astringent, like most other species of the family, it would doubtless contribute to relieve in many cases where the lungs were affected, as in hæmoptoës, and some others: but it does not seem by any means to deserve that high character in medicine which has been given to it.

Conclusion.

Dr. W. remarks, by way of conclusion, that we have in this genus of plants a convincing instance of the utility which may result from the study of natural science in general, and even of its minuter and hitherto most neglected branches. From a view of the foregoing memoir it is evident that the economical uses of the lichens, in the various parts of the world, are already very considerable and important; and though it does not appear that the sensible qualities of any of them, or the experience of former ages, will warrant our ascertaining any singular powers to them in a medicinal way, yet posterity will doubtless find the means of employing them to many valuable purposes in human life to us unknown.

XCII. On the Fossil Bones of an Alligator, found on the Sea-shore, near Whitby in Yorkshire. By Capt. Wm. Chapman. p. 688.

In Jan. 7, 1758, was discovered on the sea-shore, about half a mile from Whitby, part of the bones of a large animal. The ground they laid in was what they call alum-rock; a kind of black slate, that might be taken up in flakes, and was continually wearing away by the surf of the sea, and the washing of stones, sand, &c. over it every tide. The bones were covered 5 or 6 feet with the water every full sea, and were about 9 or 10 yards from the cliff, which was nearly perpendicular, and about 60 yards high, and was continually wearing

* Lichenoides pulmonium reticulatum vulgare marginibus peltiferis. Dill. Hist. Musc. 212. Lichenoides peltatum arboreum maximum, Raii Syn. p. 76. Musc. pulmonarius C. B.—Orig.

away, by the washing of the sea against it; and to judge by what had happened in his own memory, it must have extended beyond these bones less than a century ago. There were several regular strata or layers of stone, of some yards thickness, that ran along the cliff, nearly parallel to the horizon and to each other. He mentioned this to obviate an objection, that this animal might have been upon the surface, and in a series of years might have sunk down to where it lay; which would appear impossible, at least when the stones, &c. had taken their present consistence.

Where there is only the superior maxilla remaining, there are no teeth; but the sockets are visible and deep, and at the same distanees from eaeh other as the teeth in the other part of the jaw. The tip or extremity of the bill was entire for 4 or 5 inehes, having both maxillæ, with their teeth, and towards the point large fangs. Part of the bill and head were eovered with the roek.

There were 10 vertebræ, from 3 to 4 half inehes in diameter, and about 3 inehes long, some of them separated in taking up. They were about 2 inches in the rock. There was observed something like bone to streteh from the vertebræ, and intending to take it up whole, begun to cut at what they thought a proper distance; but found they cut through a bone; and with the vertebræ brought up 3 or 4 inehes of the os femoris, with the ball, eovered with the periosteum: but the animal had been so crushed hereabouts, that they could make little of the socket or os innominata. Several of the ribs eame up with the vertebræ: they were broken, and laid parallel to the vertebræ; but not quite close, there being some of the rock between them. The periosteum is visible on many of the bones. There were 12 vertebræ remaining in the rock, with whieh they are almost covered, especially towards the extremity.

The place where these bones lay, was frequently eovered with sea-sand, to the depth of 2 feet, and seldom quite bare; which was the oeeasion of their being rarely seen: but being informed that they had been discovered by some people 2 or 3 years before, they had one of them with them on the spot, who told them that when he first saw it, it was entire, and had 2 short legs on that part of the vertebræ wanting towards the head. Though they could not suspect the veracity of this person, they thought he was mistaken; for they had hitherto taken it for a fish. But when they took it up, and found the os femoris above mentioned, they had cause to believe his relation true, and to rank this animal among those of the lizard kind: by the length (something more than 10 feet) it seemed to have been an alligator.

3

XCIII. Description of a Rare Species of Orthocératites, in a Letter from Dr. Nicholas de Himsel, to William Watson, M. D., F. R. S. From the Latin. p. 692.

A species of orthoceratites found in Kelwick near Fulham. The fragments were imbedded in a grey calcareous stone, and the specimens were of various sizes.

Fig. A, pl. 8. A portion of an orthoceratites, the lower part of which adheres to the stone; it consists of calcareous stone, and gradually decreases to the top. Although from the fragments we might suppose the orthoceratites to be a cylinder, yet the fragments when joined, shew it to be a cone, and it seems to me, from the thickness of its syphon, that this orthoceratites must have been more than two feet long. I have seen a specimen in another calcareous stone found at the same place, of two feet and a half length, but it was so brittle that it was not possible to get it out entire. This portion of orthoceratites is coated by its surrounding shell at a b c. Five joints are visible, which were once the cavities or thalami, and are situated close together, and through them passes on one side a thickish siphon or tube, which in almost all the orthoceratites in my possession is always remote from the centre, but here is situated at the periphery. The siphon lessens gradually, from which circumstance we may judge of the length of the cone of this specimen. The inner part of the crust or shell a b c which covers the joints, is crystalline, consisting of small irregular sparry crystals.—Fig. B. Another portion in which the joints are thinner, and the siphon placed at the periphery, covered with its shell at a. At b is seen the shelly septum, which proceeding from the superior joint invests the part r s of the siphon.—Fig. B. C. A portion of an orthoceratites of grey calcareous stone, but the siphon consists of sparry fluor finely crystalized.—Fig. C. Another portion divided through the axis, and with the outer part covered by the shell, marked with slight circular striæ.—From the interior part fig. D its internal conformation appears, The smaller siphon x z, placed between the centre and the periphery, appears freed from its surrounding joint half way, the remaining half being covered by the lower joint c d e. The joint a b is filled up by pellucid polygonal crystallized spar; but the joint c d e is filled by flesh-coloured sparry stone. The superior part of the siphon is shown at x, where the rays going from the circumference to the centre are visible. Between the sparry crystals in the joint a b appear black streaks here and there, which are filled with asphaltum: so likewise the cavity of the joint a b, as well as the convex surface of the joint c e, which is received into the first concavity, are coated by a lamina of asphaltum.—Fig. E. Another part of a larger orthoceratites, shewing its exterior.—Fig. F. Its interior as divided through the middle, with a pretty thick siphon. a b c and the other slight streaks are the diaphragms separating the joints a b which constitute the siphon, and surround the thinner membrane of the siphon, which is still apparent at o and p. These diaphragms are thicker in the vestiges of the siphon, and are placed on each other at their lower part. The siphon in this specimen is situated between the centre and periphery.—Fig. G. The striated calcareous matter which filled the siphon, and its interior part, looking toward the centre opposite the periphery.—Fig. H. The posterior side, nearer the periphery.—Fig. I. The concave part of a large joint in which the whitish particles of the testaceous diaphragm now crystallized m n r are seen, with the siphon passing through them.—Fig. K. Another portion of a large orthoceratites, shewing its external appearance, with the siphon g placed at the periphery. The diaphragms are here seen proceeding on each side from the joints, joining each other, and concealing the siphon from the exterior part. A is a small portion of the thickish shell, with which this orthoceratites is still covered.

XCIV. *A Further Account of the Effects of Electricity in the Cure of some Diseases.** *By Mr. Patrick Brydone.* p. 695.

A young woman of Aiton had her right leg drawn back by a contraction of the muscles that bend the knee, so that she had not been able to put that foot to the ground for near 12 months. She had taken the advice of some surgeons in the country, and had used several remedies to no purpose. At last, hearing the cure of the paralytic woman, whose case Mr. B. sent some time before, she insisted on being brought hither; and underwent a course of electrical shocks for nearly 2 months, receiving every day at least 50 or 60 in the following manner. She sat close by the machine, and grasping the phial in her hand, she presented the wire to the barrel or conductor, and drew the sparks from it for about $\frac{1}{2}$ a minute. The phial being thus charged, she then touched her knee with the wire, and thus received such severe strokes, as would sometimes instantly raise a blister on the part. The joint was at last so much relaxed, as that she could walk home with the help of a crutch, though her leg was so weak that she had very little use of it. After she had continued in this state for some weeks, she was advised to use the cold bath: but that soon brought back the contraction; and he had been since informed that she was worse than ever.

A soldier's wife of about 30 years of age, was seized with a slight palsy, about Newcastle, on her way to this country: but before she got to this place, she had lost all the feeling in her left side, and so far the power of it, that she was brought to Mr. B. in a cart. After receiving 600 strokes from the electrical machine in the usual way, and in the space of 2 days, she recovered the use of her side, and set out on foot to make out the rest of her journey. However, for fear of a relapse, he gave her a recommendatory letter to Mr. Sommer, surgeon at Haddington, as she was to pass through that town, and as he knew that he was likewise provided with an electrical apparatus.

A young woman from Home, a village in Berwickshire, complained of a coldness and insensibility in her left hand and wrist, of two years standing. When Mr. B. felt that hand, it was as cold as stone, while the other was sweating; and she told him that it never had been warmer all that time. He made her draw the sparks from an egg (which for some other purpose was suspended by a wire from the conductor) for about half an hour; and at the end of that time he found the dead hand in a far greater sweat than the other. She then wrapt it up in a piece of flannel, as she used so do, and retired. Next day she told him, that since the operation she had been able to put off and on her cloaths without help, which she had not been able to do for a 12 month before. She was again electrized; and believing she was then quite well, she went away: but some weeks

* For the first account, see p. 163 of this vol. of these abridgments.—Orig.

after the coldness of her hand beginning to return, she made him another visit, was again electrized, and was dismissed a 2d time apparently cured. This was about 2 months before, and he had heard nothing of her afterwards.

He adds further, that several persons had been relieved of rheumatic pains, by electrizing the parts effected. And a woman was cured of a deafness of 6 months standing, contracted, as she imagined, by cold. This woman held the phial in her hand, while another person standing on a cake of resin gave her the shock, by putting the end of the wire into her ear. This manner of electrizing brought always on a profuse sweat over the head, which was encouraged by wrapping it up in flannel. The first day she came here to Mr. B. she could scarcely hear what was spoken by those about her; but in 5 days she seemed to be perfectly cured.*

XCV. An Account of the Black Assize at Oxford, from the Register of Merton College in that University. Communicated by John Ward, LL.D. With some additional Remarks. p. 699.

Anno nono D. Bickley Custodis, 1577.†

Vicessimo‡ primo Julii in vestiario Dn̄us custos et octo Seniores dispensarunt cum Decreto de concione et appictantia habendis, die Dominico post festum S^ti^ Petri ad vincula, ne vocata et conveniente turba, morbus ille, qui ante quinque dies quamplurimos infestarat, dissipatior et periculosior fiat. Etenim 15, 16, et 17, hujus Julii aegrotant plus minus trecenti homines; et infra duodecim dierum spatium mortui sunt (ne quid errem) centum scholares, praeter cives non paucos. Tempus sine dubio calamitosissimum et luctu plenum. Nam quidam lectos disserentes,§ agitati nescio quo morbi et doloris furore, suos custodes baculis caedunt et abigunt; alii per areas et plateas insanientium more circumcursant; alii in profundum aquarum praecipites insiliunt; nemo tamen, summo Deo gratia, desperanter perit. Franguntur omnium animi. Fugiunt medici, non propter necessitatem fratrum, sed propter se et cistas creati. Relinquuntur miseri. Domini, doctores, et collegiorum praefecti, ad unum pene omnes abeunt. Custos noster; longe omnium vigilantissimus, domi apud nos manet; in aegrotis omnem curam, laborem, diligentiam impensus‖ collocat; die toto, et nocte etiam intempesta, eos sedulo invisit. Moriuntur e nostris quinque. Omnis aula, omne collegium, aut domi, aut in via ad patriam, suos habet mortuos. Mirari quis posset multitudinem ad medicastrorum domos cum matulis citato cursu properantium. Pharmacopolarum etiam conservata, syrupos, olea, aquas

* To these cases are subjoined the attestations of the patients themselves, and of the minister of the parish.

† It was judged proper to reprint this curious historical document in the original latin.

‡ Sic in regist. § Sic in regist. ‖ Sic in regist.

dulces, pixides, cujusque generis confectiones, brevissimo tempore exhausta. Laborant aegroti vehementissimo tum capitis tum stomachi dolore; vexantur phrenesi; privantur intellectu, memoria, visu, auditu, et caeteris etiam sensibus. Crescente morbo, non capiunt cibos, non dormiunt, ministros aut custodes non patiuntur. Semper, vel in ipsa morte, mirae eorum strenuitas et corporis robur; et eo declinante, omnia modis impense contrariis eveniunt. Nulli complexioni aut constitutioni parcitur; cholericos tamen praecipue hic morbus molestos habet; cujus ut causas, sic et curas ignorant medici. Natum suspicantur multi, vel ex foetida et pestilenti furum e carceribus prodeuntium aëre (quorum duo vel tres sunt ante paucos dies in vinculis mortui) vel ex artificiosis diabolicis et plane papisticis flatibus e Lovaniensi barathro excitatis, et ad nos scelestissime et clam emissis. Nam illi solum et hic et alibi decumbunt aegroti, qui in castro, et guilda, quam appellant, aula, quinto et sexto hujus mensis adsunt.* Assisiorum judices, dominus Robertus Bell, capitatis baro scaccarii etc. qualem hactenus non peperit Anglia; dominus Johannes Barrham, dominae reginae serviens ad legem; papisticae pravitatis uterque apertissimi hostes et acerrimi vindices: vicecomes Oxoniensis comitatis†, equites aurati duo, armigeri et pacis justiciarii octo, generosi plures, horum non pauci famuli, omnes (uno aut altero exceptis) de grandi, ut loquuntur, jure, statim post fere relictam Oxoniam mortui sunt. Et ut quisque fortissimus, ita citissime moritur. Foeminae non petuntur, nec certe pauperes; neque etiam inficitur quisquam, qui aegrotorum necessitatibus subministrarit, aut eos inviserit. Sed ut fuit morbus hic insigniter violentus, ita neque diu duravit. Nam infra unius mensis curriculum ad pristinam pene sanitatem restituuntur omnes; ut jam denuo mirari possis tot scholares, tot etiam cives, urbem et plateas linteis capitibus obambulantes, et nomen clementissimi Dei nostri in omne aevum suspicere.‡

Vicessimo quarto Julii Joannes May, socius et artium magister, in collegio vitam finit. Sepelitur in ecclesia. Vicessimo septimo ejusdem Browne clericus moritur in collegio. Vicessimo octavo ejusdem Gaunte portionista moritur in collegio. Vicessimo nono Dnus Lea, electus probationarius 20 Julii, moritur in collegio.

Additional Remarks, by Tho. Birch, D.D., Sec. R. S. p. 702.

Camden, in his Annals of Queen Elizabeth,§ observes, that almost all, except women and children, who were present at the assizes at Oxford, at the trial of Rowland Jenkes, a Bookseller there, for seditious words, died, to the number of about 300. Mr. John Stow, in his Chronicle of England,|| enlarges

* Sic in regist. † Sic in regist. ‡ Sic in regist.

§ Page 285, edit. Lugd. Batav. 1625. || Page 681, edit. London, 1631.

this number, and affirms, that there died in Oxford 300 persons, and in other places 200 and odd, from the 6th of July to the 12th of August; after which died not any of that sickness; for one of them infected not another: and this historian agrees with Camden, that not any one woman or child died thereof. Dr. George Ethryg, a physician, who practised at that time at Oxford,* in the 2d book of his Hypomnemata quædam in aliquot Libros Pauli Æginetæ, seu Observationes Medicamentorum, quæ hâc ætate in usu sunt, printed at London in 1588, in 8vo, mentions, that on the first night of the appearance of the disease about 600 fell sick of it; and that the next night 100 more were seized in the villages near Oxford. Lord Bacon, in his Natural History, evidently refers to this, and one or two more instances of the same kind in the following passage, Century x. N° 914. "The most pernicious infection next the plague is the smell of the gaol, where prisoners have been long and close and nastily kept; whereof we have had in our time experience twice or thrice, when both the judges that sat upon the gaol, and numbers of those that attended the business, or were present, sickened upon it and died. Therefore it were good wisdom, that in such cases the gaol were aired before they be brought forth." We have likewise an account in Mr. Anthony Wood,† that at the quarter-session at Cambridge, in Lent, in the year 1522, and the 13th of the reign of Henry the 8th, the justices, gentlemen, and bailiffs, with most of the persons present, were seized with a disease, which proved mortal to a considerable number of them; those who escaped having been very dangerously sick. With regard to the unhappy instance of the same kind of contagion, which happened at the session in the Old Bailey, in May 1750, see Dr. Pringle's excellent work, intitled, Observations on the Diseases of the Army in Camp and in Garrison.‡

XCVI. A Description of the Plan of Peking, the Capital of China; sent to the Royal Society by Father Gaubil, è Societate Jesu. Translated from the French. p. 704.

King che. The Court.—In this plan are the inclosures of walls, which form as it were three cities. The 1st is the imperial palace, or imperial city. It is called Kong tching or Tse kin. The 2d inclosure is Hoang tching. The 3d inclosure is King tching, or Royal City. Maps and descriptions of this being to be met with in other books; any further account of it is omitted as unnecessary here.

* Wood Hist. et Antiqu. Universit. Oxon. lib. i. p. 295, and Athen. Oxon. vol. i. col. 237.
† Hist. et Antiquit. Universit. Oxon ubi supra. ‡ Page 290, 2d edit.

XCVII. An Attempt to Improve the Manner of Working the Ventilators by the Help of the Fire-Engine. By Keane Fitzgerald, Esq. F.R.S. p. 727.

Dr. Hales wishing to extend his useful ventilators to those who work in mines at great depths under ground, where the lives of many are lost by noxious vapours, occasioned by the want of a free circulation of air; and finding by experience, that ventilators worked by wind do not operate above one third part of the year, and in calm hot weather, when most wanted, do not operate at all; he applied to Mr. Fitzgerald for assistance in contriving a machine to work the ventilator by the help of the fire-engine, which is now generally used in all mines for drawing off the water: and which he accordingly attempted, and hoped it would answer the purpose.

As the lever of the fire-engine works up and down alternately, and performs at a common medium about a dozen strokes in a minute, it was necessary to contrive some way to make the beam, though moving alternately, to turn a wheel constantly round one way, and also to increase the number of strokes to 50 or 60 in a minute. The model of a machine for this purpose is composed of 4 wheels of different sizes, 2 clicks, 3 pinions, and a fly; which is put into motion by the part of a wheel fixed to the arch of a lever of the fire-engine. The wheel which is turned by the lever, or rather moved up and down by it, is loose on its arbor; and likewise one of the rochets, and the wheel next to it. The outside rochet and outside wheel are fixed on the arbor. There are two pinion-wheels fixed on the arbor; one on each side, near the edge of the wheel moved by the lever which turns them. There are also two clicks; one fixed to the great wheel, the other to the frame. These exclusive of the wheel that moves the fly.

The effect is, when the lever moves the wheel downwards, its click forces the rochet fixed on the arbor to move along with it, and the other wheels the same way. When it moves upwards the click fixed on the frame stops the larger rochet, and the wheel next to it, which are pinned together. This wheel being stopped, and the great wheel carried upwards by the lever, the pinion towards the edge of the great wheel is forced round it, and moves the pinion on the other side the great wheel; which pinion moves the wheel fixed on the arbor, the contrary way to the great wheel, which is carried upwards by the lever. By which means the arbor is constantly turned the same way, when the lever of the fire-engine is moved either upwards or downwards.

Upon the arbor there is also another great wheel fixed, which turns a pinion: on the arbor of which pinion is a crank to move the ventilator, and also a fly fixed to the end, to help the motion of the crank, which in the model is turned 3 times for each stroke of the lever, and may be increased or diminished, ac-

cording to the number of teeth in the pinion. The number of teeth in the great wheel moved by the lever is 66; but need not have teeth above half way round. The wheel fixed to the rochet has 33 teeth, and its pinion 11. The wheel fixed on the arbor on the outside, has 24 teeth, and its pinion 16. The wheel which turns the fly has 90 teeth, and the pinion turned by this wheel 10. The greater the number of teeth in the rochets, the better. This machine may also be applied to other useful purposes at mines; and it may be easily made to turn a mill to grind corn; or to turn a wheel to raise coals, or whatever else is wanted to be raised from the mines.

XCVIII. Of some Experiments concerning the Different Refrangibility of Light. By Mr. John Dollond. With a Letter from James Short, M. A., F.R.S. p. 733.

Mr. Short's introductory letter is as follows:

I have received the inclosed paper from Mr. Dollond, which he desires may be laid before the Royal Society. It contains the theory of correcting the errors arising from the different refrangibility of the rays of light in the object-glasses of refracting telescopes; and I have found on examination, that telescopes made according to this theory are entirely free from colours, and are as distinct as reflecting telescopes. The following by Mr. Dollond:

It is well known, that a ray of light, refracted by passing through mediums of different densities, is at the same time proportionally divided or spread into a number of parts, commonly called homogeneal rays, each of a different colour; and that these after refraction, proceed diverging: a proof that they are differently refracted, and that light consists of parts that differ in degrees of refrangibility. Every ray of light passing from a rarer into a denser medium, is refracted towards the perpendicular; but from a denser into a rarer one, from the perpendicular; and the sines of the angles of incidence and refraction are in a given ratio. But light consisting of parts which are differently refrangible, each part of an original or compound ray has a ratio peculiar to itself; and therefore the more a heterogene ray is refracted, the more will the colours diverge, since the ratios of the sines of the homogene rays are constant; and equal refractions produce equal divergencies. That this is the case when light is refracted by one given medium only, as suppose any particular sort of glass, is out of all dispute, being indeed self-evident; but that the divergency of the colours will be the same under equal refractions, whatever mediums the light may be refracted by, though generally supposed, does not appear quite so clearly.

However, as no medium is known which will refract light without diverging the colours, and as difference of refrangibility seems thence to be a property inherent in light itself, opticians have on that consideration, concluded, that equal

refractions must produce equal divergencies in every sort of medium: whence it should also follow, that equal and contrary refractions must not only destroy each other, but that the divergency of the colours from one refraction would likewise be corrected by the other; and there could be no possibility of producing any such thing as refraction, which would not be affected by the different refrangibility of light; or, in other words, that however a ray of light might be refracted backward and forward by different mediums, as water, glass, &c. provided it was so done that the emergent ray should be parallel to the incident one, it would ever after be white; and conversely, if it should come out inclined to the incident, it would diverge, and ever after be coloured. From which it was natural to infer, that all spherical object-glasses of telescopes must be equally affected by the different refrangibility of light, in proportion to their apertures, whatever material they may be formed of.

But it seems worthy of consideration, that notwithstanding this notion has been generally adopted as an incontestable truth, yet it does not seem to have been hitherto so confirmed by evident experiment, as the nature of so important a matter justly demands; and this it was that determined Mr. D. to attempt putting the thing to issue by the following experiment. He cemented together two plates of parallel glass at their edges, so as to form a prismatic or wedge-like vessel, when stopped at the ends or bases; and its edge being turned downward he placed therein a glass prism with one of its edges upward, and filled up the vacancy with clear water; thus the refraction of the prism was contrived to be contrary to that of the water, so that a ray of light transmitted through both these refracting mediums would be refracted by the difference only between the two refractions. Therefore, as he found the water to refract more or less than the glass prism, he diminished or increased the angle between the glass plates, till he found the two contrary refractions to be equal; which he discovered by viewing an object through this double prism; which, when it appeared neither raised nor depressed, he was satisfied that the refractions were equal, and that the emergent rays were parallel to the incident.

Now, according to the prevailing opinion, the object should have appeared through this double prism quite of its natural colour; for if the difference of refrangibility had been equal in the two equal refractions, they would have rectified each other: but the experiment fully proved the fallacy of this received opinion, by showing the divergency of the light by the prism to be almost double of that by the water; for the object, though not at all refracted, was yet as much infected with prismatic colours, as if it had been seen through a glass wedge only, whose refracting angle was near 30 degrees. This experiment will be readily perceived to be the same as that which Sir Isaac Newton mentions, book 1, part 2, prop. 3, experiment 8, of his Optics. But how it comes

to differ so very remarkably in the result, Mr. D. does not take upon him to account for; but only adds, that he used all possible precaution and care in the process, and that he kept the apparatus by him to evince the truth of what he wrote, whenever he might be properly required so to do.

He plainly saw then, that if the refracting angle of the water-vessel could have admitted of a sufficient increase, the divergency of the coloured rays would have been greatly diminished, or entirely rectified; and there would have been a very great refraction without colour, as now he had a great discolouring without refraction: but the inconveniency of so large an angle, as that of the vessel must have been to bring the light to an equal divergency with that of the glass prism, whose angle was about 60 degrees, made it necessary to try some experiments of the same kind by smaller angles.

He ground a wedge of common plate glass to an angle of somewhat less than 9 degrees, which refracted the mean rays about 5 degrees. He then made a wedge-like vessel, as in the former experiment, and filling it with water, managed it so that it refracted equally with the glass wedge; or in other words, the difference of their refractions was nothing, and objects viewed through them appeared neither raised nor depressed. This was done with an intent to observe the same thing over again in these small angles which he had seen in the prism: and it appeared indeed the same in proportion, or as near as could be judged; for notwithstanding the refractions were here also equal, yet the divergency of the colours by the glass was vastly greater than that by the water; for objects seen by these two refractions were very much discoloured. Now this was a demonstration, that the divergency of the light, by the different refrangibility, was far from being equal in these two refractions. He also saw, from the position of the colours, that the excess of divergency was in the glass; so that he increased the angle of the water-wedge by different trials, till the divergency of the light by the water was equal to that by the glass; that is, till the object, though considerably refracted by the excess of the refraction of the water, appeared nevertheless quite free from any colours proceeding from the different refrangibility of light; and, as near as he could then measure, the refraction by the water was about $\frac{5}{4}$ of that by the glass. Indeed he was not very exact in taking the measures, because the business was not at that time about the proportions, so much as to show that the divergency of the colours, by different substances, was by no means in proportion to the refractions; and that there was a possibility of refraction without any divergency of the light at all.

Having, about the beginning of the year 1757, tried these experiments, he soon after set about grinding telescopic object-glasses on the new principles of refractions, which he had gathered from them; which object-glasses were compounded of two spherical glasses with water between them. These glasses be

had the satisfaction to find, as he had expected, free from the errors arising from the different refrangibility of light: for the refractions by which the rays were brought to a focus, were every where the differences between two contrary refractions, in the same manner, and in the same proportions as in the experiment with the wedges.

However, the images formed at the foci of these object-glasses, were still very far from being so distinct as might have been expected from the removal of so great a disturbance; and yet it was not very difficult to guess at the reason, when it is considered, that the radii of the spherical surfaces of those glasses were required to be so short, in order to make the refractions in the required proportions, that they must produce aberrations or errors in the image, as great, or greater than those from the different refrangibility of light. And therefore, seeing no method of getting over that difficulty, he gave up all hopes of succeeding in that way. And yet, as these experiments clearly proved, that different substances diverged the light very differently in proportion to the refraction; he began to suspect that such variety might possibly be found in different sorts of glass, especially as experience had already showed that some made much better object-glasses in the usual way than others: and as no satisfactory cause had as yet been assigned for such difference, there was great reason to presume, that it might be owing to the different divergency of the light by their refractions.

Therefore the next business to be undertaken, was to grind wedges of different kinds of glass, and apply them together, so that the refractions might be made in contrary directions, in order to discover, as in the foregoing experiments, whether the refraction and divergency of the colours would vanish together. But a considerable time elapsed before he could set about that work; for though he was determined to try it at his leisure, for satisfying his own curiosity, yet he did not expect to meet with a difference sufficient to give room for any great improvement of telescopes; so that it was not till the latter end of the year that he undertook it, when his first trials convinced him that this business really deserved his utmost attention and application.

He discovered a difference far beyond his hopes, in the refractive qualities of different kinds of glass, with respect to their divergency of colours. The yellow or straw-coloured foreign sorts, commonly called Venice glass, and the English crown glass, are very near alike in that respect, though in general the crown glass seems to diverge the light rather the less of the two. The common plate glass made in England diverges more; and the white crystal or flint English glass, as it is called, most of all. It was not now his business to examine into the particular qualities of every kind of glass that he could come at, much less to amuse himself with conjectures about the cause, but to fix on such two sorts as their difference was the greatest; which he soon found to be the crown, and

the white flint or crystal. He therefore ground a wedge of white flint of about 25°, and another of crown of about 29°, which refracted nearly alike; but their divergency of the colours was very different. He then ground several others of crown to different angles, till he got one which was equal, with respect to the divergency of the light, to that in the white flint; for when they were put together, so as to refract in contrary directions, the refracted light was entirely free from colour. Then measuring the refractions of each wedge, he found that of the white glass to be to that of the crown nearly as 2 to 3; and this proportion would hold very nearly in all small angles. Therefore any two wedges made in this proportion, and applied together, so as to refract in a contrary direction, would refract the light without any difference of refrangibility.

To make therefore two spherical glasses, that shall refract the light in contrary directions, it is easy to understand, that one must be concave, and the other convex; and as the rays are to converge to a real focus, the excess of refraction must evidently be in the convex; and as the convex is to refract most, it appears from the experiment, that it must be made with crown glass, and the concave with white flint glass. And further, as the refractions of spherical glasses are in an inverse ratio of their focal distances, it follows, that the focal distances of the two glasses should be inversely as the ratios of the refractions of the wedges: for being thus proportioned, every ray of light that passes through this combined glass, at whatever distance it may pass from its axe, will constantly be refracted, by the difference between two contrary refractions, in the proportion required, and therefore the different refrangibility of the light will be entirely removed.

Having thus got rid of the principal cause of the imperfection of refracting telescopes, there seemed to be nothing more to do, but to go to work on this principle; but he had not made many attempts, before he found that the removal of one impediment, had introduced another equally detrimental (the same as he had before found in two glasses with water between them): for the two glasses, that were to be combined together, were the segments of very deep spheres; and therefore the aberrations from the spherical surfaces became very considerable, and greatly disturbed the distinctness of the image. Though this appeared at first a very great difficulty, yet he was not long without hopes of a remedy: for considering that the surfaces of spherical glasses admit of great variations, though the focal distance be limited, and that by these variations their aberrations may be made more or less, almost at pleasure; he plainly saw the possibility of making the aberrations of any two glasses equal; and as in this case the refractions of the two glasses were contrary to each other, their aberrations, being equal, would entirely vanish. And thus at last he obtained a perfect theory for making object glasses, to the apertures of which he could scarcely conceive any limits; for if

the practice could come up to the theory, they must certainly admit of very extensive ones, and of course bear very great magnifying powers.

But the difficulties attending the practice are very considerable. In the first place, the focal distances, as well as the particular surfaces, must be very nicely proportioned to the densities or refracting powers of the glasses; which are very apt to vary in the same sort of glass made at different times. Secondly, the centres of the two glasses must be placed truly on the common axis of the telescope, otherwise the desired effect will be in a great measure destroyed. Add to these, that there are 4 surfaces to be wrought perfectly spherical; and any person, but moderately practised in optical operations, will allow, that there must be the greatest accuracy throughout the whole work.

Notwithstanding so many difficulties, as here enumerated, after numerous trials, and a resolute perseverance, he brought the matter at last to such an issue, that he could construct refracting telescopes, with such apertures and magnifying powers, under limited lengths, as, in the opinion of the best and undeniable judges, who have experienced them, far exceed any thing that has been hitherto produced, as representing objects with great distinctness, and in their true colours.

XCIX. Of some Extraordinary Effects arising from Convulsions. By W. Watson, M. D., F. R. S. p. 743.

In January 1757, Dr. W. was concerned for a young gentlewoman, who, if the number, continuance, and frequency of their returns, be considered, suffered the most violent and severe convulsions he ever knew. At some times the muscular spasms were general; at other times single muscles only, or a number of them, subservient to some particular purpose in the animal economy, were affected. And such was the peculiarity of this case, that after, and in proportion as any single muscle, or any determined number of muscles, had been in a state of spasm, a paralytic inability succeeded to those muscles, which very much disordered and impaired, and several times even for no small continuance prevented the patient from performing several of her necessary functions. When the muscles, for instance, subservient to deglutition had been convulsed, for many hours after the fits had left her she had not been able to swallow a single drop of liquid: so that when attempts had been made to cause her to drink, unless the liquor was immediately thrown back, there was imminent danger of her being strangled. When her eyes had been affected, several times a complete gutta serena, and total blindness, ensued; the patient being able to bear the strong day-light with open eyes, without being sensible of its influence, or in the least contracting her widely dilated pupils. After one of these fits the blindness continued full 5 days: and Dr. W. began to be in fear for the return of her sight.

It is well known that vocification is performed in the aspera arteria; but that the articulation of sounds into syllables and words is modulated principally by the tongue, and muscles about the larynx. In this case, very early in the disease, the spasms seized the muscles about the larynx: the consequence of which was, that after they were over, the patient was unable to utter a word. This faculty however she again once recovered; but it continued a very short time, as the fits returned, which again left her deprived of the power of speech. After having lost her voice a second time, her power of speech did not return, even after she was freed from her convulsions, and her general health restored. Fourteen months passed, while this patient continued absolutely speechless; when, after having violently heated herself by 4 hours dancing, on a sudden her power of speech returned, and it has continued perfectly free ever since.

What is still further remarkable in this case is, that during the whole time of this patient's continuing speechless, her life was rendered yet more uncomfortable by her having, from the injury to her brain by the spasms, forgot how to write, so as to express her meaning that way: but on the recovery of her speech, this faculty likewise returned, which she has retained ever since. During the severity of this disease, which continued several weeks, almost every day of which, from the number and violence of the convulsions, Dr. W. feared would be the patient's last, nothing was left unattempted, which he imagined could tend to prevent the return of the spasms, or lessen their effects. His endeavours so far happily succeeded, that her fits did not return; but the consequences of them continued, more particularly her inability to speak. After some months, however, when she was recruited in her strength, he was desirous of trying the effects of electricity, more particularly applied about her throat. This was accordingly attempted; but such was the state of her nerves, and their sensibility to its effects, that electrizing brought back the fits, which again affected her sight; so that he was compelled to desist, lest, in endeavouring to restore her speech, he might not only fail in this attempt, but might possibly bring on a permanent blindness. He determined therefore to trust the whole to time, which happily removed all her complaints.

C. On an Extraordinary Storm of Hail in Virginia. By Francis Fauquier, Esq. Lieut. Governor of Virginia, and F. R. S. p. 746.

This storm happened on Sunday the 9th of July, about 4 o'clock in the afternoon, and was preceded by some thunder and lightning. It was a small cloud that did not seem to threaten much before its breaking, and did not extend a full mile in breadth. It passed over the middle of the town of Williamsburgh, and the skirts of the town had but little of it. Its course was from N. by W. to S. by E. The hail-stones, or rather pieces of ice, were most of them of an oblong

square form; many of them an inch and a half long, and about three-fourths of an inch wide and deep; and from one side of most of them there proceeded sharp spikes, protuberant at least half an inch. He says he cooled his wine, and froze cream, with some of them the next day; and they were not totally dissolved when he went to bed on Monday night. This storm broke every pane of glass on the north side his house, and destroyed all his garden things entirely.

He mentions likewise the heats as having been rather more than usual in that country this summer; and particularly on the 9th of August his thermometer (which is hung on the outside of his house on the north aspect) was at 97, by Fahrenheit's graduation, and some other days as high as 94 or 95.

CI. An Extraordinary Case of a Diseased Eye. By D. P. Layard, M. D., F. R. S. p. 747.

In October 1755 Dr. L. communicated to Dr. Maty, and he inserted in the last vol. of his Journal Britannique, the case of Susannah Earle, who, in consequence of the hooping cough, was afflicted with a protruded eye. The present case is somewhat similar to that girl's in its first appearance and progress, but by accident attended with a 2d disease.

John Law, a robust lad, 13 years old, in Easter week 1756, beating dung about a close with unusual force, on a sudden felt a violent pain in his left eye. The pain increased, an inflammation ensued, and the eye grew daily larger. The poor boy's mother after several unsuccessful applications, brought her son, Oct. 7, to Mr. Daniel Hopkins, surgeon, in Huntingdon; and having desired Dr. L.'s opinion, both examined the eye together.

The left eye was protruded out of its orbit, and hung down over the cheek to the upper lip. The coats were greatly discoloured, all the vessels turgid, the sight totally lost, and the humours appeared like fluctuating pus. They saw the necessity of an immediate extirpation, to save the right eye, already greatly inflamed; and having apprized the mother and boy of the state the eye was in, a consultation was desired with 2 surgeons of St. Ives; accordingly Mr. Skeeles and Mr. Want very charitably met Mr. Hopkins and Dr. L. the next day. On Mr. Want's pressing with his finger on the pupil, the globe burst at the edge of the iris, and discharged pus. The extirpation of the eye was unanimously agreed on, and immediately performed. Mr. Hopkins made a puncture with a lancet close to the external and small canthus of the eye, and then with a pair of crooked scissars took off all the distended globe close to the eye-lids. He then cleaned the cavity of the purulent humours, and filled it with soft lint, over which he applied bolsters dipped in warm red wine and water, and the monoculus bandage to keep on the whole dressings. The lad was bled in the arm; nitrous medicines and anodynes were prescribed, and a suitable regimen. The fever and

inflammation of the eye gradually decreased; the suppuration of the wound in a few days was good, the distended eye-lids contracted, and a cure was soon expected.

But on Nov. the 7th the lad went to open the street-door, and it being a cold and rainy evening, he quickly felt the bad effects of the cold wind, which drove the rain in upon him. That night the wound became again very painful, the eye-lids puffed up, and next day appeared much inflamed, as were all the contents of the orbit. Fungous excrescences soon followed, and an intermittent fever. An emetic being improper, he was purged with rhubarb, and afterwards took the bark infused in red wine. The fever was removed after some time; but the contents of the orbit continued increasing, and the fungous excrescences became so large and spongy, as to be of equal bulk with the diseased eye before extirpation. All topical applications, to contract this fungus, were ineffectual, and the application of caustics or escharotics was prudently avoided, lest they should produce a carcinomatous ulcer. The discharge was chiefly a purulent serum: on which account, ever since the beginning of November he was kept on a dry diet. In February 1757, the remaining coats of the eye began to appear at the most prominent parts of the excrescence, and seemed white like a part of the conjunctiva. On touching it with the finger, a distinct fluctuation was felt, and an hydrophthalmia perfectly discovered; but neither the thickness of the coats, nor the sensibility of the parts, would permit a puncture to be made, till the cyst, which appeared formed by the distension of one of the coats of the eye, was more free from the fungus.

The cyst continued daily to extend itself, and to separate the fungous edges; the fluctuation became more manifest, and the membranes thinner. At length on the 15th of June, Mr. Hopkins opened the cyst with the point of a lancet, and let out a large cup-full of limpid serum, without smell or taste. The boy felt very little pain in this operation. The cavity was filled with dry lint, and compresses dipped in warm red wine and water were applied over it. All the night following, and several days after, a great discharge of serum came away. On the 19th the fungus was considerably lessened. Mr. Hopkins then dressed the wound with warm unguentum é gummi elemi, and washed the fungus with a lotion of aq. calcis, ros. et tinct. myrrhæ. On the 23d, on his removing the dressings, he saw the cyst loose and collapsed; which he extracted with his forceps, without the least difficulty or pain to the patient. The fungus daily wasted afterwards, the wound digested well, and the lad was entirely cured on the 7th of August,

The right eye became perfectly strong, and he continued free from complaint The remainder of the coats of the eye, and of the muscles, bore up the eye-lids, that when uncovered he only seemed to have closed the left eye: however, he

wore all the winter a black patch over it, to guard against fresh cold. The cyst, when first taken away, measured $3\frac{1}{2}$ inches in length, $1\frac{1}{2}$ in diameter, and contained a large cup-full of water. It appeared to be the tunica sclerotica, was of a clear pellucid white, and of so delicate a texture, as scarcely to admit of being touched without tearing; and when dried with all possible care, became so brittle, that Mr. Hopkins could hardly preserve it in the manner he had sent it.

Remarks.—In both S. Earle's and J. Law's cases, the eye was distended by the accumulation of the aqueous humour, separated in great quantity by the repeated straining of the blood-vessels in the hooping cough, which might gradually relax and enlarge the aqueous ducts of S. Earle's eye; and possibly by the rupture of those ducts, and of some blood-vessels, at the time J. Law exerted himself violently in beating dung about the close; for in either case the impetus of the blood must have been so violent as to produce those effects. However, from the hydrophthalmia succeeding the operation on Law, the fungous excrescence, and continual serous discharge during several months from the wound, it plainly appears that an abundance of aqueous humour was discharged at first by the distension or laceration of the aqueous ducts, and latterly for want of a contraction of those vessels, and of the lymphatics, which were no longer of use.

Both these cases showed the necessity of inquiring particularly into the causes of diseases of the eyes, as well as of other parts of the body; for by barely attending to the symptoms, the disease will not be removed, though the symptoms be alleviated. Bleeding, and moderate evacuations, would doubtless at first have decreased the tension and pain, and assuaged the inflammation; but both topical applications, and internal medicines, were properly to be adapted, and a suitable diet regulated. Not to mention the absurd and impertinent abuse of empirics, what benefit could accrue, in both these cases, from unctuous, laxative, or emollient applications, from drastic and mercurial purges? Though such applications might be well intended, to take off the tension and inflammation; yet as the distension of the blood-vessels only increased gradually, as the globe of the eye was enlarged; so whatever application relaxed the coats of the eye must infallibly stretch out the vessels yet farther, and cause a greater pain and inflammation; which drastic and mercurial purges would also increase. The only method then to be pursued in such bad cases would be at first to endeavour to remove the fulness of the blood, and make use of such topical remedies as would contract without irritation. If the cause remains, as the hooping cough in S. Earl s case, no amendment of the eye can be expected, while the patient's blood-vessels are continually strained by frequent coughing. This illness therefore should be attended to, and removed as soon as possible.

But should the eye be so enlarged as to protrude itself out of the orbit, there seems no other way to lessen the bulk of the eye, than by making a puncture

with a proper instrument, to let out the aqueous humour; and then apply such agglutinant and contracting collyria, as may reduce the distended coats and vessels to their former size. This operation should be performed before the humours are vitiated, the sight lost, the vessels in a state of suppuration, and the coats of the eye too far extended; for at that time nothing less than extirpation can be of use. Professor Nuck, in his Tractatus de Ductibus Oculorum Aquosis, p. 120, relates the success he had in curing a young man by 5 repeated punctures, and a strict observance in a proper use of all the non-naturals.

CII. On the Heat of the Weather in Georgia. By H. Ellis, Esq. Governor of Georgia, and F. R. S. p. 754.

One cannot here sit down to any thing that requires much application but with extreme reluctance; for such is the debilitating quality of our violent heats at this season (July), that an inexpressible languor enervates every faculty, and renders even the thought of exercising them painful. It is now (writes Mr. Ellis) about 3 o'clock; the sun bears nearly s.w., and I am writing in a piazza, open at each end, on the N.E. side of my house, perfectly in the shade: a small breeze at S.E. blows freely through it; no buildings are nearer to reflect the heat than 60 yards: yet in a thermometer hanging by me, made by Mr. Bird, and compared by the late Mr. George Graham with an approved one of his own, the mercury stands at 102. Twice it has risen this summer to the same height; viz. on the 28th of June, and the 11th of July. Several times it has been at 100, and for many days successively at 98; and did not in the nights sink below 89. It is highly probable that the inhabitants of this town breathe a hotter air than any other people on the face of the earth. The greatest heat we had last year was but 92, and that but once: from 84 to 90 were the usual variations; but this is reckoned an extraordinary hot summer. The weather-wise of this country say it forebodes a hurricane; for it has always been remarked, that these tempests have been preceded by continual and uncommon heats. I must acquaint you however that the heats we are subject to here are more intense than in any other parts of the province, the town of Savannah being situated on a sandy eminence, and sheltered all round with high woods. Yet it is remarkable that this very spot, from its height and dryness, is reckoned equally healthy with any other in the province.

I have frequently walked 100 yards under an umbrella, with a thermometer suspended from it by a thread to the height of my nostrils, when the mercury has risen to 105; which is prodigious. At the same time I have confined this instrument close to the hottest part of my body, and have been astonished to observe that it has subsided several degrees. Indeed I never could raise the mercury above 97 with the heat of my body. I have traversed a great part of

this globe, not without giving some attention to the peculiarities of each climate; and I can fairly pronounce that I never felt such heats any where as in Georgia. I know experiments on this subject are extremely liable to error; but I presume I cannot now be mistaken, either in the goodness of the instrument, or in the fairness of the trials, which I have repeatedly made with it. This same thermometer I have had thrice in the equatorial parts of Africa; as often at Jamaica, and the West India islands; and on examination of my journals, I do not find that the quicksilver ever rose in those parts above the 87th degree, and to that but seldom: its general station was between the 79th and 86th degree; and yet I think I have felt those degrees, with a moist air, more disagreeable than what I now feel.

In my account of the late expedition to the north-west, I have observed that all the changes and variety of weather that happen in the temperate zone throughout the year, may be experienced at the Hudson's Bay settlements in 24 hours. But I may now extend this observation; for in my cellar the thermometer stands at 81, in the next story at 102, and in the upper one at 105; and yet these heats, violent as they are, would be tolerable but for the sudden changes that succeed them. On the 10th of December last the mercury was at 86; on the 11th it was so low as 38 of the same instrument. What havock must this make with a European constitution? Yet but few people die here out of the ordinary course; though indeed one can scarcely call it living, merely to breathe, and trail about a vigorless body; yet such is generally our condition from the middle of June to the middle of September.

CIII. The Invention of a General Method for determining the Sum of every 2d, 3d, 4th, or 5th, &c. Term of a Series, taken in order; the Sum of the whole Series being known. By Tho. Simpson, F. R. S. p. 757.

As the doctrine of series is of very great use in the higher branches of the mathematics, and their application to nature, every attempt tending to extend that doctrine may justly merit some degree of regard. The subject of the present paper will be found an improvement of some consequence in that part of science. And how far the business of finding fluents may, in some cases, be facilitated by it, will appear from the examples subjoined, in illustration of the general method here delivered.

The series propounded, whose sum (s) is supposed to be given (either in algebraic terms, or by the measures of angles and ratios, &c.) is here represented by $a + bx + cx^2 + dx^3 + ex^4$ &c. and Mr. S. first gives the solution of that case, where every 3d term is required to be taken, or where the series to be summed is $a + dx^3 + gx^6 + hx^9$ &c. By means of which, the general method of proceeding, and the solution of every other case, will appear evident.

Here then every 3d term being required to be taken, let the series ($a + dx^3 + gx^6$ &c.) whose value is sought, be conceived to be composed of 3 others.

$$\tfrac{1}{3} \times (a + b \times px + c \times p^2x^2 + d \times p^3x^3 + e \times p^4x^4 \text{ \&c.})$$
$$\tfrac{1}{3} \times (a + b \times qx + c \times q^2x^2 + d \times q^3x^3 + e \times q^4x^4 \text{ \&c.})$$
$$\tfrac{1}{3} \times (a + b \times rx + c \times r^2x^2 + d \times r^3x^3 + e \times r^4x^4 \text{ \&c.})$$

having all the same form, and the same coefficients with the series first proposed, and where the converging quantities px, qx, rx, are also in a determinate (though yet unknown) ratio to the original converging quantity x. Now in order to determine the quantities of these ratios, or the values of p, q, and r, let the terms containing the same powers of x, in the two equal values, be equated in the common way.

So shall,

$$\tfrac{1}{3}b \times px + \tfrac{1}{3}b \times qx + \tfrac{1}{3}b \times rx = 0$$
$$\tfrac{1}{3}c \times p^2x^2 + \tfrac{1}{3}c \times q^2x^2 + \tfrac{1}{3}c \times r^2x^2 = 0$$
$$\tfrac{1}{3}d \times p^3x^3 + \tfrac{1}{3}d \times q^3x^3 + \tfrac{1}{3}d \times r^3x^3 = dx^3$$
$$\tfrac{1}{3}e \times p^4x^4 + \tfrac{1}{3}e \times q^4x^4 + \tfrac{1}{3}e \times r^4x^4 = 0, \text{ \&c.}$$

And consequently,

$$p + q + r = 0$$
$$p^2 + q^2 + r^2 = 0$$
$$p^3 + q^3 + r^3 = 3$$
$$p^4 + q^4 + r^4 = 0, \text{ \&c.}$$

Now make $p^3 = 1$, $q^3 = 1$, and $r^3 = 1$; that is, let p, q, and r, be the three roots of the cubic equation $z^3 = 1$, or $z^3 - 1 = 0$: then, seeing both the 2d and 3d terms of this equation are wanting, not only the sum of all the roots ($p + q + r$) but the sum of all their squares ($p^2 + q^2 + r^2$) will vanish, or be equal to nothing, by common algebra, as they ought to fulfil the conditions of the first two equations. Also, since $p^3 = 1$, $q^3 = 1$, and $r^3 = 1$, it is also evident, that $p^4 + q^4 + r^4$ ($= p + q + r$) $= 0$, $p^5 + q^5 + r^5$ ($= p^2 + q^2 + r^2$) $= 0$, $p^6 + q^6 + r^6$ ($= p^3 + q^3 + r^3$) $= 3$. Which equations being, in effect, nothing more than the first 3 repeated, the values of p, q, r, above assigned, equally fulfil the conditions of these also: so that the series arising from the addition of 3 assumed ones will agree, in every term, with that whose sum is required: but those series, of which the quantity in question is composed, having all of them the same form and the same coefficients with the original series $a + bx + cx^2 + dx^3$ &c. ($=$ s), their sums will therefore be truly obtained, by substituting px, qx, and rx, successively, for x, in the given value of s. And, by the very same reasoning, and the process above laid down, it is evident, that, if every nth term, instead of every 3d term, of the given series be taken, the values of p, q, r, s, &c. will then be the roots of the equation $z^n - 1 = 0$;* and that the sum of all the terms so taken, will be truly obtained by substituting

* If α, β, γ, δ, &c. be supposed to represent the co-sines of the angles $\frac{360°}{n}$, $2 \times \frac{360°}{n}$, $3 \times \frac{360°}{n}$, &c. the radius being unity; then the roots of the equation $z^n - 1 = 0$ (expressing the several values of p, q, r, s, &c.) will be truly defined by 1, $\alpha + \sqrt{\alpha\alpha - 1}$, $\alpha - \sqrt{\alpha\alpha - 1}$, $\beta + \sqrt{\beta\beta - 1}$, $\beta - \sqrt{\beta\beta - 1}$, &c. The demonstration of this will be given farther on.—Orig.

px, qx, rx, sx, &c. successively for x, in the given value of s, and then dividing the sum of all the quantities thence arising by the given number n.

The same method of solution holds equally, when, in taking every nth term of the series, the operation begins at some term after the first. For all the terms preceding that may be transposed, and the whole equation divided by the power of x in the first of the remaining terms; and then the sum of every nth term, beginning at the first, will be found by the preceding directions; which sum, multiplied by the power of x that before divided, will evidently give the true value required to be determined. Thus, for example, let it be required to find the sum of every 3d term of the given series $a + bx + cx^2 + dx^3 + ex^4$, &c. ($=$ s), beginning with cx^2. Then, by transposing the first two terms, and dividing the whole by x^2, we shall have $c + dx + ex^2 + fx^3$ &c. $= \frac{s - a - bx}{xx}$ ($=$ s′). From which having found the sum of every 3d term of the series $c + dx + ex^2 + fx^3$ &c. beginning at the first c, that sum, multiplied by x^2, will manifestly give the true value sought in the present case.

And here it may be worth while to observe, that all the terms preceding that at which the operation, in any case, begins, may (provided they exceed not in number the given interval n) be entirely disregarded, as having no effect at all in the result. For if in that part $(\frac{-a - bx}{xx})$ of the value of s′, above exhibited, in which the first terms, a and bx, enter, there be substituted px, qx, rx, successively, for x, according to the prescript, the sum of the quantities thence arising will be $-\frac{a}{p^2x^2} - \frac{a}{q^2x^2} - \frac{a}{r^2x^2} - \frac{b}{px} - \frac{b}{qx} - \frac{b}{rx}$, which, because $p^3 = 1$, $q^3 = 1$, &c. or $p^2 = \frac{1}{p}$, $q^2 = \frac{1}{q}$, &c. may be expressed thus: $-\frac{a}{xx} \times (p + q + r) - \frac{b}{x} \times (p^2 + q^2 + r^2)$. But, that $p + q + r = 0$, and $p^2 + q^2 + r^2 = 0$, has been already shown; whence the truth of the general observation is manifest. Hence it also appears, that the method of solution above delivered, is not only general, but includes this singular beauty and advantage, that in all series whatever, the terms of which are to be taken according to the same assigned order, the quantities (p, q, r, &c.), by which the solution is performed, will remain invariably the same. The greater part of these quantities are indeed imaginary ones; and so likewise will the quantities be that result from them, when substitution is made in the given expression for the value of s. But by adding together, as is usual in like cases, every two corresponding values, so resulting, all marks of impossibility will disappear.

If, in the series to be summed, the alternate terms, viz. the 2d, 4th, 6th, &c. should be required to be taken under signs contrary to what they have in the original series given; the reasoning and result will be nowise different; only instead of making $p^3 + q^3 + r^3$, or $p^n + q^n + r^n$, &c. $= + 3$ or $+ n$, the

same quantity must here be made $= -3$ or $-n$. Whence p^n being $= -1$, $q^n = -1$, &c. the values of p, q, r, &c. will in this case be the roots of the equation $z^n + 1 = 0$.

It may now be proper to set down an example or two of the use and application of the general conclusions above derived. First then, supposing the series, whose sum is given, to be $x + \frac{x^2}{2} + \frac{x^3}{3} + \frac{x^4}{4} \dots\dots + \frac{x^m}{m} + \frac{x^{m+1}}{m+1} + \frac{x^{m+2}}{m+2} \dots\dots + \frac{x^{m+n}}{m+n} + \frac{x^{m+n+1}}{m+n+1} +$ &c. $= -$ H. log. $\overline{1-x}$ ($=$ S); let it be hence required to find the sum of the series $(\frac{x^m}{m} + \frac{x^{n+n}}{m+n} + \frac{x^{n+2n}}{m+2n}$ &c.) arising by taking every n^{th} term, beginning with that whose exponent (m) is any integer less than n. Here the terms preceding $\frac{x^m}{m}$ being transposed, and the whole equation divided by x^m, we have $\frac{1}{m} + \frac{x}{m+1} + \frac{x^2}{m+2} + \frac{x^3}{m+3}$, &c. $= -\frac{1}{x^m} \times$ H. log. $(1-x) - \frac{x + \frac{1}{2}x^2 \text{ \&c.}}{x^m}$. In which value, let px, qx, rx, &c. be successively substituted for x (according to prescript) neglecting entirely the terms $\frac{x + \frac{1}{2}x^2}{x^m}$, as having no effect at all in the result: whence we get $-\frac{1}{(px)^m} \times \log.(1-px) - \frac{1}{(qx)^m} \times \log.(1-qx) - \frac{1}{(rx)^m} \times \log.(1-rx)$, &c. Which multiplied by x^m (the quantity that before divided) gives $-\frac{1}{p^m} \times \log.(1-px) - \frac{1}{q^m} \times \log.(1-qx) - \frac{1}{r^m} \times \log.(1-rx)$, &c. $= n$ times the quantity required to be determined.

But now to get rid of the imaginary quantities q, r, &c. by means of their known values $\alpha + \sqrt{\alpha\alpha - 1}$, $\alpha - \sqrt{\alpha\alpha - 1}$, &c. it will be necessary to observe, that as the product of any two corresponding ones $(\alpha + \sqrt{\alpha\alpha - 1}) \times (\alpha - \sqrt{\alpha\alpha - 1})$ is equal to unity, we may therefore write $(\alpha - \sqrt{\alpha\alpha - 1})^n$ $(= r^m)$ instead of its equal $\frac{1}{q^m}$, and $(\alpha + \sqrt{\alpha\alpha - 1})^m$ $(= q^m)$ instead of its equal $\frac{1}{r^m}$: by which means the two terms, wherein these two quantities enter, will stand thus; $-(\alpha - \sqrt{\alpha\alpha - 1})^m \times$ Log. $(1-qx) - (\alpha + \sqrt{\alpha\alpha - 1})^m \times$ Log. $(1-rx.)$

But if A be assumed to express the co sine of an arch (Q,) m times as great as that $(\frac{360^\circ}{n})$ whose co-sine is here denoted by α; then will $\mathrm{A} - \sqrt{\mathrm{AA} - 1} =$*

* Because $\frac{-\dot{x}}{\sqrt{1-xx}}$ and $\frac{-\dot{\mathrm{X}}}{\sqrt{1-\mathrm{XX}}}$ are known to express the fluxions of the circular arcs whose co-sines are x and X, it is evident, if those arcs be supposed in any constant ratio of 1 to n, that $\frac{n\dot{x}}{\sqrt{1-xx}} = \frac{\dot{\mathrm{X}}}{\sqrt{1-\mathrm{XX}}}$, and consequently that $\frac{n\dot{x}}{\sqrt{xx-1}}$ $(= \frac{n\dot{x}}{\sqrt{-1} \times \sqrt{1-xx}} = \frac{\dot{\mathrm{X}}}{\sqrt{-1} \times \sqrt{1-xx}})$ $= \frac{\dot{\mathrm{X}}}{\sqrt{\mathrm{XX}-1}}$. Whence, by taking the fluents, $n \times \log.(x + \sqrt{xx-1})$ or $\log.(x + \sqrt{xx-1})_n = \log.\ (\mathrm{X} + \sqrt{\mathrm{XX}-1};)$ and consequently $(x + \sqrt{xx-1})^n = \mathrm{X} + \sqrt{\mathrm{XX}-1}$: whence also seeing $x - \sqrt{xx-1}$ is the reciprocal of $x + \sqrt{xx-1}$, and $\mathrm{X} - \sqrt{\mathrm{XX}-1}$ of $\mathrm{X} + \sqrt{\mathrm{XX}-1}$, it is also evident that $(x - \sqrt{xx-1})^n = \mathrm{X} - \sqrt{\mathrm{XX}-1}$. Hence, not only the truth of the above assump-

$(\alpha - \sqrt{\alpha\alpha - 1})^m$, and $A + \sqrt{AA - 1} = (\alpha + \sqrt{\alpha\alpha - 1})^m$: which values being substituted above, we thence get

$-A \times [\log.(1 - qx) + \log.(1 - rx)] + \sqrt{AA - 1} \times [\log.(1 - qx) - \log.(1 - rx)]$; of which the former part (which, exclusive of the factor A, is hereafter denoted by M) is manifestly equal to $-A \times \log.(1 - qx) \times (1 - rx)$ by the nature of logarithms $= -A \times \log.[1 - (q + r)x + qrx^2] = -A \times \log.(1 - 2\alpha x + xx)$ by substituting the values of q and r: which is now entirely free from imaginary quantities. But in order to exterminate them out of the latter part also, put $y = \log.(1 - qx) - \log.(1 - rx)$; then will $\dot{y} = \frac{-q\dot{x}}{1 - qx} + \frac{r\dot{x}}{1 - rx} = -\frac{\overline{q - r} \times \dot{x}}{1 - \overline{q + r} \times x + xx} = -\frac{2\sqrt{\alpha\alpha - 1} \times \dot{x}}{1 - 2\alpha x + xx} = -\frac{2\sqrt{-1} \times \sqrt{1 - \alpha\alpha} \times \dot{x}}{1 - 2\alpha x + xx}$; where $\frac{\sqrt{1 - \alpha\alpha} \times \dot{x}}{1 - 2\alpha x + xx}$ expresses the fluxion of a circular arch (N) whose radius is 1, and sine $= \frac{\sqrt{1 - \alpha\alpha} \times \dot{x}}{1 - 2\alpha x + xx}$; consequently y will be $= -2\sqrt{-1} \times N$: which, multiplied by $\sqrt{AA - 1}$, or its equal $\sqrt{-1} \times \sqrt{1 - AA}$, gives $2\sqrt{1 - AA} \times N$; and this value being added to that of the former part, found above, and the whole being divided by n, we thence obtain $\frac{-AM + 2\sqrt{1 - AA}}{n}, \times N$, or $\frac{1}{n} \times (-\text{co-s. } Q \times M + \sin Q \times 2N)$ for that part of the value sought depending on the two terms affected with q and r. Whence the sum of any other two corresponding terms will be had, by barely substituting one letter or value for another: so that,

$$\frac{1}{n} \times \begin{cases} -\log.(1 - x) \\ -\text{co-s. } Q \times M + \sin. Q \times 2N \\ -\text{co-s. } Q' \times M' + \sin. Q' \times 2N' \\ -\text{co-s. } Q'' \times M'' + \sin. Q'' \times 2N'' \\ -\text{\&c.} \qquad\qquad + \text{\&c.} \end{cases}$$

will truly express the sum of the series proposed to be determined; M, M', M'', &c. being the hyperbolical logarithms of $1 - 2\alpha x \times \alpha x$, $1 - 2\beta x + xx$, $1 - 2\gamma x + xx$, &c. N, N', N'', &c. the arcs whose sines are $\frac{x\sqrt{1 - \alpha\alpha}}{\sqrt{1 - 2\alpha x + xx}}$, $\frac{x\sqrt{1 - \beta\beta}}{\sqrt{1 - 2\beta x + xx}}$, $\frac{x\sqrt{1 - \gamma\gamma}}{\sqrt{1 - 2\gamma x + xx}}$, &c. and Q, Q', Q'', &c. the measures

tion, but what has been advanced in respect to the roots of the equation $z^n - 1 = 0$, will appear manifest. For if $x \pm \sqrt{xx - 1}$ be put $= z$, then will $z^n = x \pm \sqrt{xx - 1})^n = x \pm \sqrt{xx - 1}$: where assuming $x = 1 = \text{co-s. } 0 = \text{co-s. } 360° = \text{co-s. } 2 \times 360° = \text{co-s. } 3 \times 360°$, &c. the equation will become $z^n = 1$, or $z^n - 1 = 0$; and the different values of x, in the expression $(x \pm \sqrt{xx - 1})$ for the root z, will consequently be the co-sines of the arcs $\frac{0}{n}$, $\frac{360°}{n}$, $\frac{2 \times 360°}{n}$, &c. these arcs being the corresponding submultiples of those above, answering to the co-sine x ($= 1$.) —— In the same manner, if x be taken $= -1 = \text{co-s. } 180° = \text{co-s. } 3 \times 180° = \text{co-s. } 5 \times 180°$, &c. then will $z^n = -1$, or $z^n + 1 = 0$; and the values of x will, in this case, be the co-sines of $\frac{180°}{n}$, $3 \times \frac{180°}{n}$, $5 \times \frac{180°}{n}$, &c.—Orig.

of the angles expressed by $\frac{360°}{n} \times m$, $2 \times \frac{360}{n} \times m$, $3 \times \frac{360}{n} \times m$, &c. And here it may not be amiss to take notice, that the series $\frac{x^m}{m} + \frac{x^{m+n}}{m+n} + \frac{x^{m+2n}}{m+2n} +$ &c. thus determined, is that expressing the fluent of $\frac{x^{m-1}\dot{x}}{1-x^n}$; corresponding to one of the two famous Cotesian forms. From which, and the reasoning above laid down, the fluent of the other form, $\frac{x^{m-1}\dot{x}}{1+x^n}$, may be very readily deduced. For, since the series $(\frac{x^m}{m} - \frac{x^{m+n}}{m+n} + \frac{x^{m+2n}}{m+2n} - \frac{x^{m+3n}}{m+3n}$ &c.) for this last fluent, is that which arises by changing the signs of the alternate terms of the former; the quantities p, q, r, &c. will here, agreeably to a preceding observation, be the roots of the equation $z^n + 1 = 0$; and consequently, α, β, γ, δ, &c. the co-sines of the arcs $\frac{180°}{n}$, $3 \times \frac{180°}{n}$, $5 \times \frac{180°}{n}$, &c. as appears by the foregoing note. So that, making Q, Q', Q'', &c. equal here to the measures of the angles $\frac{180°}{n} \times m$, $3 \times \frac{180°}{n} \times m$, $5 \times \frac{180°}{n} \times m$, &c. the fluent sought will be expressed in the very same manner as in the preceding case; except that the first term, — log. $(1-x,)$ arising from the rational root $p = 1$, will here have no place.

After the same manner, with a small increase of trouble, the fluent of $\frac{x^{m-1}\dot{x}}{1 \pm 2lx^n + x^{2n}}$ may be derived, m and n being any integers whatever. Mr. S. now sets down one example, where the impossible quantities become exponents of the powers in the terms where they are concerned.

The series here given is $1 - x + \frac{x^2}{2} - \frac{x^3}{2.3} + \frac{x^4}{2.3.4} - \frac{x^5}{2.3.4.5}$, &c. = the number whose hyp. log. is $-x$, and it is required to find the sum of every n^{th} term beginning at the first. Here the quantity sought will, according to the general rule, be truly defined by the n^{th} part of the sum of all the numbers whose respective logarithms are $-px$, $-qx$, $-rx$, &c. which numbers, if N be taken to denote the number whose hyp. log. $= 1$, will be truly expressed by N^{-px}, N^{-qx}, N^{-rx}, &c. whence, by writing for p, q, r, &c. their equals 1, $\alpha + \sqrt{\alpha\alpha - 1}$, $\alpha - \sqrt{\alpha\alpha - 1}$, $\beta + \sqrt{\beta\beta - 1}$, $\beta - \sqrt{\beta\beta - 1}$, &c. and putting $\acute{\alpha} = \sqrt{1 - \alpha\alpha}$, $\beta' = \sqrt{1 - \beta\beta}$, &c. we shall have $\frac{1}{n} \times (N^{-px} + N^{-qx} + N^{-rx}$ &c.) $= \frac{1}{n}$ into $N^{-x} + N^{-\alpha x} \times (N^{-\acute{\alpha}x\sqrt{-1}} + N^{\acute{\alpha}x\sqrt{-1}}) + N^{-\beta x} \times (N^{-\beta'x\sqrt{-1}} + N^{\beta'x\sqrt{-1}} +$ &c.) But $N^{-\acute{\alpha}x\sqrt{-1}} + N^{\acute{\alpha}x\sqrt{-1}}$ is known to express the double of the co-sine of the arch whose measure, to the radius 1, is $\acute{\alpha}x$. Therefore we have $\frac{1}{n}$ into $N^{-x} + N^{-\alpha x} \times 2$ co-s. $\acute{\alpha}x + N^{-\beta x} \times 2$ co-s. $\beta'x$, &c. for the true sum, or value proposed to be determined.

The solution of this case in a manner little different, I have given says Mr. S. some time since in another place; where the principles of the general method,

here extended and illustrated are pointed out. I shall put an end to this paper with observing, that if, in the series given, the even powers of x, or any other terms whatever be wanting, their places must be supplied with cyphers; which, in the order of numbering off, must be reckoned as real terms.

Observation of a Lunar Eclipse, made at Lisbon, July 30, 1757. *By J. Chevalier, F. R. S. From the Latin.* p. 769.

At	9^h	15^m	18^s	Beginning of the penumbra.
	9	22	24	Beginning of the eclipse, doubtful.
	9	23	34	Certainly now begun.
	10	55	40	The greatest obscuration.
	12	28	26	End of the eclipse.

CV. Singular Observations on the Manchenille Apple. By J. A. Peyssonel, M.D., F.R.S. From the French.* p. 772.

The cruel effects of the manchenille are well known: its milk, which the savages make use of to poison their arrows, makes the wounds mortal. The rain which washes the leaves and branches causes blisters to rise like boiling oil; even the shade of the tree makes those who repose under it to swell; and its fruit is esteemed a deadly poison.

Of the following facts Dr. P. can vouch for the truth: One Vincent Banchi, of Turin in Piedmont, a strong robust man, and a soldier, of about 45 years of age, belonging to the horse, was a slave with the Turks 11 years, having been taken prisoner at the siege of Belgrade. He was overseer of Dr. P.'s habitation towards the month of July of the year 1756. He was one day walking on the sea-side, and seeing a great number of apples on the ground, was charmed with their beautiful colours and sweet smell, resembling that of the apple called d'apis: he ate of them ignorantly, and found they had a subacid taste; and having eaten 2 dozen, he filled his pockets, and came home eating the rest, till the negroes told him it was mortal.

About an hour afterwards his belly swelled considerably, and he felt a consuming fire in his bowels. He could not keep himself upright; and at night the swelling of his belly increased, with the burning sensation of his bowels. His lips were ulcerated with the milk of the fruit, and he was seized with cold sweats: but the principal negro made him a decoction of the leaves of a ricinus† in water, and made him drink plentifully of it, which brought on a vomiting,

* The manchenille or mancaneel-tree is the *hippomane mancinella* of Linneus: it is elegantly figured in the Appendix to Catesby's Carolina.

† Avellana purgatrix; in French, medicinier.—Orig.

followed by a violent purging; both which continued for 4 hours, during which it was thought he would die. At length these symptoms became less; and the negroes made him walk, and stir about by degrees; and soon after they were stopped. Rice-gruel, which they gave him, put an end to all these disorders; and in 24 hours he had no more ailments nor pain; the swelling of his belly diminished in proportion to his evacuations upward and downward, and he had continued his functions without being any more sensible of the poison. We see by this that the effects of the poison of the manchinelle are different from those of the fish at Guadaloupe.

CVI. Abstract of a Letter from Mr. Wm. Arderon, F. R. S. to Mr. Henry Baker, F. R. S. on giving Magnetism and Polarity to Brass. p. 774.

Mr. A. having made experiments on the magnetism of brass, among many pieces that he had tried, were several that readily attracted the needle; but whether they had this property originally, or received it by hammering, filing, clipping, or any other such like cause, he could not determine. He had a very handsome compass-box made of pure brass, as far as he could judge: the needle being taken out, and placed on a pin fixed properly in a board, and clear of all other magnetics, the box will attract this needle at half an inch distance; and if suffered to touch, will draw it full 90 degrees from the north or south points; and he thought those parts of the box marked north and south attracted the strongest. The cover of the box also attracted the needle nearly as much as the box itself.

As to your supposition, says Mr. A., that iron may be mixed with the brass, I do not know; but I have been informed it cannot be, as brass fluxes with a much less degree of heat than iron, and iron naturally swims on fluid brass. Besides, many of the specimens of brass I have tried were new as they came from the mill, where they were wrought into plates, and I presume were not mixed;* yet these I have given the magnetic virtue to when they had it not; and some pieces of brass, which naturally attract the needle, seem to the eye as fine a bright yellow as any other, and are as malleable as any I ever met with. Pieces of brass without any magnetic power, by properly hammering and giving them the double touch, after Mr. Mitchel's method, I have made attract and repel the needle, as a magnet does, having 2 regular poles. You will observe, when you try this bar, that the same poles repel each other, and the contrary poles attract; which proves this piece of brass to be endued with true magnetic virtue and polarity. However it must be noted, that though the same poles repel each other,

* This refers to Mr. Baker's having supposed, that old iron and old brass may be mixed sometimes, and melted down together.—Orig.

yet like natural magnets, in contact, or nearly so, they attract each other: therefore when you would show the repelling power of this brass bar, you must not bring it nearer the needle than $\frac{2}{10}$ of an inch. Magnetic brass does not attract iron, not even the least particle, as far as I can find: whether this is owing to the weakness of magnetism in the brass, or to some other cause, I do not pretend to know.

I have tried to infuse magnetic virtue in several pieces of copper, lead and pewter; but all my endeavours have not been able to make them attract the needle at all. Indeed, when I have held a piece of pewter that I have tried to make magnetical to the needle, the needle would tremble, but not approach the pewter. I send you another piece of brass, whose either end attracts either of the poles; this I have infused the magnetic virtue into, and can at any time, so as to attract and repel the needle; but, like steel that is set a low blue, it loses that polarity in a few hours; which may arise from its being too short for its weight, or from its different temper of hardness or softness. A third piece I also send, which with all my endeavours I cannot make attract the needle in the least; and yet I can perceive no difference between the appearance of this piece and that of those which do.

It is well known, that brass has been sometimes found to affect and disturb the magnetic needle; but to give magnetism and polarity to brass, has not, that I have yet heard, been before attempted. I therefore have taken the liberty to lay the above account before this Royal Society, and have also brought the pieces of brass mentioned therein, which have been thus made magnetical.

CVII. Of the Sea Polypus. By Mr. H. Baker, F. R. S.* p. 777.

The kinds of sea polypi are understood to be—1st, The polypus, particularly so called, the octopus, preke, or pour-contrel: to which kind the present subject belongs. 2dly, The sepia, or cuttle-fish. 3dly, The loligo, or calamary. And each of these has its different species and varieties.† The ancients add the nautilus; and some sorts of star-fish might perhaps be not improperly ranged among them.

All of the first kind have 8 arms, placed at equal distances round the head; below the arms are 2 eyes, and the body is short and thick. The cuttle-fish and the calamary have each of them 10 arms; of which 8 are shorter ones, tapering gradually to a point from the head, where they all rise to their extremities: the other 2 (frequently called tentacula) are 3 or 4 times as long, perfectly round,

* The species here described by Mr. Baker is the American cuttle-fish, and is not to be confounded with the common cuttle-fish or sepia octopodia of Linneus. It is figured in Seba's Thesaurus, vol. iii. pl. 1, fig. 2 and 3, from a much larger specimen than the present.

† Vide Wilkin's real Character, p. 131. Bellon. Aquat. p. 330.—Orig.

slender, and of an equal thickness for above 2-thirds of their whole length; then spreading into a form nearly like that of the shorter arms. Great numbers of acetabula or suckers are placed somewhat irregularly on each of the shorter arms, and on the spreading parts of the tentacula, where some of the suckers are a great deal larger than the rest. The body of the cuttle-fish is broad and flat, having within it a broad friable white bone; that of the calamary is a sort of cartilaginous case holding the intestines, of a roundish oblong shape, furnished with 2 fins, and having within it a thin transparent elastic substance like isinglass. The mouth of the pour-contrel, cuttle-fish, and calamary, is placed in the fore part of the head, between the arms, having a horny beak, hard and hooked like a parrot's, which some writers call the teeth. The eyes of them all are nearly in the same position.

As the subject under examination resembles in some particulars all the above kinds of polypi, this short account of them may it is hoped render the following description of it the more intelligible: and with the same view, Mr. George Edwards, F.R.S. had been so obliging as to make drawings of the animal itself, in 4 different positions, as in fig. 1, 2, 3, 4, pl. 9. Our polypus is of the pour-contrel kind, and of that species called bolytæna. In fig. 1 is shown the anterior part of this animal, which has much the appearance of a star-fish. Here are 8 arms about 3 inches in length, united at their roots, and placed circularly at equal distances in the same plane, which has a considerable sinking towards the centre. These arms diminish from their rise to their extremities, and end exceedingly small. Near the head they are quadrilateral, but the under side contracting gradually to an edge, they become towards the ends trilateral. On the upper side of each arm are 2 rows of acetabula or suckers, standing in a beautiful order, as close as they can well be placed, and beginning from the centre of all the arms. These suckers are perfectly circular, with edges flat on the top, and a round cavity in the middle of each. They are largest in the widest part of the arm, and lessen as the arm diminishes, till they become so small as hardly to be discernable. It is very difficult to tell their number: Mr. B. counted as far as 50 in a row, but there were many more; and he thinks the 8 arms have more than a thousand on them. They rise some height above the surface of the skin; and wherever they are not, the skin of the arms (unless on the under-side) is granulated like shagreen.

Fig. 2 represents the polypus so placed as to show the situation of the eyes and the form of its body, and also in what manner the arms are turned back in the specimen before us; but we may suppose them thus disposed merely in the act of dyeing, and that when alive they are moveable in all directions. Fig. 3 presents another view of this polypus, its arms extended circularly with their under sides next the eye, and the body so disposed as to show the transverse opening a, the

oval bag issuing from it b, and the pipe rising upward towards the arms c. Fig. 4 shows the polypus with its transverse opening and the pipe rising from it, but without the oval bag; it is figured thus by Rondeletius and Gesner, and the specimen at the British Museum has also this appearance. It is here shown with the arms extended forwards. K is a magnified figure of one of the acetabula or suckers; of which there are 2 rows on each arm of this polypus, as before described.

Mr. Needham, in his description of the suckers of the calamary, (which he had many opportunities of examining while alive, and whose mechanism is probably the same as in those of our polypus) informs us, 'that the action of the suckers depends partly on their shape, which, when they are extended, resembles nearly that of an acorn-cup, and partly on a deep circular cartilaginous ring, armed with small hooks, which is secured in a thin membrane something transparent, by the projection of a ledge investing the whole circumference about the middle of its depth, and not to be extracted without some force. That each sucker is fastened by a tendinous stem to the arm of the animal: which stem, together with part of the membrane that is below the circumference of the cartilaginous ring, rises into and fills the whole cavity when the animal contracts the sucker for action. In this state whatever touches it is first held by the minute hooks, and then drawn up to a closer adhesion by the retraction of the stem and inferior part of the membrane, much in the same manner as a sucker of wet leather sustains the weight of a small stone.' Vide Microscopical Discoveries, p. 22.

M shows one of the cartilaginous rings armed with small hooks. The ring this is drawn from was taken out of a large sucker of a larger polypus. By these suckers the polypus can fix itself to rocks, and prevent its being tossed about in storms and tempests; but their principal use must doubtless be to seize and hold its prey: and to this purpose they are most admirably adapted; for when they are all applied and act together, unless the polypus pleases to withdraw them, nothing can get from it whose strength is insufficient to tear off its arms. Something like these suckers is found by the microscope in the minute fresh-water polype, by which it is able to bind down and manage a worm much larger and seemingly stronger than itself. In like manner the stella arborescens, which may also be called a polypus, though it has not suckers, yet by the hooks along its arms, and the multiplicity of their branchings, which have been counted as far as 80,000, it can, by spreading its arms abroad like a net, so fetter and entangle the prey they inclose when they are drawn together, as to render it incapable of exerting its strength: for however feeble these branches or arms may singly be, their power united becomes surprizing. And we are assured Nature is so kind to all these animals, that if in their struggles any of their arms are broken off,

after some time they will grow again; of which a specimen at the British Museum is an undoubted proof; for a little new arm is there seen sprouting forth in the room of a large one that had been lost.

It is evident from what has been said, that the sea-polypus must be terrible to the inhabitants of the waters, in proportion to its size, and Pliny mentions one whose arms were 30 feet in length; for the close embraces of its arms, and the adhesion of its suckers, must render the efforts of its prey ineffectual either for resistance or escape, unless it be endued with an extraordinary degree of strength. Sea polypi are frequent in the Mediterranean: but Mr. Haviland of Bath, to whom we are obliged for this, which is of a different species, thinks it came from the West Indies, where it is called a cat-fish. That like it in the British Museum also came from thence.

CVIII. *On the Fossil Skeleton of an Animal found in the Alum Rock near Whitby. By Mr. Wooller.* p. 786.

It is in this rock that the ammonitæ, or snake-stones, as they are commonly called, are found, which have doubtless been formed in the exuviæ of fishes of that shape; and though none of that species are now to be met with in the seas thereabouts, yet they in many particulars resemble the nautilus, which is well known. The internal substance of those stones, on a section, appears to be a stony concretion, or muddy spar. Stones of the same matter or substance, in the shape of muscles, cockles, &c. of various sizes, are also found in it, and now and then pieces of wood hardened and crusted over with a stony substance are likewise found in it. Many naturalists have already observed, that among the vast variety of extraneous substances found at several depths in the earth, where it is impossible they should have been bred, there are not so many productions of the earth as of the sea; and it appears by the accounts of authors, both ancient and modern, that bones, teeth, and sometimes entire skeletons of men and animals, have been dug up or discovered in all ages, and the most remarkable for size commonly the most taken notice of. In the first particular this skeleton will probably appear to have belonged to an animal of the lizard kind, quadruped and amphibious; and as to its size, much larger than any thing of that kind ever met with or found in this part of the world; though, from the accounts of travellers, something similar is still to be met with in many of the rivers, lakes, &c. of the other three.

When the annexed drawing of it was taken, Jan. 5, 1758, (fig. 5, pl. 9) there remained no more of the vertebræ than is there expressed; that is, 10 between D and F, and 12 between G and H: but when it was first discovered, about 10 years before, they were complete; and there was besides the appearance of

what was then thought to have been fins, near the back part of the head at A, the same as appeared further backward at E, when this design was made. The vertebræ, &c. now wanting having been either dug up by curious persons, or washed away by the violence of the waves at high water, and the accidental beating about of stones, sand, &c. during that time; the water covering this skeleton several feet at high water in spring tides; the cavities in the rock still remaining as in the design.

The substance of the bones, with their periosteum, on the covered or under side, in most parts remain entire, and their native colour in some places in a good measure preserved, and the teeth with their smooth polish plainly to be discovered. Part of the mandible near the extremity was covered with a shelf of the rock about 3 inches thick; which being cut away and removed, both the mandibles appeared under it complete, with the teeth of the upper and under one, plainly locking or passing by each other. These appeared to be of the dentes exerti or fang kind, as well as all the others in the narrow part of the mandible, and farther backwards they were not observed. From this ledge or shelf the mandible towards B is single, and appears to be the upper one of the living animal; and from the head not being exactly in the line of the body, that part has been inverted, or quite turned over, and the body itself, as appears from the transverse processes of the vertebræ, lies on the right side. There appears one row of teeth only on each side of the mandible, and they are about $\frac{3}{4}$ of an inch asunder.

The mandible BA, the cranium gh, and the vertebræ from D to F, were attempted to be taken up whole; but the bones being rendered extremely brittle, and the rock in which they were fixed being a brittle blackish slate, with joints or fissures running in every direction, would not hold together: the whole therefore fell in many pieces, the vertebræ in the joints only, which makes them easy to join together again, and besides shows very plainly the transverse and spinal processes, with the foramen in the latter for the spinal marrow. It was now that a piece of the os femoris, about 4 inches long, showed itself in the sparry concreted substance at E, together with a piece of the os innominatum, to which it had been articulated or joined. This, with what has been before remarked, will sufficiently prove this to have been an animal of the quadruped, and probably, from the shape of the cranium peculiar to fishes, of the amphibious kind. At the same time many pieces of the costæ or ribs, as broken and crushed up against the vertebræ were plainly visible. The cavities of all the bones were filled with a substance, which appeared the same as the rock itself; and the substance on each side the vertebræ, as they lay, was a mixture of sparry concreted matter with that of the rock itself, which is a blackish slate. The animal, when living, must have been at least 12 or 14 feet long. And the di-

mensions of the whole, or particular parts of the skeleton, may be measured from the scale annexed. aa, &c. are the ammonitæ or snake-stones.

This skeleton lay about 6 yards from the foot of the cliff, which is about 60 yards in perpendicular height, and must have been covered by it probably not much more than a century ago. The cliff there is composed of various strata, beginning from the top, of earth, clay, marle, stones both hard and soft, of various thicknesses, and intermixed with each other, till it comes down to the black slate or alum rock, and about 10 or 12 feet deep in this rock, this skeleton laid horizontally, and exactly as designed. The probability that this cliff has formerly covered this animal, and extended much more into the sea, is not in the least doubted of by those that know it. The various strata, of which it is composed, are daily mouldering and falling down; and the bottom, being the slaty alum rock, is also daily beaten, washed, and worn away, and the upper parts undermined, whence many thousand tuns often tumble down together. Many ancient persons now living, whose testimony can be no way doubted of, remember this very cliff extending in some places 20 yards farther out than it does at present. In short there is sufficient evidence, that at the beginning it must have extended near a mile farther down to the sea than it does at present; and so much the sea has there gained of the land.

These are the principal facts and circumstances attending the situation and discovery of this skeleton; which from the condition it is in, and from the particular disposition of the strata above the place where it is found, seem clearly to establish the opinion, and almost to a demonstration, that the animal itself must have been antediluvian, and that it could not have been buried or brought there any otherwise than by the force of the waters of the universal deluge. The different strata above this skeleton never could have been broken through at any time, in order to bury it, to so great a depth as upwards of 180 feet; and consequently it must have been lodged there, if not before, at least at the time when those strata were formed, which will not admit of a later date than that above mentioned.

P.S. In the 49th vol. p. 639, of the original Philosophical Transactions, or vol. 10, p. 712 of these Abridgments, an animal is described by Mr. Edwards, which was brought from the Ganges, and resembles this in every respect. He calls it lacerta (crocodilus) ventre marsupio donato, faucibus merganseris rostrum æmulantibus.

CIX. On the Phœnician Numeral Characters anciently used at Sidon. By the Rev. J. Swinton, M. A. of Christ Church, Oxon., F. R. S. p. 791.

Reverend Sir,

Having, by the assistance of the Palmyrene numeral characters, lately made a

discovery, which may perhaps hereafter be of considerable service to chronology; I could not longer defer, though now deeply engaged in other matters, communicating it to the Royal Society. Nor will the memoir containing this, I flatter myself, be deemed altogether unworthy the attention of that learned and illustrious body. For, unless he be greatly deceived, it will bid fair, Mr. S. thinks, that the Phœnician dates of several ancient Sidonian coins evince the notation of the Phœnicians, at least those of Sidon, when they first appeared, were extremely similar to, if not nearly the same with, that of the Palmyrenes. To prove this, he describes below several such coins in his possession.

Fig. 1, pl. 10, is a small brass coin of Sidon; exhibits on the reverse 3 Phœnician letters, that form the word SIDON, over the prow of a ship, the usual symbol of the city where it was struck. The first 2 characters in the exergue, though somewhat imperfect, appear manifestly enough to be schin and tzade; as the former occurs on the Palmyrene marbles, and the latter on several very valuable Phœnician coins. The others so nearly resemble the numeral characters of the Palmyrenes, that they may undoubtedly be considered as pointing out to us a date. Which if we admit, the schin and tzade will seem to be the initial letters of the words שנת צידן, THE YEAR OF SIDON, OR IN THE YEAR OF SIDON. That the first of the numeral characters here stands for 20, we may infer from the correspondent one of the Palmyrenes, to the form of which it is by no means unlike. This will likewise be confirmed by the dates preserved on other Phœnician coins, which will be immediately produced. The next, denoting a less number, and not representing 5, which we find always expressed by minute right lines on the Sidonian medals, must indubitably occupy the place of 10. The 6 following strokes, after what has been just observed, will be acknowledged to add 6 to the foregoing numbers; so that the inscription in the exergue will no longer remain a mystery, the whole only importing, IN THE YEAR OF SIDON XXXVI.

Fig. 2 represents one of three other coins of Sidon, of almost entirely the same type; only one of them exhibits a date in Greek numerals, and 2 bear Phœnician dates. The Greek numerals are EOT, 375; and the Phœnician correspond with the numbers 120, 127, to both of which are prefixed the above-mentioned initial letters. We meet with draughts of 2 similar medals in Arigoni, adorned with characters, expressing the number 128, 130. All these coins present to our view a turrated head and a branch of palm, pointing out to us the country to which they belong; and on the reverse the usual symbol of Sidon. The year denoted by the Greek date EOT, is the 375th of the æra of Seleucus; and those denoted by the Phœnician numerals answer to the 120th, 127th, 128th, and 130th, of the proper æra of Sidon, as will be hereafter more fully evinced. Hence we may certainly collect, that these pieces were struck at Sidon in the years of Christ 11, 18, 19, 21, and 64.

Three coins of Sidon, (fig. 3) different from the former, occur in Sig. Haym, and 7 more in Mr. S.'s little cabinet, whose type is altogether the same, with Phœnician dates, preceded by the 2 aforesaid initial letters upon them. To which may be added 5 preserved in the noble cabinet bequeathed to Christ-Church, Oxon, by archbishop Wake, and another in the valuable collection of the Rev. Dr. Barton, canon of the said collegiate church, and a worthy member of this Society. One side of these medals all exhibit the head of Jupiter, and on the reverse the prow of a ship, the common symbol of Sidon. Most of them had various Phœnician letters at first impressed on the upper part of the reverse, and one of them (which is pretty remarkable) nearly the same characters there that appear in the exergue. The first of the coins mentioned here was struck in the year of Sidon 5. This has been perfectly well preserved, and is more curious than any of the rest; which were emitted from the mint at Sidon in various years of the proper æra of that city, viz. the 107th, 108th, 110th, 111th, 112th, 114th, 115th, 116th, 117th, and 119th. The most ancient of the Phœnician coins now considered, preceded the commencement of the Christian æra 104 years, and is consequently 153 years older than the earliest Palmyrene inscription that has hitherto come to our hands.

Some years before Mr. S. published a small brass medal of Sidon, with the heads of Jupiter and Juno on one side, and the prow of a ship on the reverse (fig. 4); two more coins of the same type he has since acquired, and another may be seen in Sig. Haym. These 4 pieces only exhibit the years of Sidon 125 and 132.

Mr. S.'s collection also affords two (fig. 5) other Phœnician medals of Sidon, and archbishop Wake's noble cabinet one, of the same type, with different Phœnician dates in the exergue. To these may be added 5, with the publication of which the learned world has been obliged by Sig. Arigoni. The anterior faces of these coins are adorned with a veiled head, representing the genius of the city where they were struck; and the reverses with a human figure leaning on a pillar, and holding a branch of palm in its right hand. Several Phœnician letters also appear, which may perhaps at first sight seem to render it somewhat doubtful, whether the medals belong to Sidon or not. But every suspicion arising hence must immediately vanish, when we cast our eyes on the two initial elements, and the numeral characters in the exergue; which clearly enough indicate the pieces to have been struck at Sidon, in the 83d, 87th, 95th, 105th, 106th, 108th, 114th, and 116th years of the æra peculiar to that city. A Phœnician coin of Sidon likewise occurs in one of Sig. Arigoni's plates, and another (fig 6) in Mr. S.'s collection, with the turrited head and branch of palm visible on 3 of the medals above described, which indisputably appertain to that city, together

with the very Phœnician letters and symbol impressed on the Sidonian coins now before me.

VI.—Fig. 7 is another brass Phœnician medal of Sidon, not a little resembling those above-mentioned, both in workmanship and size, presenting on one side the head of Jupiter, and on the other a human figure with a lance in its right hand. This coin, which has never yet been published, is adorned with a Phœnician legend on the reverse, different from those of all the others that have hitherto appeared. The date impressed in the exergue answers to the 26th year of Sidon.

VII.—Fig. 8 is a Phœnician medal of Sidon, being a small brass one, with a veiled head on the anterior face, and the prow of a ship on the reverse. The year of Sidon, preserved in the exergue is 74.

The last Phœnician medals, Mr. S. at present produces, to settle the point in view, are (fig. 9) two entirely agreeing both in type and form, as remarkable as any of the others. They exhibit on one side the head of Jupiter laureated, with a beard; and on the reverse a double cornucopia, with 3 or 4 Phœnician elements, one or two of which are in a great measure defaced. A brass medal of Sidon occurs in Archbishop Wake's collection, as well as one in Mr. S.'s with the head of Jupiter done exactly after the same manner as the present pieces, and Europa carried by a bull on the reverse; which, exclusive of the inscriptions in the exergue, demonstrate the latter to belong to Sidon. The first of Mr. S.'s was struck in the 143d year of the proper æra of that city, and the 2d 5 years after.

For the further illustration of what has been here advanced, it will be requisite to observe, that two æras were anciently followed at Sidon; the æra of Seleucus, and another peculiar to the inhabitants of that city. On the Greek brass coins of Sidon, according to F. Frœlich, both these epochs seem to have been used. However, the supputation pointed out to us by the date on the Greek medal above-mentioned was undoubtedly made according to the æra of Seleucus; since otherwise the year exhibited by that date must have been nearly coincident with the 266th of Christ, which by those versed in this kind of literature will never be allowed. For had the piece presented to our view so recent a date as Sidon first became a Roman colony in the reign of Elagabalus, above 40 years before; the reverse ought to have been adorned with some other letters intimating this, as were those of the Sidonian coins posterior to that event. As certain is it that all the Phœnician medals of Sidon, whose numeral characters have been interpreted here, acknowledge no other epoch than the proper one of that city, which commenced in the year of Rome 643.

The powers of the Phœnician numeral characters anciently used at Sidon, now discovered, having been for many ages unknown; they are now deduced

from the coins above described. Mr. S. has constructed a table (vol. x. pl. 13) of the numeral characters themselves, from unity to a thousand; which will demonstrate, in the clearest manner possible, the great affinity between them and those of the Palmyrenes. From this table it plainly appears, that the people of Sidon had no particular character to denote 5, while the Phœnician numerals here explained were in vogue among them; that they expressed 20 by a character, during that period, not very different from the correspondent one used at Tadmor; and that in all other respects the Phœnician notation then prevailing at Sidon was, in a manner, the same with that of the Palmyrenes. See pl. 13, vol. x. It may not be improper to observe, that 2 of the Sidonian coins above considered, exhibit the Phœnician word מא, equivalent to the Hebrew מאת, and Syriac מאא, a hundred, instead of the centenary numeral character. This, in conjunction with the appearance of that character, occupying the very place of the term מא, on others of those coins, first induced him to believe that the inscription preserved by every one of them in the exergue could be nothing else but a date.

The Palmyrene and Phœnician numerals, deduced from coins and inscriptions, may perhaps be thought not unworthy a place among the arithmetical characters of various nations, formerly collected by Bishop Beveridge; and consequently may be allowed to render somewhat more complete the chronological institutions, or rather the chronological arithmetic, of that learned and judicious author.

CX. Of the Irregularities in the Motion of a Satellite arising from the Spheroidical Figure of its Primary Planet: in a Letter to the Rev. J. Bradley, D. D., F. R. S. By Mr. Charles Walmssley, F. R. S. Dated from Bath, Oct. 21, 1758. p. 809. *From the Latin.*

Since the time that astronomers have been enabled, by the perfection of their instruments, to determine with great accuracy the motions of the celestial bodies, they have been solicitous to separate and distinguish the several inequalities discovered in these motions, and to know their cause, quantity, and the laws according to which they are generated. This seems to furnish a sufficient motive to mathematicians, wherever there appears a cause capable of producing an alteration in those motions, to examine by theory what the result may amount to, though it comes out never so small: for as one can seldom depend securely upon mere guess for the quantity of any effect, it must be a blameable neglect entirely to overlook it without being previously certain of its not being worth our notice.

Finding therefore it had not been considered what effect the figure of a planet differing from that of a sphere might produce in the motion of a satellite revolving about it, and as it is the case of the bodies of the earth and Jupiter,

which have satellites about them, not to be spherical but spheroidical, I thought it worth while to enter upon the examination of such a problem. When the primary planet is an exact globe, it is well known that the force by which the revolving satellite is retained in its orbit, tends to the centre of the planet, and varies in the inverse ratio of the square of the distance from it; but when the primary planet is of a spheroidical figure, the same rule then no longer holds: the gravity of the satellite is no more directed to the centre of the planet, nor does it vary in the proportion above-mentioned; and if the plane of the satellite's orbit be not the same with the plane of the planet's equator, the protuberant matter about the equator will by a constant effort of its attraction endeavour to make the two planes coincide. Hence the regularity of the satellite's motion is necessarily disturbed, and though upon examination this effect is found to be but small in the moon, the figure of the earth differing so little from that of a sphere, yet in some cases it may be thought worth notice; if not, it will be at least a satisfaction to see that what is neglected can be of no consequence. But however inconsiderable the change may be with regard to the moon, it becomes very sensible in the motions of the satellites of Jupiter both on account of their nearer distances to that planet when compared with its semidiameter, as also because the figure of Jupiter so far recedes from that of a sphere. This is shown and exemplified in the 4th satellite; in which case indeed the computation is more exact than it would be for the other satellites: for as my first design was to examine only how far the moon's motion could be affected by this cause, I suppose the satellite to revolve at a distance somewhat remote from the primary planet, and the difference of the equatoreal diameter and the axis of the planet not to be very considerable. There also arises this other advantage from the present theory, that it furnishes means to settle more accurately the proportion of the different forces which disturb the celestial motions, by assigning the particular share of influence which is to be ascribed to the figure of the central bodies round which those motions are performed.

I have added at the end a proposition concerning the diurnal motion of the earth. This motion has been generally esteemed to be exactly uniform; but as there is a cause that must necessarily somewhat alter it, I was glad to examine what that alteration could amount to. If we first suppose the globe of the earth to be exactly spherical, revolving about its axis in a given time; and afterwards conceive that by the force of the sun or moon raising the waters, its figure be changed into that of a spheroid, then according as the axis of revolution becomes a different diameter of the spheroid, the velocity of the revolution must increase or diminish: for since some parts of the terraqueous globe are removed from the axis of revolution and others depressed towards it, and that in a different proportion as the sun or moon approaches to or recedes from the equator,

when the whole quantity of motion which always remains the same is distributed through the spheroid, the velocity of the diurnal rotation cannot be constantly the same. This variation however will scarcely be observable, but as it is real, it may not be thought amiss to determine what its precise quantity is. I am sensible the following theory, as far as it relates to the motion of Jupiter's satellites, is imperfect, and might be prosecuted further.

LEMMA 1. To find the gravity of a far distant body, to the circumference of a circle from the particles of matter constantly attracting in the duplicate ratio of the distances inversely.

Let NIK (fig. 6, pl. 9) be the circumference of a circle, to which gravitates every point of a distant body S placed without the plane of the circle. To this plane draw the perpendicular SH; and through the centre X draw the right line HXK cutting the circle in I and K; also draw SR parallel to HX; then produce SH to the given distance SD; also draw DC parallel to HX, and XC to SD. Then, drawing any chord MN cutting the diameter IK perpendicularly in L, on SR demit the perpendiculars MR, NR, meeting in R; and joining SM, SN, it will be $SM = SN$, $MR = NR$, $SR = HL$.

Now call $SD = k$, HX or $DC = h$, $XL = x$, $CX = z$, $XI = r$; then will $HL = h - x$, and $SH = k - z$. But SM is to SH, as the attraction $\frac{1}{SM^2}$ of the body S towards the particle M in the direction SM, to its attraction in the direction SH, which therefore will be $\frac{SH}{SM^3}$; but $SR = HL$, and $SM^2 = SR^2 + MR^2 = SR^2 + SH^2 + ML^2$; hence $\frac{SH}{SM^3} = \frac{SH}{(HL^2 + SH^2 + ML^2)^{\frac{3}{2}}}$; and drawing mn parallel to MN, the force by which the body S is drawn to the small arcs Mn, Nn, will be expounded by $\frac{SH \times 2Mm}{SM^3} = SH \times 2Mm \times (HL^2 + SH^2 + ML^2)^{-\frac{3}{2}}$. But $HL^2 + SH^2 + ML^2 = k^2 - 2kz + z^2 + h^2 - 2hx + r^2$; hence, putting $kk + hh = ll$, then $(HL^2 + SH^2 + ML^2)^{-\frac{3}{2}} = \frac{1}{l^3} + \frac{3kz}{l^5} + \frac{3hx}{l^5} - \frac{3rr}{2l^5} - \frac{3zz}{2l^5} + \frac{15k^2z^2}{2l^7} + \frac{khzx}{2l^7} + \frac{15h^2x^2}{2l^7}$, neglecting the further terms on account of the great distance of the body S. Therefore, if d be written for the circumference IMKN, the gravity of the body S to that whole circumference in direction SH, or the fluent of the fluxion $SH \times 2Mm \times (HL^2 + SH^2 + ML^2)^{-\frac{3}{2}}$ becomes $(k - z)\, d \times (\frac{1}{l^3} + \frac{3kz}{l^5} - \frac{3rr}{2l^5} - \frac{3zz}{2l^5} + \frac{15k^2z^2}{2l^7} + \frac{15h^2r^2}{4l^7}$. In a similar manner will be obtained the gravity of the same body S according to SR. Q. E. I.

LEMMA 2. To determine the gravity of a very distant body to an oblate spheroid.

Retaining what has been proved in the former lemma; let C be the centre of the spheroid, parallel to the equator, of which let the circle IMK be parallel. Let the greater semiaxis of the spheroid be a, the less semiaxis b, their difference c, supposed very small; also D the circumference of the equator. With the centre

C, and radius equal to the less semiaxis, conceive a circle to be described cutting IK in i; then the gravity in direction SD, by which the body S is urged towards the matter situated between the circumference IMKN and the circumference described with the centre X, and radius Xi, will be equal to the gravity determined in the preceding lemma drawn into the line Ii. But Ii : c : : IX : a, and d : D : : IX : a; hence I$i \times d$: D $\times$ c : : IX^2 : a^2, that is, from the nature of the ellipse, (because CX $=$ z, and IX $=$ r,) I$i \times d$: D $\times$ c : : $b^2, - z^2 : b^2$, therefore I$i \times d = \frac{D \times c}{bb}$ $(b^2 - z^2,)$ also $rr = aa - \frac{a^2z^2}{bb}$; but $bb - zz$ may be written for rr in the following calculous, by reason of the smallness of the difference of the semiaxis into which all the terms are drawn. Therefore the gravity of the body S on the matter between the aforesaid circumferences will be expressed by

$\frac{D \times c}{bb} \times (bb - zz) \times (k - z) \times (\frac{1}{l^3} + \frac{3kz}{l^5} - \frac{3b^2}{2l^5} - \frac{15z^2}{4l^5} + \frac{15b^2h^2}{4l^7} + \frac{45k^2z^2}{4l^7}.)$ And if there be added the gravity on the similar matter on the other side of the centre C at an equal distance from the centre, because then CX or z become negative, the gravity of the body S on this double matter will be

$\frac{D \times c}{bb} \times (bb - zz) \times (\frac{2k}{l^3} - \frac{6kz^2}{l^5} - \frac{3kb^2}{l^5} + \frac{15k^3z^2}{l^7} + \frac{15kh^2b^2}{2l^7} - \frac{15kh^2z^2}{2l^7}.)$ Now draw this gravity into $\dot{z}$, and taking the fluent or sum of all the gravities, making $z = b$, the total gravitation of the body S on the whole matter above the interior globe in the direction SD perpendicular to the equator, produces D $\times$ c $\times$ $(\frac{4kb}{3l^3} - \frac{4kb^3}{5l^5} + \frac{2kh^2b^3}{l^7}.)$ By a like reasoning the gravitation of the body S on the same matter in the direction SR parallel to the equator, will be found equal to D $\times$ c $\times$ $(\frac{4hb}{3b^3} + \frac{2hb}{5l^5} - \frac{2hk^2b^3}{l^7}.)$ Then if there be added the gravitation of the body S on the interior globe, viz. on the one side $\frac{2kb^3D}{3al^3}$, and on the other $\frac{3hb^3D}{3al^3}$, it will give the gravity of the body S on the whole spheroid. Q. E. I.

CoROL. Therefore the gravity of the body S in direction SD, is to the gravity of the same in direction SR or DC, on the matter of the spheroid incumbent on the interior globe, as $\frac{2k}{3} - \frac{2kb^2}{5l^2} + \frac{kh^2b^2}{l^4}$ to $\frac{2h}{3} + \frac{hb^2}{5l^2} - \frac{hk^2b^2}{l^4}$; and therefore, if the former gravity be expounded by k, the latter will be expressed by $h - \frac{3hb^2}{5l^2}$ very nearly. Hence; since DC $= h$, it appears that the gravity of the body S on the oblate spheroid, does not tend to the centre C, but to a point c of the line DC in the plane of the equator lying nearer to the point D.

PROP. 1. To determine the Forces Disturbing the Motion of a Satellite Revolving about its Primary.—Let now the aforesaid spheroid represent any planet, and the body S a satellite revolving about the planet as a primary. The quantity of matter incumbent on the interior globe of the spheroid is equal to

$\frac{4b^2cD}{3a}$ or $\frac{4bcD}{3}$ nearly; and if that matter were placed in the centre C of the spheroid, it would attract the satellite S in direction SC with the force $\frac{4bcD}{3l^2}$, which reduced to the direction SD is $\frac{4bckD}{3l^3}$, and to the direction DC is $\frac{4bchD}{3l^3}$. Since then the force $\frac{4bcD}{3l^2}$ does not disturb the motion of the satellite, as it tends to the centre of motion, and it is reciprocally proportional to the square of the distance from the same centre, the other two forces, $\frac{4bckD}{3l^3}$, $\frac{4bchD}{3l^3}$, into which that is resolved, will also not disturb that motion. Therefore from the force $D \times c \times (\frac{4kb}{3l^3} - \frac{4kb^3}{5l^5} + \frac{2kh^2b^3}{l^7})$ take the force $\frac{4bckD}{3l^3}$, and from the force $D \times c \times (\frac{4hb}{3l^3} + \frac{2hb^3}{5l^5} - \frac{2hk^2b^3}{l^7})$ take $\frac{4bchD}{3l^3}$, and there will remain the forces $D \times c \times (-\frac{4kb^3}{5l^5} + \frac{2kh^2b^3}{l^7})$, $D \times c \times (\frac{2hb^3}{5l^5} - \frac{2hk^2b^3}{l^7})$, the disturbing forces of the satellite S. Let Sr (fig. 7,) denote the force $D \times c \times (\frac{2hb^3}{5l^5} - \frac{2kh^2b^3}{l^7})$, and let it be resolved into the force Sq tending to the centre C of the primary planet, and which, because of the similar triangles Srq, SDC, is equal to $D \times c \times (\frac{2b^3}{5l^4} - \frac{2k^2b^3}{l^6})$, (putting as before SD $= k$, DC $= h$, SC $= l$,) and into the force rq parallel to SD and equal to $D \times c \times (\frac{2kb^3}{5l^5} - \frac{2k^3b^3}{l^7})$; and this latter force taken from the force $D \times c \times (-\frac{4kb^3}{5l^5} + \frac{2kh^2b^3}{l^7}$, will leave $D \times c \times \frac{4kb^3}{5l^5}$ for the disturbing force in the direction SD. Hence, since the total mass of the planet is $\frac{4}{3}$ abD, the whole gravity of the satellite on the planet will be $\frac{2abD}{3l^2}$ nearly, or $\frac{2b^2D}{3l^2}$; and this gravity is to the force $D \times c \times \frac{4kb^3}{5l^5}$, as 1 to $\frac{6kbc}{5l^3}$.

Then of this force $D \times c \times \frac{4kb^3}{5l^5}$, in direction SD, that part which acts in direction SC, is $D \times c \times \frac{4k^2b^3}{5l^6}$; which added to the force Sq, gives $D \times c \times (\frac{2b^3}{5l^4} - \frac{6k^2b^3}{5l^6})$ the disturbing force tending to the centre of the primary planet; and this force is to the satellite's gravity $\frac{2b^2D}{3l^2}$ on the primary, as $\frac{3bc}{5l^2} - \frac{5k^2bc}{5l^4}$ to 1. Q. E. I.

Corol. Let CK (fig. 8,) denote the line of intersection of the planes of the planet's equator and satellite's orbit; and resolve the force SD $= \frac{6kbc}{5l^3}$, which acts perpendicularly to the plane of the equator, into the force DR perpendicular to the plane of the satellite's orbit, and into the force SR lying in the same plane. Produce SR till it meet CK in K; then SK will be perpendicular to CK, and the plane SDK perpendicular to the plane of the satellite's orbit; and because of the

similar triangles SDK, SRD, if m denote the sine and n the cosine of the angle SKD, viz. of the inclination of the satellite's orbit to the planet's equator, radius being 1; then will $DR = SD \times n = \frac{6kbcn}{5l^3}$, and $SR = SD \times m = \frac{6kbcm}{5l^3}$; 1 being the total gravity of the satellite on its primary. Now because the force SR lies in the plane of the satellite's orbit, it changes not the situation of that plane; indeed it accelerates or retards the motion of the revolving satellite; but this acceleration or retardation, because of the shortness of the time, does not amount to any sensible quantity. The force DR perpendicular to the same plane continually changes its situation, and generates the motion of the nodes, defined in the following proposition.

PROP. 2. To find the Motion of the Node arising from the aforesaid cause.

By the motion of the node, in this prop. I mean the motion of the intersection of the planes of the planet's equator and of the satellite's orbit; and the satellite's orbit I suppose to be very nearly circular. Let S be the place of the satellite in its orbit SN, whose centre is C, (fig. 9); SF an arc described with the centre C perpendicular to the circle of the planet's equator FN; SB an arc described with the same centre perpendicular to the orbit SN; and SB take the lineola Sr equal to double the space which the satellite can run through, when impelled by the force DR determined in the preceding corollary, while it describes the small arc pS in its own orbit; through the points r, p, with the centre C, describe the circle rpn cutting the equator in n, which will show the situation of the satellite's orbit after that particle of time, the node N being changed to n. Join SC, CN, and draw SH perpendicular to the line of the nodes CN, also Nm perpendicular to rpn.

Now since the lineolas Sr, Nm, are as the sines of the arcs Sp, SN, it will be Sp : Sr : : SH : Nm; then in the right-angled triangle Nmn, it will be m : 1 : : Nm : Nn; hence by compos. of ratios S$p \times m$: Sr : : SH : $Nn = \frac{Sr \times SH}{Sp \times m}$: and the Sp being given, Nn or the motion of the node is as Sr × SH. In the rightangled spherical triangles SFN, the sine of the angle N, (that is, the angle of the inclination of the satellite's orbit to the planet's equator,) is to the sine of the arc SF, as radius is to the sine of the arc SN, that is, $m : \frac{k}{l} : : 1 : SH$, and therefore $\frac{k}{l} = m \times SH$; therefore $\frac{k}{l}$ is as SH. But the force Sr is as $\frac{k}{l}$ by the corol. of the preceding prop. and therefore is as SH: hence Sr × SH, and so Nn also, is as SH^2, that is, the horary motion of the node, generated by the foregoing force, is in the duplicate ratio of the satellite's distance from the node. And because the sum of all the SH^2, in the whole periodical time of the planet, is half the sum of all the SC^2, therefore the periodic motion is the half of that which, if the satellite were always in its greatest declination from the planet's

equator, could be generated in the same time. Let then the satellite be in its greatest declination, or inquadrature with the node, then will SN be a quadrant of a circle, and Nm the measure of the angle Npm or spr, in which case Nn, or the horary motion of the node, will be to Nm, that is to the angle spr, as 1 is to m; but the angle spr, is to double the angle subtended by the versed sine of the arc sp described in the same time by the gravity of the satellite to the primary, that is, to the angle sc$p$ which is the horary motion of the satellite about the primary, as the force sr is to the gravity of the satellite to the primary, that is, by corol. to prop. 1, as $\frac{6kbcn}{5l^3}$ to 1, or, because in this case $\frac{k}{l} = m$, as $\frac{6bcmn}{5l^2}$ to 1. Hence, by joining the ratios, the horary motion of the node is to the horary motion of the satellite, as $\frac{6bcn}{5l^2}$ to 1 : and if S denote the apparent periodic time of the sun, and L the periodic time of the satellite about its primary, since the horary motion of the satellite is to the horary motion of the sun as S is to L, the horary motion of the node will be to the horary motion of the sun, as $\frac{6bcn}{5l^2} \times \frac{S}{L}$ to 1; and in the same ratio will the annual motion of the node be to the sun's annual motion, that is to 360°. Therefore, if the satellite continued the whole year at its greatest declination from the equator of the primary, the aforesaid force arising from the spheroidical figure of the primary, will generate in the same time a motion of the node $= \frac{6bcn}{5l^2} \times \frac{S}{L} \times 360°$; and from what is said above the true annual motion of the node will be the half of this, viz. $\frac{3bcn}{5l^2} \times \frac{S}{L} \times 360°$. Q. E. I.

COROL. If computation be made for the moon, assuming the medium inclination of her orbit to the terrestrial equator, n will be the co-sine of 23° 28½′; and putting the earth's semiaxis $b = 1$, the moon's mean distance from the earth's centre will be $l = 60$ nearly; and hence on the hypothesis that the difference of the semiaxes $c = \frac{1}{229}$, it will be $\frac{3bcn}{5l^2} \times \frac{S}{L} \times 360° = 11\frac{1}{2}''$; and if it should be $c = \frac{1}{177}$, the earth remaining uniformly dense, that motion will be $= 15''$. This will be the annual regressive motion of the moon's nodes in the plane of the terrestrial equator, which reduced to the ecliptic, as will be afterwards taught, for the various position of the nodes, becomes much quicker.

But this motion of the intersection of the orbits of Jupiter's satellites with the plane of his equator will be much greater; and it will be computed sufficiently accurate by the above formula, at least for the satellites not too near to Jupiter. Thus, for the 4th satellite it will be L $= 16^d\ 16^h\ 32^m$, $b = 1$, $l = 25{\cdot}299$ nearly, and the difference of Jupiter's semiaxes $\frac{1}{13}$; and putting the inclination of that satellite's orbit to Jupiter's equator $= 3°$, n will be the

co-sine of this inclination, and hence $\frac{3bcn}{5l^2} \times \frac{S}{L} \times 360^\circ = 34'$ nearly, the annual motion of the 4th satellite's nodes in antecedentia in the plane of Jupiter's equator.

Prop. 3. To Reduce the Motion of the Moon's Nodes, above determined, to the Ecliptic.——Let NAD (fig. 10,) be the equator, AGE the ecliptic cutting the equator in A, E the vernal equinox, A the autumnal, LGN the moon's orbit cutting the ecliptic in G and the equator in N, LD a great circle perpendicular to the equator; and let DN, LN be quadrants of circles. In a given time by the aforesaid force, let the intersection N be transferred to n, and describe the circle Lgn for the situation of the lunar orbit after that time, and cutting the ecliptic in g. Also, for brevity's sake, call the intersection N the equatorial node, and the intersection G the ecliptic node. Then, drawing Nm, Gd perpendiculars on the moon's orbit, it is Nn : Nm : : 1 : sin. GNA, and Nm : Gd : : 1 : sin. LG, also Gd : Gg : : sin. Ngd : 1 ; hence uniting the ratios gives Nn : Gg : : sin. Ggd : sin. GNA × sin. LG, and therefore Gg = Nn × $\frac{\text{sin. GNA} \times \text{sin. GL}}{\text{sin. G}gd}$. Write s for the sine and t for the cosine of the angle Ggd of the inclination of the moon's orbit to the ecliptic, to radius 1 ; v for the sine and u the co-sine of the arc EG ; p for the sine and q the co-sine of the obliquity of the ecliptic; then by the resolution of the spherical triangle GAN, it will be cos. GNA $= n = qt + psu$, and hence sin. GNA $= \sqrt{(1 - q^2t^2 - 2pqstu - p^2s^2u^2;)}$ but 1 may be written for t, and the term $p^2s^2u^2$ may be rejected on account of its smallness, s being the sine of $5^\circ\ 8\frac{1}{2}'$; hence then sin. GNA $= \sqrt{(pp - 2pqsu)}$; also sin. GNA : sin. GA or v : : sin. GAN or p : sin. GN, therefore sin. GN or cos. LG $= \frac{pv}{\text{sin. GNA}}$, and sin. LG $= u - \frac{qbvv}{p}$, also sin. GNA × sin. LG $= pu - qs$ very nearly. Therefore Gg = Nn × $\frac{pu - qs}{s}$, which is the motion of the lunar nodes in the given time in the plane of the ecliptic: which if that given time be the solar year, Nn will $= \frac{3bcn}{5l^2} \times \frac{S}{L} \times 360^\circ$, hence that annual ecliptic motion of the nodes, neglecting any change in the situation of the nodes from any other causes in that time, gives

$$\frac{3bc}{5l^2} \times \overline{qt + psu} \times \frac{pu - qs}{s} \times \frac{S}{L} \times 360^\circ, \text{ or also } \frac{3bcq}{5l^2} \times \frac{pu - qs}{s} \times \frac{S}{L} \times 360^\circ$$

nearly. Q. E. I.

Prop. 4. To Determine the Variation of the Inclination of the Lunar Orbit to the Plane of the Ecliptic arising from the Spheroidal Figure of the Earth.—Let ANH (fig. 11) be the equator, AG the ecliptic, and A the point of the autumnal equinox: let NGRM be the moon's orbit, cutting the ecliptic in G and the equator in N, in which take the arcs NL, GR, equal to quadrants of circles. Now if the equatorial node N in a particle of time by the aforesaid force be supposed transferred to n, and through L be described the circle nLr, it will show this

situation of the moon's orbit after that time; and if on the same be demitted the perpendiculars NM, RR, the latter RR will denote the variation in the inclination of the lunar orbit to the ecliptic generated in the same time. Also $Nn : Nm :: 1 : m$, and $Nm : Rr :: 1 : \sin. LR$; but because $NL = GR$, $NG = LR$; hence, uniting the ratios, $Nn : Rr :: 1 : m \times \sin. NG$: whence it appears that the momentary variation of inclination is proportional to the sine of the distance of the moon's ecliptic node from the equatorial node. On the diameter NM demit the perpendicular GK, and Gh being the decrement of the arc NG made while the equatorial node N described the arc Nn, make hk parallel to GK, then it will be $1 : GK$ or $\sin. NG :: Gh : Kk$; and therefore now it will be $Nn : Rr :: Gh : m \times Kk$; and hence the sum of all the variations Rr, generated in the time while the ecliptic node G describes the arc MG, will be to the sum of as many motions Nn, that is to the motion of the equatorial node N made in the same time, as the sum of all the $Kk \times m$, to the sum of as many arcs Gh, that is, as $m \times MK$ to MG. Let NH of the node N in the time of a revolution of the node G from the one equinox to the other, then the variation of the inclination generated in the same time, or the total variation, will be $\frac{2m \times NH}{MGN}$. Hence, since $\frac{NH}{MGN}$ expresses the ratio of the motion of the equatorial node to the motion of the ecliptic node, there results the following theorem: the motion of the moon's ecliptic node is to the motion of the equatorial node, as double the sine of the mean inclination of the lunar orbit to the equator, to the sine of the total variation in the inclination of the same orbit to the ecliptic.

In this computation I assumed the mean inclination of the lunar orbit to the equator, viz. $23° 28\frac{1}{2}'$, since in the revolution of the node it is increased as much on the one side as diminished on the other. But the motion of the moon's ecliptic node, is to the motion of her equatorial one, as $19° 20\frac{1}{2}'$ to $11\frac{1}{2}''$ or $15''$, or as 6055 or 4642 to 1, hence by the above theorem the total variation in the inclination comes out $27''$ or $35''$, according as the difference of the earth's axis is $\frac{1}{229}$ or $\frac{1}{177}$. By this quantity then the inclination of the lunar orbit, to the ecliptic will be increased in the moon's ascending node passing from the vernal equinox to the autumnal, and equally diminished in the other half of the node's revolution. In any place G between the equinoxes, the variation of the inclination is to the total variation, as the versed sine of the arc MG is to the diameter, as is evident; or the difference between half the total variation and the variation sought, is to the said half total variation, as the cosine of the arc MG to radius, that is as $u - \frac{qsvv}{p}$ to 1. Q. E. I.

PROP. 5. To Investigate the Motion of the Apses in a Satellite's Orbit nearly Circular, so far as arises from the Spheroidal Figure of the Primary Planet.

By prop. 1, the disturbing force, by which the satellite is urged towards the

centre of the primary planet, is to the satellite's gravity to its primary, as $\frac{3bc}{5l^2} - \frac{9bck^2}{5l^4}$ to 1; or, because by prop. 2, $\frac{k}{l}$ is $= m \times$ SH (fig. 9), viz. putting m for the sine of the inclination of the planet's orbit to the equator of the primary, and writing y for SH, as $\frac{3bc}{5l^2} \times (1 - 3m^2y^2)$ to 1; and the sum of these forces in the whole circumference whose radius is 1, is to the gravity of the satellite as often taken, as $\frac{3bc}{5l^2} \times (1 - \frac{3}{2}m^2)$ to 1. Therefore the mean force, which may be supposed to act uniformly on the satellite, while it makes its period in an orbit nearly circular, is to its gravity to the primary, as $\frac{3bc}{5l^2} \times (1 - \frac{3}{2}m^2)$ to 1; and by this force will the apses be moved, if no regard be had to the other force perpendicular to the radius of the orbit, and which through one-half of the satellite's revolution tends one way, and the contrary way through the other half. Now because, from what has been demonstrated in this and prop. 1, it follows that the gravity of the satellite about the planet, whose figure is an oblate spheroid, revolving generally at the distance l, is to the gravity of the same at the greater distance L, as $\frac{1}{l^2} + \frac{B}{l^4} \times (1 - \frac{3}{2}m^2)$ to $\frac{1}{L^2} + \frac{B}{L^4} \times (1 - \frac{3}{2}m^2)$, B being a given quantity of very small value, or as $\frac{1}{l^2}$ to $\frac{1}{L^2} - \frac{B}{l^2L^2}(1 - \frac{3}{2}m^2) + \frac{B}{L^4}(1 - \frac{3}{2}m^2)$ very nearly; therefore the gravity of the satellite is diminished more than in the duplicate ratio of the distance increased when m is less than $\sqrt{\frac{2}{3}}$, that is, when the inclination of the satellite's orbit to the planet's equator is less than 54° 44′; but is diminished in a less ratio when m is greater than $\sqrt{\frac{2}{3}}$, that is, when that inclination exceeds 54° 44′; and therefore in the former case the apses of the satellite's orbit proceeds forward, in the latter recedes. And the quantity of this progress or regress will be thus known.

By exam. 3, prop. 45, lib. 1, Newton's Princip. if to the centripetal force, which is as $\frac{1}{l^2}$, be added another force as $\frac{e}{l^4}$, that is, which may be to the centrifugal force $\frac{1}{l^2}$, as $\frac{e}{l^4}$ to 1, the angle of revolution from the one apsis to the same will be $360° \sqrt{\frac{1+e}{1-e}}$, or $\frac{360°}{1-e}$ very nearly, the quantity e being very small. Further, since the motion of the satellite revolving in its orbit, is to the motion of the apsis, as $\frac{360°}{1-e}$ to $\frac{360°}{1-e} - 360°$, that is, as 1 to e, the motion of the apsis, in the time of the satellite's revolution to a star, will be $= 360° \times e$; and this motion of the apsis will be to the motion of the same in any other given time, as the periodic time of the satellite is to that given time. But in our prop. $e = \frac{3bc}{5l^2}(1 - \frac{3}{2}m^2)$; hence is given the motion of the apsis sought. Q. E. I.

Corol. If this determination be referred to the moon, there will be $b = 1$, $l = 60$, $m =$ sine of 23° 28½′; and if $c = \frac{1}{229}$; then will $e = \frac{1}{1682203}$, and

the motion of the moon's apogee in 100 years will be $= 16'$ nearly in consequentia; if it be $c = \frac{1}{177}$, then will $e = \frac{1}{1393742}$, and the motion of the apogee $= 20'.7$. By this quantity then is the mean motion of the moon's apogee to be diminished as determined by observations, to obtain the motion generated by the sun's force.

Of the Variation of the Earth's Diurnal Motion.

If the figure of the earth were quite spherical whatever might be the axis of rotation, the same quantity of motion remaining in the globe, the velocity of rotation would remain the same; but it is otherwise when, by reason of the forces of the sun and moon, the earth takes the form of an oblong spheroid by the ascent of the waters. For here I do not consider the oblate figure of the earth from the redundant matter at the equator, but I suppose it spherical, unless so far as it is changed to spheroidical by the elevation and the depression of the waters. Now in a spheroid of this kind, though the quantity of motion remain the same, by the transverse axis changing its inclination to the axis of rotation, it is manifest that the velocity of rotation will be changed also; and since the transverse axis passes always through the sun or moon, it will every moment change its position in respect of the axis of rotation, because of the motion by which these two planets by turns recede from the equator and approach to it.

PROB. To Investigate the Variation of the Earth's Diurnal Motion arising from the aforesaid Cause.

Let the oblong spheroid ADCd (fig. 12) represent a fluid earth, whose centre is T, transverse axis AC joining the centres of the earth and sun or moon, Dd the less axis, EO the equatorial diameter, and XZ the axis of diurnal motion. With the centre T and radius TD describe the circle BDd cutting the transverse axis AC in B, and draw BK perpendicular to TE; then from any point P of the circle having drawn PM perpendicular to the axis XZ and cutting TA in H, let Ppr be the circumference of the circle which the point P will describe by its diurnal rotation, to any point p of which draw Tp, and produce it to meet the spheroidal surface at q; then demitting pG perpendicular on PM, and GF on TA, if through the points A, q, C be conceived to pass an ellipse similar and equal to the ellipse ADC, from the nature of the curve, and because our spheroid differs but very little from a sphere, it will be $pq = \text{AB} \times \frac{\text{TF}^2}{\text{TP}^2}$ very nearly. Now let u denote the velocity of a particle in the earth's equator revolving by the diurnal motion about the axis XZ at the distance of the semi-diameter TP, then will $\frac{u \times \text{PM}}{\text{TP}}$ be the velocity of the particle P describing the circle Ppr; and since $\text{TF} = \frac{\text{GM} - \text{HM}}{\text{TP}} \times \text{TK} + \text{TH}$, the motion of the whole lineola pq will be $= pq \times \frac{u \times \text{PM}}{\text{TP}} \times \frac{u \times \text{AB} \times \text{PM}}{\text{TP}^3} \times \frac{\text{GM} - \text{HM}}{\text{TP}} \times \text{TK}^2 + \text{TH}$; therefore the sum of these motions in

the circuit of the circle Ppr, that is, the motion of the superficies contained between the circle Ppr and the spheroid, in the direction Tp, will be equal to the circumference of this circle drawn into $\frac{u \times \mathrm{AB} \times \mathrm{PM}}{\mathrm{TP}^3} \times (\frac{\mathrm{TK}^2 \times \mathrm{PM}^2}{2\mathrm{TP}^2} + \frac{\mathrm{TK}^2 \times \mathrm{HM}^2}{\mathrm{TP}^2} - \frac{2\mathrm{TK} \times \mathrm{HM} \times \mathrm{TH}}{\mathrm{TP}} + \mathrm{TH}^2)$; or, because HM : TM :: TK : BK, and TH : HM :: TP : TK, by writing d for the circumference of the circle BDd, that motion will be $= \frac{u \times \mathrm{AB} \times d}{2\mathrm{TP}^6} \times (\mathrm{TK}^2 \times \mathrm{PM}^4 + 2\mathrm{BK}^2 \times \mathrm{TM}^2 \times \mathrm{PM}^2)$. Then the sum of these motions in the whole circuit of the globe collected, that is, the motion of the whole matter incumbent on the globe BDd, will be $= \frac{u \times \mathrm{AB} \times d^2}{32} \times \frac{3\mathrm{TP}^2 - \mathrm{BK}^2}{\mathrm{TP}^2}$. When the planet is in the plane of the equator, make BK $= 0$, and then the aforesaid motion is $= \frac{u \times 3\mathrm{AB} \times d^2}{32}$. But the motion of the globe QPR about the same axis, it is easily demonstrated, is $\frac{u \times \mathrm{TP} \times d^2}{16}$, therefore the motion of the whole earth is $\frac{u \times \mathrm{TP} \times d^2}{16} + \frac{u \times \mathrm{AB} \times d^2}{32} \times \frac{3\mathrm{TP}^2 - \mathrm{BK}^2}{\mathrm{TP}^2}$, which, since it must remain always the same, making v denote the velocity in the superficies of the terrestrial equator when the planet is in the plane of the equator, it will then be $\frac{v \times \mathrm{TP} \times d^2}{16} + \frac{v \times 3\mathrm{AB} \times d^2}{32} = \frac{u \times \mathrm{TP} \times d^2}{16} + \frac{u \times \mathrm{AB} \times d^2}{32} \times \frac{3\mathrm{TP}^2 - \mathrm{BK}^2}{\mathrm{TP}^2}$; hence, writing 1 for TP since it is the radius to the sine BK of the angle BTK, it will be $v : u :: \mathrm{TP} + \frac{3}{2}\mathrm{AB} - \frac{1}{2}\mathrm{AB} \times \mathrm{BK}^2 : \mathrm{TP} + \frac{3}{2}\mathrm{AB}$; and hence, because the altitude AB is extremely small in respect of the semidiameter TP, $u - v : v :: \mathrm{AB} \times \mathrm{BK}^2 : {}^2\mathrm{TP}$, and $u - v = v \times \frac{\mathrm{AB} \times \mathrm{BK}^2}{2\mathrm{TP}}$. But for v it is evident may be written the mean angular velocity of the earth, because differing from it by a very small quantity and drawn into the small quantity $\frac{\mathrm{AB} \times \mathrm{BK}^2}{2\mathrm{TP}}$, and because the times of the earth's revolutions about its centre are reciprocally as the angular motions u, v, then the difference of the earth's revolutions between when the planet is in the equator and when distant from it by the angle BTK, is $= 23^h\ 56^m \times \frac{\mathrm{AB} \times \mathrm{BK}^2}{2\mathrm{TP}}$. Because then the horary acceleration is to the mean horary motion of the earth about its centre, as $\mathrm{AB} \times \mathrm{BK}^2$ is to 2TP, or (because the sine p of the inclination of the ecliptic to the equator is to radius 1, as the sine BK is to the sine of the planet's distance from the equinox, which sine call k), as $\mathrm{AB} \times p^2 \times k^2$ to 2TP; therefore the horary acceleration of the earth's rotation increases in the duplicate ratio of the sine of the planet's distance from the equinoctial point; and the sum of all those accelerations, while the planet passes from the equinox to the solstice, is to the sum of as many mean horary motions, that is, the total acceleration generated in that time, is to that time, as the sum of all the quantities $\mathrm{AB} \times p^2 \times k^2$ in a quadrant of the circle, is to the sum of as many 2TP; that is, because the sum of all the k^2 in a quadrant of the circle is half the sum of as many squares

of the radius, as AB × p^2 to 4TP. Therefore, if P denote the 4th part of the planet's periodic time about the earth, the total acceleration of the earth's motion about its axis, generated in the passage of the planet from the equinox to the solstice, will be $= \frac{AB \times P \times p^2}{4TP}$; and the retardation will be the same in the planet's transit from the solstice to the equinox. Hence naturally arises this theorem: "The square of the diameter is to the square of the sine of ecliptic's obliquity, as the 4th part of the periodic time of the sun or moon, is to another time; then the earth's semidiameter is to the difference of the semiaxis, as the time just found, is to the acceleration sought.

The rise AB of the water due to the sun's force is about 2 feet, the earth's mean semidiameter TP being 19615800; hence the theorem produces, for the earth's acceleration turning about its centre, made while the sun passes from the equinox to the solstice, the quantity 1″ 55‴ in parts of time. And if by the moon's force the waters rise to the height of 8 feet, the acceleration of the earth's rotation hence arising, while the moon moves from the equator to her greatest declination, will be 34‴. And the sum of these accelerations, which obtains when these two planets occupy the solstitial points, since it exceeds not $2\frac{1}{2}$‴ of time, or 37‴ of a degree, will hardly be sensible. Q. E. I.

CXI. Observations on the History of the Norfolk Boy. By J. Wall, M.D. p. 836.

Dr. W. attributes the evacuation of worms in this boy's case to the oil in the mixture of paint which he swallowed; and mentions that he had afterwards prescribed oil as a vermifuge, with good success.

CXII. On the Corona Solis Marina Americana; The American Sea-Sun-Crown. From the French of J. A. Peyssonel, M. D., F. R. S.* p. 843.

CXIII. On several Rare Species of Barnacles. By J. Ellis, Esq. F.R.S. p. 845.

Some rare and extraordinary new species of barnacles, lately received from abroad, were so different from any of the common species, that Mr. E. was resolved to inquire into the nature of an animal, which, like a Proteus, appears in so many different shapes or coverings in different parts of the world. For this end he consulted that excellent collection in the British Museum, and some others in the cabinets of his friends. This marine animal is called by writers on natural history, balanus, and concha anatifera: but Linneus calls the internal active part, or fish, the animal triton, and the covering or testaceous habitation lepas, which he says is a multivalved shell, composed of unequal valves. The animal triton he describes, as having an oblong body, a mouth with a tongue in it, twisted

* This is some species of actinia, but for want of a figure, it is hardly possible to tell what particular species is intended; neither is the description clear, so that it was thought not worth reprinting.

about in a spiral manner; 16 tentacula or claws; 6 of the hinder ones on each side cheliferous. This account differing from that given by the ingenious Mr. Needham, in his Microscopical Essays, Mr. E. gives the character of this animal, as it appeared from many observations made on it, while alive in salt water; and these he compared not only with many dried specimens of other varieties, but likewise with some that were preserved in spirits; and he found that the parts of the animal agree in all the species.

The experiments that he made, were on the common English barnacle, viewed by a microscope, which is very frequently met with, in the winter, on oysters and other shell-fish. This animal has 24 claws, or cirrhi (fig. A, pl. 11) which are disposed in the following manner: the 12 longest stand erect, rising from the back part of the animal; they are all joined in pairs near the bottom, and inserted in one common base. These appear like so many yellow curled feathers: they are clear, horny, and articulated. Every joint is furnished with 2 rows of hairs on the concave side. The animal, in order to catch its prey, is continually extending and contracting these arched hairy claws, which serve it for a net. The 12 smallest claws are placed next to these, 6 on each side: these are divided into pairs; that is, 2 claws to one stem, like the chelæ or claws of the crab. These are more pliable, and fuller of hairs, than the others, and seem to do the office of hands for the animal. The whole number of claws lessen in size gradually each way, from the tallest in the back, to the last but one of each side in the front; which last 2 are of the middle size.

The proboscis or trunk rises from the middle of the base of the larger claws, and is longer than any of them: this the animal moves about in any direction with great agility: it is of a tubular figure, transparent, composed of rings lessening gradually to the extremity, where it is surrounded with a circle of small bristles, which likewise are moveable at the will of the animal. These, with other small hairs on the trunk, disappear when it dies. Along the inside of this transparent proboscis the spiral dark-coloured tongue appears very plain: this the animal contracts and extends at pleasure.

The mouth appears like that of a contracted purse, and is placed in front, between the fore claws. In the folds of this membranous substance are 6 or 8 horny laminæ or teeth standing erect, each having a tendon proper to direct its motion. Some of these teeth are serrated, others have tufts of sharp hairs instead of indentations on the convex side, that point down into the mouth; so that no animalcule that becomes their prey can escape back. Under the mouth lie the stomach, intestines, and the tendons by which they adhere to the shell. This then is the general character of the animal of the whole genus, whether with stems or without.

Mr. E. next gives a short description of the several kinds he had met with,

dividing them into 2 kinds; those that have stems, and those that adhere by their shelly bases. The first and most remarkable of those that have stems, is the barnacle:

Fig. 1. This differs from the lepas of Linneus in not having a testaceous, only a cartilaginous or fleshy covering. On the top of it are 2 erect tubular figures like ears: these have a communication with the internal parts of the animal (fig. 1. b). These inner parts agree with the general character already given. The stem, which is here dissected, was full of a soft spongy yellow substance, which appeared, when magnified, to consist of regular oval figures, connected together by many small fibres, and no doubt are the spawn of the animal. This extraordinary animal (of which there were 7 together) was found sticking to the whale barnacle (See fig. 1 and 7) by Mr. Smith of Stavenger in Norway, who cut both kinds together off a whale's lip, that was thrown upon that coast last year, 1757, and immediately immersed them in spirits of wine; by which means Mr. E. was able more exactly to describe them. Mr. E. called this animal the naked fleshy barnacle with ears; but it appears to claim, he thinks, the name of triton rather than lepas, according to Linneus, as having no shelly habitation.

Fig. 2 is the next animal of this class: this is not yet described. Mr. E. found several of them sticking to the warted Norway sea fan, sent here by Dr. Pontoppidan, the bishop of North Bergen: from its appearance, Mr. E. called it the Norway sea fan penknife. The stem of this is covered with little testaceous scales. The upper part of the animal is inclosed in 13 distinct shells, 6 on each side, besides the hinge-shell at the back, which is common to both sides: these are connected together by a membrane that lines the whole inside. One of these is magnified a little at fig. 2, a, the better to express the figure and situation of each shell.

Fig. 3 is taken from D'Argentville's Lithologie, pl. 30, fig. H, who says it is found in the British channel sticking to sea-plants; and that these shells consist of 5 pieces. This, from its appearance, Mr. E. called the British channel penknife, to distinguish it from the other.

Fig. 4 is a species of barnacle called poussepieds by the French, and described by Rondeletius as commonly found adhering to rocks on the coast of Brittany. He says the people there boil and eat the stem, which is first of a mouse colour, and afterwards becomes red like our prawns. There are many heads, that rise out of one stem, each consisting of 2 shells, in which are the same parts of the animal as in the other species. This Mr. E. called the cornucopia barnacle. Some of the shells of this barnacle were drawn from a specimen in the British Museum. This lepas is the mitella of Linn.

Fig. 5 and 6 are the barnacles called conchæ anatiferæ: these are the sorts so well known to sailors, and formerly supposed to produce a large species of duck called a barnacle. These consist of 5 shells. The tube, that supports one of these kinds, branches out like some species of corallines, bearing a shelled animal at the end of each branch. They are generally found adhering to pieces of wood in the sea, and most ships have some of them sticking to their bottoms. Those of the southern and warmer climates are generally of a larger kind than those of the colder and more northern.

The next division of these animals is, those that adhere by the base of their shells, having no stems. Here he observes, that the bottoms of the several species of this division conform in shape to the substances they adhere to, or grasp them in such a peculiar manner, as to render their situation secure from the violence of the element they live in. Another provision of nature for the security of these animals, are the 4 opercula, which, on their retreating into the great shell, they can draw to so close after them, as to secure themselves from outward danger.

Fig. 7, represents the whale barnacle, called pediculus ceti, just as it was cut off the whale's lip, with the naked barnacles with ears, already described. Fig. 7, a, is the bottom of the shell. This has the appearance of the gills of a mushroom. All the spaces between these laminæ were filled with the blubber of the whale: by this means they adhere to the gristly skin of the fish. The narrow

cavities between the branched laminæ are the places where the ligaments or tendons, that move the opercula, are inserted.

Fig. 8, is the cup barnacle, taken off an East India ship from Sumatra. The testaceous flat bottom of this was marked with the seams and lines of the sheathing, and with the rust of the nails. In one of these shells the animal is represented protruding his claws through the opercula.

Fig. 9, is called the bell-shaped barnacle. This was taken off the bottom of a ship from Jamaica, and had its flat testaceous base marked as the former.

Fig. 10, represents part of a most elegant specimen in the curious collection of Dr. John Fothergill. It is called the tulip barnacle, and very properly, as well from the shape of its shell as the beautiful stripes of red mixed with white. It adheres to a piece of the true red coral, and was fished up near Leghorn. It is not improbable, but that these groups of barnacles, growing at the same time with the animals that formed the red coral, may have received an addition to their fine red colour from the coral.

Fig. 11, is a group of barnacles of a conical form, composed of purplish tubes like small quills. Fig. 11, a, represents one of the same, with a view of its base, from the collection of Mr. Peter Collinson, F.R.S. This was brought from the East Indies. The insides of these shells have the appearance of the spongy parts of bones.

Fig. 12, is called the tortoise-wart barnacle, being often found upon that animal. This shell is of a plano-convex shape, and looks like polished ivory. The divisions between the valves represent a star with 6 points. If these shells are put into soap-lees, they will in a few hours separate into 6 pieces or valves, each shelly valve having 2 ears, like the scallop-shell: so that this species has its valves connected by membranes, instead of testaceous sutures, as most of the others have. Fig. 12, a, represents the under part of the same shell.

Fig. 13, is marked with 6 rays like a star, as the former: but is much deeper in proportion to its diameter. Several of this kind were found sticking to a crab, that was lately brought from the island of Nevis; whence Mr. E. called it the American crab's-wart.

Fig. 14, is called the side-mouth barnacle. This was found on the southern coast of Africa, near the Cape of Good Hope, where it adheres to a particular species of striated purple muscle. Fig. 14, a, represents 2 of the opercula of this barnacle remarkably horned. The shell of this is very thin; but its obliquity may probably be owing to its situation.

Fig. 15. This egg-shaped barnacle with a small mouth is found in clusters sticking to the buccinum tribe of shells in the West Indies.

Fig. 16, is the Cornish barnacle, shaped like a cone, and with a small mouth. This is described and figured by the Rev. Wm. Borlase, F.R.S. in his Natural History of Cornwall.

Fig. 17, is the common English barnacle, found in such plenty on all rocks and shells round this island. From the animal of this, examined in the microscope, Mr. E. took the character of the fish of the barnacle genus.

Fig. 18, Mr. E. called the limpet-shaped barnacle, from its likeness to some species of that shell. It was brought from Greenland, and with several more was found sticking to a very large species of muscle.

Fig. 19, a. This sea-fan, with the barnacles inclosed in it, was brought from Gibraltar. Mr. E. called it the slipper barnacle from its shape. See fig. 19. These shell-fish adhere, while they are young, to the slender branches, which are produced by the animals that compose this species of sea-fan; and as the next succession of young animals of this sea-fan creep up its sides, to increase the bulk and extension of these first-formed ramifications, they inclose the shells all round, leaving only their mouths or apertures open, for the barnacles to procure their food. But it frequently happens that the animals of the sea-fans destroy these barnacles, by overrunning and involving them in the very centre of their stems. These small barnacles, interspersed here and there on the branches, have

been taken for fruit or berries by some gentlemen, who consider the internal or horny part of the sea-fans as vegetables.

Fig. 20, is a very curious barnacle, taken from an elegant specimen in the British Museum; which from its figure Mr. E. called the Persian crown.

As to the nature of these animals: on opening the shells of many of the common English barnacles (fig. 1) while they were alive, Mr. E. found the lower part of the shell, which contained a cavity equal to two-thirds of the whole, full of spawn; so that the barnacles, which adhere by the base of their shells, as well as those that are supported by fleshy tubes, are propagated by eggs, which they send forth in inconceivable numbers; as appears by the clusters of young shells, which we find adhering not only to the parent animals, but to all hard substances near them. The bottom shell of these animals, as well as their upper shells, vary in form according to their situation, which occasions some difficulty in determining their several species with exactness. The form of the base shell of our common English barnacle, is the flat radiated figure represented adhering to a scallop shell in the front of a group of them at fig. 17. The barnacles at fig. 8, 9; 14, 15; and 20, have the same kind of base.

Mr. E. observed a singular kind of flat balanus, on a white mandrepora coral from the coast of Italy, in the possession of Mr. Mendez D'Acosta, F. R. S. whose base appears sunk into the coral, and of the form of an inverted cone, bending a little to one side. The inward surface of this conical base shell appears curiously striated with tubular radii, which terminate on the surface of the coral, to receive the extremities of the 6 valves, that compose the upper shell. This peculiar form of the base seems owing to the animals of the coral and of the barnacle growing up together, the latter keeping possession of its proper space, while the former grew close about it.

The bottom shell of the barnacle like a limpet, at fig. 18, increases from a small point by many thin shelly margins, which exactly correspond to the indentations which we observe on the base of the outer shell; so that it appears not unlike the drawing of a fortification in miniature. The Rev. Mr. Borlase is now of opinion, that the Cornish barnacle at fig. 16, which he has described in his History of Cornwall, is rather a limpet or patella.

CXIV. A Further Account of the Poisonous Effects of the Oenanthe Aquatica Succo viroso crocante of Lobel, or Hemlock Dropwort. By W. Watson, M. D., F. R. S. p. 856.

In June 1746, Dr. W. communicated to the R. S. some observations concerning the oenanthe aquatica succo viroso crocante of Lobel,* in respect to its

* *Oenanthe crocata*, Linn.

poisonous effects on some French prisoners at Pembroke. These observations were afterwards published in the Phil. Trans. * with an accurate representation of the plant itself, from an original drawing by that complete artist Mr. Ehret. That Dr. W. then thought the more necessary, as it was of no small importance to the public, to be well acquainted with a plant, the effects of which, when taken into our bodies, were so much to be dreaded. That account, as well as the representation of the plant, were re-published from the Transactions into the periodical works of that time; whence a more extensive knowledge of and acquaintance with this plant might have been hoped for. A late instance however had evinced, that these endeavours had not had their full effect, as the plant in question was not then sufficiently known and attended to.

John Midlane, a cabinet maker of Havant in Hampshire, aged about 58, and of a gross habit of body, was advised to make use of the water parsnip, as a remedy for a severe scorbutic disorder, which he had long been troubled with; and for which he had taken a variety of medicines. Instead of the water parsnip, which he purposed to take, there were gathered for him some roots of the oenanthe above mentioned; a large one of which was pounded in a mortar, and the juice squeezed through a linen cloth amounted to about 5 spoonfuls. This was suffered to stand all night, and the next morning (Mar. 31, 1758), at about ½ past 5, he drank the whole quantity, except the sediment.

In about an hour and half after he had taken this juice, he walked about the town on some business: and a little before 7, on his return home, about 100 yards from his own house, he first complained that he was ill; and having walked about 30 yards farther, was so bad as to go into a neighbour's house to rest himself. He was soon led to his own house by 2 men, and told them that he was affected as though he had lost the use of his limbs. When he was placed in his chair, he complained greatly of pain all over him; but particularly in his head. His stomach was immediately after affected, and he had great retchings to vomit. At the 2d attempt he threw up about ½ a pint of a clear watery liquor; at the first and third attempt he discharged scarcely any thing. He was then seized with a great propensity to go to stool, which went off in 3 minutes. After this he with the greatest difficulty was conducted up stairs to bed, where he pulled off part of his clothes himself. When he was put to bed, he was attacked with very severe convulsions, which in about a ¼ of an hour deprived him of his senses; and continued, with a few intermissions, till he died, a little before 9 o'clock; which was about 3 hours and half after the juice had been taken. A profuse sweat accompanied the whole of these symptoms: he foamed consider-

† See Phil. Trans. No. 480, p. 227. Vol. ix, p. 256, of these Abridgments.

ably at the mouth, and his belly swelled greatly. He purged very much soon after he was dead, but not before.

As this poor man had taken this dose before his family were up, no one could imagine from what his disorder arose; and consequently the apothecary, who was called to him, was able to form a judgment of his case only from the symptoms; as on his coming he found his patient senseless, and who had not, while his mind was undisturbed, told any one the probable cause of his complaints. He took from him however about 10 oz. of blood, and endeavoured to get some vinum ipecacuanhæ into his mouth: but his jaws were closed so fast, not above a spoonful passed, and that by the accident of his mouth opening of itself.

The symptoms, with which the person above-mentioned was attacked, were much the same as those which were observed in the French prisoners, who were poisoned by the same root at Pembroke. In both instances occurred those severe muscular spasms, which kept the under jaw so close to the upper, that, while the spasm continued, scarcely any force could separate them. In both instances likewise a considerable time passed before the persons, who had eaten of this root, though they had taken enough of it to destroy them, perceived themselves disordered by it.

CXV. Extract of a Letter to John Eaton Dodsworth, Esq. from Dr. George Forbes of Bermuda, relating to the Patella, or Limpet Fish, found there. p. 859.

As a curiosity for your esteemed friend Mr. Theobald, the captain will deliver you 2 fishes, quite singular here, and never before observed among us. The one is of the shell kind, and changed its figure so often, that it was difficult to make a drawing. However it was at length taken in two different positions, as in fig. 21, 22, pl. xi. The small one may be called the sea-batt; and in some sort resembles that species of animals when it is swimming.

Additional Remarks by Charles Morton, M. D., F. R. S.

The patella, or limpet-fish, whose generic characters, as enumerated by bishop Wilkins, are, that it is an exanguious testaceous animal, not turbinated; a univalve, or having but one shell; being unmoved; sticking fast to rocks or other things; the convexity of the shell somewhat resembles a short obtuse-angled cone, having no hole on the top.

CXVI. On the Cinnamon, Cassia, or Canella. By Taylor White, Esq. F. R. S. p. 860.

The cinnamon, cassia, or canella, are shrubs of no great height: they grow in Ceylon, Malabar, Java, Sumatra, and other places in the East Indies; as in the island of St. Thomas, and on the coast of Coromandel. They are described by Mr. Ray, in his History of Plants, vol. ii. f. 1559, under the title de Arbori-

bus Pruniferis. Linneus, in his Species Plantarum, places them under the title enneandria monogynia, by the name laurus. The leaf, flower, and fruit, of this plant, are particularly described by Mr. Ray.

The leaf is smooth and shining; has one large vein running through the midst, and a remarkable one on each side; the middle one generally running near the length of the leaf. The leaves differ in shape, some being more acute, others more oval or obtuse. The flowers grow in an umbel, somewhat like the laurus tinus; but they are small, consisting of one petal, of a tubular form at the bottom, and divided at the top into 6 segments in the form of a star. The flowers are succeeded by berries growing out of a capsula, like acorns in shape; which berries contain a shining seed. Mr. Ray's description of the flower, in his Description of the Cinnamon of Malabar, is extremely accurate; as is also the figure in the Hortus Malabaricus, N° 54, and the description, fol. 107, under the name carua. Mr. W. therefore refers to those.

The matter of the present inquiry is, whether the cinnamon of Ceylon is the same sort of plant with that growing in Malabar, Sumatra, &c. differing only by the soil or climate in which it grows, which is the opinion of Garcias; or from the culture or manner of curing the plant, as Mr. W. is inclined to believe; or whether it is really a different genus or species of plant, as many people believe, and some botanical writers seem to indicate. The distinction therefore, which these writers would make us believe there is between these plants, consists in the leaves of the one being oval, the other sharp-pointed; and that the nerves are limited at the bottom in the cinnamon, but not so in the cassia: for as to the semper florens, mentioned by Burman, that must undoubtedly be common to both.

Now as to the different shape of the leaves, we know how often this happens by seminal varieties, and from the age of plants, as in the leaves of holly and ivy; and that even the shapes of leaves vary greatly on the very same plant, and sometimes on the same branch; as in the ash, and many other plants, the leaves of the young shoots are more oval than those on the old boughs, which are generally more pointed. But this variety is much more frequent in the plants of warm countries. In the sassafras, part of the leaves generally near the bottom of the plant are plain, while the other leaves are divided into 3 lobes or segments. There are also great differences in the leaves of almost every one of the American oaks. In the Virginian cedar, the berries of the same plant produce some plants with juniper leaves, and others with leaves like the savin; and some plants with both leaves growing on the same plant.

Burman has, in his figures of 2 of these plants, made them extremely different. In that of Ceylon he has made all the leaves oval; and to make the difference greater, has drawn the rudiments of the berries; to which he has added

the flower, or part of it, at the top of the style or rudiment of the fruit: and in that of Malabar he has drawn the flower growing in the umbel. But his drawing of the cinnamon of Ceylon agrees with no one specimen in the British Museum; and scarcely is one leaf to be found of the shape which he gives.

The first figure, which Mr. W. produces, is a drawing, which he procured from Mr. Ehret in the year 1754: which was from a specimen given to Mr. Ehret by Mr. Empson in that year, of the cinnamon of Ceylon. See fig. 1, pl. 12. This agrees in every thing with the drawing of the cinnamon of Malabar in the Hort. Malab. fig. 54, fol. 107, and there called carua; except that it wants the fruit: but that defect is supplied by Mr. Ray's description of the cinnamon of Ceylon above mentioned. See fig. of the fruit, fig. 2.

In the figure in the Hort. Malabar. it may be observed, that the nerves do not go quite to the bottom of the leaf. But this is merely accidental, as will appear by the leaves of the same plant brought from Sumatra; in which, part of the leaves have veins going quite to the bottom, and united there, and the others not so. See fig. 3.

The next drawing Mr. W. produces, contains that of the leaves of the cinnamon plant, from specimens in the British Museum:

Fig. 4, A specimen, with the flower, from the collection of Mr. Courteen, who lived long in Ceylon. These leaves were more pointed, but were broken at the end.—Fig. 5, A whole leaf, with its point, in the same collection, growing on a branch, on which are the rudiments of the fruit.—Fig. 6, A leaf in Plukenet's specimens.—Fig. 7, Another leaf of the same collection, and of the same plant.—Fig. 8, A leaf of a large specimen from Boerhaave's collection.—Fig. 9, Another leaf on the same branch.—Fig. 10, A specimen from Petiver's collection. The ponits of the leaves are broken off.—Fig. 11, The flower of the first specimen.—Fig. 12, In the rudiment of the seed before formed in the state given in Burman's first drawing.

From all these specimens it plainly appears, that the distinction of foliis ovatis et lanceolatis does not appear well founded. But were it otherwise, and that the leaves of the plants differed, it would by no means be a proof of any material difference in the nature or quality of the plants; as is well known to persons conversant in natural history.

Having now given an account of the figure of these plants, and in what respect they are said herein to differ, Mr. W. proceeds to consider the pretended differences in the canella itself; which are supposed not to be in form only, but substantial and material; and are generally understood to be so by persons supposed to be acquainted with the subject. Mr. Ray states this matter fully in his Hist. Plant. vol. ii. p. 1560. From these reasons Mr. Ray draws a conclusion, that the cinnamon of Ceylon is cinnamon; and the cinnamon of Malabar, &c. is the cassia of the shops. From these specimens Mr. W. next produces, it will most plainly appear, he thinks, that these differences are merely accidents

arising from the age of the canella, the part of the tree from which it is gathered, and from the manner of cultivating and curing it.

In the Philosoph. Transact. N° 278, in Mr. Strachan's account of Ceylon, he says, that there are 2 sorts of cinnamon-trees, of which the tree which is esteemed the best has a leaf much larger and thicker than the other; but otherwise no difference is to be perceived. And in an account given some years ago to the Royal Society, 3 or 4 sorts were mentioned; and it was said the best sort was cut every 3 or 4 years.

This superiority Mr. W. then guessed to arise from the cutting the tree down every 3 or 4 years, which occasioned it to produce strong and vigorous shoots, thicker and larger leaves, as well as a greater quantity of bark, and of a superior quality. A large shoot or sucker of this plant was produced in the year 1750, or 51, by Dr. Watson, together with an account of the cinnamon-tree; which is published in the Philosop. Transact. vol. xlvii. p. 301. This shoot was a plain proof that the cinnamon was frequently cut down, and that this shoot arose from the root of a plant so cut; for it was of the size of a walking-cane; and no shrub could have produced such a shoot, unless a strong plant cut down.

The specimens which Mr. W. now produced, of the canella or bark of the cinnamon of Sumatra, he procured in the year 1755, from Mr. Tho. Combes, a gentleman then in the service of the East India Company in Sumatra, by means of a friend: and an abstract of Mr. Combes's letter on the occasion, is as follows:

I am of opinion, says Mr. C. that the true cinnamon grows no where but on the island of Ceylon, unless cassia be allowed to be the same tree, which I am inclined to think. N° 9 contains seeds of the cassia or wild cinnamon tree. As for the eeds of the true cinnamon-tree, I believe they are very difficult to be got; for as the Dutch are the sole masters of this spice, and get a good deal of money by it, they have very well guarded against the transplantation of it. I hope however that these seeds will not be unacceptable to you, as cassia itself is of some value; and as I am very doubtful whether this tree is not the same with the true cinnamon, being of opinion that the difference observed in them arises from the different method of curing their barks, or from the taking the bark from different parts of the tree, or at different seasons, or of different ages, or perhaps all these.

I have made inquiry concerning this from some very intelligent persons, and found them to be of opinion that the cassia and cinnamon tree were of the same genus. I have inquired further concerning the method of curing it at Ceylon; but as this is done by the natives, the Dutch are not very well acquainted with it; nor could I obtain any good account of it, different people giving me different relations. Some said it was the inner bark, some the middle, and some the outer; though of the young branches, they seemed in general to agree, that

it was gathered at a certain season of the year, and that one part of the cure was burying it in sand for some time. This may be tried with cassia, and may perhaps take away that viscosity or glutinous quality observed by chewing it, and which is the principal mark for distinguishing it from cinnamon. As to their chemical oils, I have heard many people say, that they are not distinguishable, otherwise than that from cinnamon is generally better, or, as it may be called, stronger than that from cassia; and accordingly bears a better price. But the Dutch company's chemist at Batavia, if I may give him this title, informed me that they are essentially different, and plainly distinguishable. But I must confess myself very doubtful of the knowledge or veracity of this chemist, and strongly suspect that they are no otherwise different than in goodness, as many other oils drawn from the same subject are.

In Persia, I think they make not so great a difference between them as elsewhere; and I myself, for want of cinnamon here for some months past, made use of the fine quilled cassia; and the difference I observe between them I imagine to arise rather from the greenness and want of dryness in the cassia, than any thing else, or perhaps from the method of curing it: for if there happens to be a little too much cassia put into my chocolate (and other things I use in it,) a little bitterish taste arises, something like what we meet with in most barks; though I do not remember to have observed this of cinnamon: but as to its boiling to a jelly, as Quincy mentions, I find no such thing, and think it bears boiling as well as cinnamon. Nor do I think its distilled water more subject to an empyreuma than that of cinnamon.

I have inquired of the country people here who bring it us, and they tell me the finest sort is the inner bark of the small branches: and indeed that it is the inner bark, I think, is evident in cinnamon as well as cassia; no outer bark of the youngest branches of any tree having, in my opinion, that smooth surface observable in both these barks.

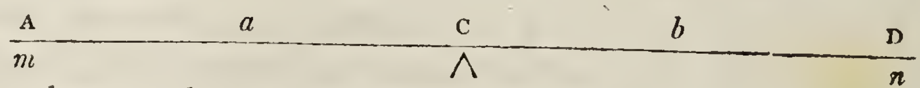
END OF THE FIFTIETH VOLUME OF THE ORIGINAL.

Art. I. On the Greatest Effect of Engines with Uniformly Accelerated Motions. By Francis Blake, Esq., F. R. S. p. 1. *Vol. LI. Anno* 1759.

The writers on the maximum of engines, or the greatest effect possible in any given time, have supposed the working parts of the machine to retain their direction, and be uniformly moved by the force of a current. They have therefore considered only the case of a uniform rotation, as in the action of grinding; where the impediments and impulses being brought to a balance, the impulses are only sufficient to prevent a decay in the generated motion. And, on that

view of the problem, the load of an engine, when the effect is a maximum, and the force a current, is determined by computation to be $\frac{4}{9}$* of the weight which would cause the engine to rest. This then being suited only to a uniform velocity both in the lever and obstacle, Mr. B. considers the case of a uniformly accelerated one in repeated vibrations. The maximum which corresponds to it is adapted to the steam-engine, and of no less importance to be determined than the other.

A general expression for the time of a stroke in such vibratory engines, will easily lead us to a computation of their effects.



Let AD be a lever, whose brachia are a and b, and supposed without weight. Let m be a power, and n a weight. Then $a : b :: n : \frac{bn}{a}$, the balance for n at A, and $m - \frac{bn}{a}$ is the effective force at A, which multiplied by the lever a gives $ma - nb$ for the efficaciousness of that force in the angular velocity of the power and weight. Now by the principles of mechanics, the inertia of any bodies revolving about a centre, is as the quantities of matter into the squares of the brachia; in the present case therefore, the whole inertia of m and n is as $ma^2 + nb^2$. Hence then, and because the velocity generated in a given particle of time is as the force directly and inertia inversely, we have $\frac{ma - nb}{ma^2 + nb^2}$ as the accelerating force or the measure of the angular velocity of the power and weight at the end of the said given particle of time. But again, the times of descent by means of uniform forces, through a given space, are inversely as the square roots of the accelerating forces, or measures of the velocities generated in a given particle of time; therefore $\sqrt{\frac{ma^2 + nb^2}{ma - nb}}$ is a general expression for the time of a stroke. This being had, the solution is easy; for, supposing n only to be variable, say as $\sqrt{\frac{ma^2 + nb^2}{ma - nb}} : n :: 1$, a constant or given time: $n\sqrt{\frac{ma - nb}{ma^2 + nb^2}}$ the effect in time 1, *ex* hypoth. the greatest effect which can possibly be produced in the said given time. Taking then, as usual, the fluxion equal 0, we have, after a proper reduction, $2a^3m^2 - 3a^2mnb + amnb^2 - 2n^2b^3 = 0$, and $n = \frac{am}{b}\sqrt{\frac{a}{b} + \left(\frac{3a - b}{4b}\right)^2} - \frac{am \times (3a - b)}{4b^2}$. Therefore, in these sorts of engines, when the brachia are given, the weight : power :: $\frac{a}{b}\sqrt{\frac{a}{b} + \left(\frac{3a-b}{4b}\right)^2} - \frac{a \times (3a - b)}{4b^2} : 1$; and if the brachia are equal, i. e. if $a = b$, the weight : power :: $\sqrt{\frac{5}{4}} - \frac{1}{2} : 1$, viz. 0.618 :

* The theory however is erroneous which brings out $\frac{4}{9}$ of the weight; it should be $\frac{1}{2}$ instead of $\frac{4}{9}$, as may be seen fully explained in vol. 3, p. 144, &c. of the Transactions of the American Philosophical Society; or in my Dictionary, v. 2, art. mill. C. H.

I nearly when the effect is a maximum. And so, in like manner, when b, m and n are given, and a is made variable, it is easy to see that, instead of the load, the best distance of the power from the fulcrum of the lever will be the result of the process; viz. $a : b :: n + \sqrt{n^2 + mn} : m$. But, this by the way.

In the proportion here determined, the power m is a weight, and therefore $ma - nb$, which is the generating force, being partly employed to overcome the inertia of the quantity of matter m, it is not wholly taken up in giving motion to the weight n; and the relative velocity is continually decreasing. But on the other hand, if m be the force of a spring, as is that of our atmosphere, or if n can be uniformly accelerated any how, in repeated vibrations, that there may be no sensible diminution of the relative velocity, the whole will be exerted on the weight to be raised; i. e. the tension of the rope or chain, by which the power is confined to act on the weight, will always be the same as though the beam were at rest; and then, by expunging ma^2 out of the expression for the greatest effect, $n\sqrt{\frac{ma-nb}{ma^2+nb^2}}$ becomes evidently enlarged to $n\sqrt{\frac{ma-nb}{nb^2}}$. The consequences are these. 1st, The greatest effect of this engine when m is a spring, will always exceed the contemporary effect where m is a weight. 2dly, The proportion of the power and weight will then be $n : m :: a : 2b$, as appears by taking the fluxion of $n\sqrt{\frac{ma-nb}{nb^2}} = 0$, and reducing the equation in the manner above. Whence the load to be raised for the greatest effect of a steam-engine, if the inertia of the materials composing its working parts be put out of the question, will be just half of what is sufficient to balance the atmosphere, whether the brachia of the lever be equal or not.

Mr. B. here adds 2 or 3 remarks on what he formerly laid down concerning the proportion of the cylinders. And, 1st, in all values of the brachia, with regard to their lengths, and all values of n, the expression $\sqrt{\frac{ma^2+nb^2}{ma-nb}}$ for the time of a stroke, when m is a weight, is the general expression to be used for the time. 2dly, m being considered as a spring, the time of a stroke is as $\sqrt{\frac{nb^2}{ma-nb}}$; and then if, according to what he there directed, a be taken variable, and m the reciprocal of a, the advantages to be gained by the breadth of the cylinder can only arise from a diminution of friction, and from the matter in the beam; for, the expression $\sqrt{\frac{nb^2}{ma-nb}}$ becomes constant, and thence the strokes are isochronal. I might, furthermore, says Mr. B. proceed to examine into these advantages, more explicitly than is there done, on the principles laid dawn, when m is a weight, But many particulars (such as the form of the brachia and various appendages, with their quantities of matter and centres of gyration) being wanting to perfect the theory of the construction, I shall drop the

inquiry when I have made only one remark more. It is this: the shortness of the brachia diminishes the resistance of the engine to motion: and therefore the inequality which I proposed in them, was in part to avail myself of that obvious advantage, without incurring the inconvenience of enlarging the pump-bores. I say it is an obvious advantage; for, the matter in the brachia, that the equilibrium may be preserved, being inversely as their lengths, and the resistance to motion in the direct ratio of the squares of those lengths, the resistance of the longer arm is to that of the shorter as the lengths of them directly.

II. Observations on the Growth of Trees. By Robert Marsham, of Stratton in Norfolk, Esq. p. 7.

Measures of Trees, taken in April 1743, before they began to shoot; and again in Autumn 1758, after the Year's Growth was completed. The Measure taken at 5 Feet from the Earth.

FIRST TABLE.

	Circumf. in Spring 1743.			Circumf. in Autumn 1758.			Increase in 16 years.			Content in 1743.			Content in 1758.			Solid Increase in 16 years.		
	Feet.	Inches.	8th of In.	Feet.	Inches.	8th of In.	Feet.	Inches.	8ths of In.	Cubic feet	Quarters.	Inches.	Cubic feet	Quarters.	Inches.	Cubic feet	Quarters.	Inches.
1. Ash, planted since 1647.........	9	10	4	11	1	0	1	2	4	60	1	318	76	3	43	16	1	157
2. Oak, past thriving, but sound....	9	4	4	10	1	0	0	8	4	54	1	336	63	2	80	9	0	176
3. Oak, about 80 years old........	6	3	3	7	8	3	1	5	0	24	1	284	36	2	408	12	1	124
4. Scotch Fir, seed in 1698........	5	4	6	6	6	0	1	1	2	17	3	48	26	1	270	8	2	222
5. Oak, planted above 60 years.....	5	11	1	7	2	3	1	3	2	21	3	224	32	0	174	10	0	382
6. Spanish Chestnut, near 60 years old	4	4	0	5	6	3	1	2	3	11	2	408	18	3	270	7	0	294
7. Another, 45 or 46 years old.....	2	9	6	4	4	4	1	6	6	4	2	391	11	2	408	7	0	17
8. Oak, planted in 1720..........	2	11	2	5	1	2	2	2	0	5	1	116	16	0	260	10	3	144
9. Scotch Fir, planted 1734, 2 feet high	1	11	6	4	0	0	2	0	2	2	2	0	10	0	0	7	2	0
10. Pinaster, planted in 1734 or 1735	2	5	1	4	3	1	1	10	0	3	2	260	11	1	68	7	2	240
11. Oak, set an acorn in spring 1719	1	7	0	2	8	2	1	1	2	1	2	116	4	1	336	2	3	220
12. Oak, planted in 1720 or 1721...	2	9	5	4	9	4	1	11	7	4	2	391	14	0	176	9	1	217
										213	0	300	322	0	333	109	0	33

Now as the 12 trees above, contained 213 cubic feet 300 inches of timber in spring 1743, and have increased to 322 cubic feet 333 inches in autumn 1758; that is, 109 cubic feet 33 inches in 16 years growth; if all the trees were of the same kind, 109 feet pays 3 per cent. for standing: and the 6 oaks pay near the same interest, though one of them, N° 2, appeared past thriving in 1743; for the increase of the 6 oaks is from 112 feet 1 quarter 171 inches of timber, to 167 feet 138 inches, i. e. 54 feet 2 quarters 399 inches; which is above 3 per cent. But if we take only the 5 thriving oaks, then their content is, from 57 feet 3 quarters 267 inches, to 103 feet 2 quarters 58 inches; i. e. 45 feet 2 quarters 223 inches of timber; or near 5 per cent. And the increase of the most thriving oak, N° 8, appears, by the above table, to pay above 12½ per cent. and the Scotch fir, N° 9, being under 2½ feet of timber in spring 1743,

and 10 feet in autumn 1758, pays above $18\frac{3}{4}$ per cent. Besides it should be considered, though he measured the largest and most thriving oak and Scotch fir in 1743, yet several others of the same age, both oaks ahd Scotch firs, have greatly exceeded the measured trees for many years past; e. g. the oak N° 11, appears by the table 2 feet 8 inches 2-8ths in circumference; and another just by it is 2 feet 11 inches 6-8ths; and an oak transplanted from this grove, is 3 feet 9 inches 5-8ths round; yet this last tree was considerably less than the first when removed, and not planted in a better soil, and yet is 1 foot 1 inch 3-8ths larger than the original tree. The first contained 4 feet 1 quarter 336 inches, and gained 2 feet 3 quarters 220 inches in 16 years: the last contains 8 feet 3 quarters 68 inches; and, supposing them equal in 1743, gained 7 feet 384 inches; i. e. above $2\frac{1}{2}$ the increase of the first tree. But notwithstanding the transplanted oak was thus much larger than the original oaks in the grove, yet as the transplanted tree does not run half the height of the trees in the grove before it heads, they differ but little in their quantity of timber.

The great Lord Bacon says, "the improvement of the ground is the most natural way of obtaining riches." What great fortunes might be raised, by those that have property, in the vast heaths and downs, or fields of poor land, in this kingdom, by planting parts of them? which would also add great beauty to the country, and render the dwelling much more comfortable to the neighbourhood, by the shade in summer, and warmth in winter. Some parts of these great wastes would produce good oak; and where the soil is moist, poplar, alder, and other aquatics, would be very profitable to the planter. The chalky soil seems the least promising; yet beeches sometimes thrive well upon it. The fir kind, especially the Scotch fir, will grow surprizingly on poor sandy land; but woods of fir should be guarded with an out-line of birch and beech, to break the force of strong winds. Birch, being the quickest grower, will best protect the young fir; but as birch, after a few years, is easily blown down, so beech will be wanted to defend the firs as they become large: for I have seen broad glades made by the wind through great woods of fir in Switzerland: which, perhaps, might have been prevented, at least in part, by an out-line of beech.

I know some think, that poor land cannot produce large trees; yet the oak at Northall in Hertfordshire, whose beautiful head spreads a circle of above 40 yards diameter, stands on a dry and deep sand; and the fine chestnuts and beeches by Mr. Naylor's grand castle of Herst Monceux in Sussex, grow in a light sandy soil: and I have found, by experience, the Weymouth, Scotch, spruce, and silver firs, which I planted in a poor sandy soil, are larger and finer trees, than others set at the same time in much better land. Perhaps it may require a rich clay to produce such trees as the noble grove of oak in the Earl of

Powis's park by Ludlow, or Lord Ducie's vast chestnut at Tortworth, in Gloucestershire, which I measured 46½ feet in circumference at near 6 feet from the ground.

III. Of some Antiquities found in Cornwall. By the Rev. William Borlase, M. A., F. R. S. p. 13.

In the year 1756 a farmer at Bossens, in the parish of St. Erth, driving his oxen from the field, perceived the foot of one of them to sink a little deeper than ordinary into the earth at A, fig. 1, pl. 13. Curiosity, and the hopes of treasure, led him soon after to search the place; where was soon discovered a perpendicular pit, circular of 2½ feet diameter. Digging to the depth of 18 feet, there was found a Roman patera fig. 1 and 2: about 6 feet deeper, the jug, fig. 3: near by, among the rubbish, the stone, fig. 4; a small millstone, about 18 inches diameter: then another patera, with two handles, in other particulars of the shape and size, as fig. 2, but unfortunately mislaid, and not now to be found. Intermixed with these were found fragments of horns, bones of several sizes, half-burnt sticks, and many pieces of leather, seemingly shreds of worn-out shoes. Having sunk to the depth of 36 feet, they found the bottom of the pit concave, like that of a dish or bowl. There was a sensible moisture, and mostly wet clay, in all parts of the pit. On each side there were holes at due distances, capable of admitting a human foot, by which persons might descend and ascend. There is no doubt but this work must have been intended for a well: but a pit so deep, and of such narrow dimensions, must have been sunk through a stony ground with much difficulty, and with tools very different from those now in use.

Going to the spot on the 22d of May, Mr. B. found, on the higher part of the tenement, in a field called the Rounds, the remains of a fort: the length of it, bearing nearly north and south, was 152 feet; the breadth, from east to west, about 136 feet. The foss on the outside was still discoverable; the walls dismantled, but sufficient remains to show, that the work was rectilinear, with the angles rounded off; a manner of fortifying which the Romans were generally fond of, as may be seen by their stations per lineam valli (Horsley Britannia Romana, p. 113, and many other places.) At the north corner, B, there was an additional building, projecting outwards beyond the rampart, about 30 feet long, not quite so wide: at the south angle at D there are the signs of a building of like kind: these were the procestria of the fort. The shape and size of the work, as it stands at present, may be seen in the drawings annexed, fig. 8.

On examining the rubbish near the pit, he found the cut stone, fig. 5, part of a large stone vase, and part of an earthen sepulchral urn: he found also some fragments of leather: a l which, with what was found before, are briefly described below.

1

Fig. 1 and 2 are two views of the patera: it was made of tin, the 20th of an inch thick, $4\frac{1}{8}$ inches wide at the brim, but narrowing downwards, was at the bottom, which was flat, $2\frac{1}{2}$ in diameter. The bottom of the inside is represented, fig. 1. Fig. 2 is the side of the same patera, by the scale annexed. It is very rare to find these seemingly trifling cups and dishes inscribed to a particular deity; but most uncommon to see them distinguished by the names of the donor and his father, as well as the name of the deity to which they were dedicated. This patera, found at Bossens, about 3 miles north-east of St. Michael's Mount, is a singular instance of the latter usage, and has an inscription engraved on its bottom, in a circular line, as in fig. 1. Which Mr. B. reads thus: Livius Modestus Driuli (or Deuiuli) f. (for filius) Deo Marti. The first 2 words are very plain (though, like the whole, a mixture of Greek and Roman characters,) and not rare in Roman history.

Fig. 3 is a jug or jar of tin also, containing 4 quarts 1 pint and $\frac{3}{5}$ of a quartern, wine measure: its weight 7 lb. $9\frac{1}{2}$ oz. It is the præfericulum of antiquarians, a vessel used to bring the holy water, or other sacred liquor, to the altar. Fig. 4 and 5 are of stone. The first and largest weighs 14 lb. 1 oz. avoirdupois, and 11 dwts. amounting to about 18 lb. Roman and 337 grains. The second and smaller stone weighs 4 lb. 1 oz. and 7 dwts. or $5\frac{1}{4}$ lb. Roman and 95 grains. By the holes these stones have near the top, they were probably designed as weights, by which provisions were bought for, and afterwards shared among the soldiers of the fort. Fig. 6 is part of a vase or bowl, sometimes made of brass, or richer metal, but here of stone. This vase was of curious grey granite, formed by turning, well polished within, somewhat discoloured without, as if it had suffered by fire.

The small millstone, by the smoothness of one side, shows that it had been much used; and was such, without any material difference, as is now used in the islands of Scilly (and elsewhere) for handmills to grind corn in times of siege and confinement, and must be absolutely necessary in all forts. The bones and horns may be supposed to have belonged to animals, either sacrificed, or killed for the sustenance of the garrison: the ashes and half-burnt sticks, are the remains of sacred or culinary fires. The fragments of leather are for the most part patched, and coarsely sewn together; but one piece, found more entire, may contribute perhaps to show the shape of the Roman calceus of those times; and may be seen fig. 7, by the same scale with the rest. Some bits of leather were also pierced with circular holes; but whether parts of the calceus, cothurnus, or any border for the habit, armour, or vehicle of the officers, enough does not remain to decide.

I shall make no other reflection at present on these antiquities, says Mr. B., than that the inscription is the first discovered in this county of such high

antiquity; and will satisfy the learned, that the Romans had penetrated into the westernmost parts of Cornwall before the empire became christian: that the sacrifical vessels, the pateræ, and præfericulum, are of tin, the natural product of Cornwall: the vase, the weights, the millstone, are also of Cornish granite: and by the walls, the religious utensils, the weights, the quantity of shoes, bones, horns, vases, urn, and ashes, this fort appears to have been that of a fixed garrison, not a temporary occasional fortification: that by the shape of this fort, and the antiquities discovered in it, it was a Roman fort.

IV. A New Improved Silk-Reel. By the Rev. Samuel Pullein, M. A. p. 21.

The improvements made in the reeling, and other operations relating to silk as well as cotton, have been so numerous and much more important since the date of this paper, that it was deemed quite unnecessary to reprint it in these Abridgments.

V. Experiments on several Pieces of Marble Stained by Mr. Robert Chambers. In a Letter from Mr. E. M. Da Costa, F. R. S. p. 30.

But before relating the experiments, it may not be improper to give some little historical account of the art itself. Kircher, in his Mundus Subterraneus, lib. viii, sect. 1, c. 9, p. 45 and 46, is the first author that mentions it. There was, says he, an artist at Rome, who painted several pieces of marble, in an elegant manner for Pope Urban VIII. He would not discover his art; therefore Kircher strove by many experiments to discover it: and he made colours, viz. tinctures of metals and minerals, which coloured the marble as finely as any the artist had done, and quite penetrated the stone; insomuch that a slab cut horizontally made as many pictures as pieces or sections. Kircher gives at large the process he used for making the colours; and observes they should always be of a mineral origin. The said author (Ibid.) also gives another method to colour marble, by vitriol, bitumen, &c. forming a design of what you like upon paper, and laying the design between 2 pieces of polished marble; then closing all the interstices with wax, you bury them for a month or 2 in a damp place. On taking them up, you will find that the design you painted on the paper has penetrated the marbles, and formed exactly the same design on them. A modern author, Wallerius, in his Mineralogy, vol. ii. gen. 58. p. 128. also recommends this method.

In the Phil. Trans. N° 7, Kircher's first method is copied. The editor however says, that method has not since been tried. He adds, that one Mr. Bird had for many years (he writes in 1666) found out a way to sink colours a considerable depth into polished marble; pieces of which were shown to King Charles 2d, soon after his restoration; and, being broken in his presence, it

was found that the colours had penetrated deep into the marbles; and that many works of his coloured marbles were seen at Oxford and London. But Mr. Bird's way of doing it is not mentioned.

In the Philos. Trans. N° 268, is a paper, intitled, "The Way of Colouring Marble." The anonymous author gives an account of the colours, &c. he used. It is observable that they are only vegetable colours. His red, he says, he extracted again from the marble, without hurting the polish, within 26 hours, with oil of tartar per deliquium; and his brown was quite discharged by aquafortis within one quarter of an hour, and the polish of the marble quite destroyed.

Mr. da Costa now proceeds to give an account of the experiments he made. He could not well suggest any more, as the method of colouring the marble, the materials of the colours, &c. are kept secret by the artist, Mr. Chambers.

A piece of marble with the several colours used, on it, like a painter's pallet, being greatly saturated with aquafortis, at different times, for 24 hours, though the polish of the marble was quite effaced, yet there was not the least discharge of any of the colours, nor were they any-wise dulled, &c.—N° 6. A deep crimson-red colour, being left 20 hours in a strong lye of common soft green soap, suffered no change; and boiled in the same lye half an hour, also suffered no change. The marble finely powdered, and aquafortis effused over it, the marble particles were nigh destroyed; but several red particles (no doubt the colour) remained. The marble, by common calcination, i. e. in a common coal fire, for half an hour, is entirely discharged of its colour. We made the experiments on four other reds, and the result was much the same as abovesaid; so that this is a standard for his reds.

N° 5. A deep sea-green, being left 20 hours in a strong lye of common soft green soap, suffered no change; but boiled in the same lye it quite discharged its green colour: however, it yet remained slightly tinctured yellowish. By common calcination the colour was quite discharged. Some other greens were tried, and answered much the same.—N° 10, 15, and 16, brownish or terrestrial yellowish colours, near to a clay colour, boiled in a strong lye of common soft green soap, they suffered no change. By common calcination the colours were discharged, but retained a greyish cast. These colours, covered for 48 hours with a layer of the said common soap, suffered no sensible change.—N° 19, A bright yellow, boiled in a strong lye of common green soft soap, suffered no change; and covered with a layer of the same soap for 48 hours, the colour is dulled. By common calcination the colours are discharged, but retain a greyish cast. Several other different shades of yellow answered much the same. Mr. Chambers has not as yet stained any marble of a blue colour.

By the above experiments we may conclude, that these colours are good, penetrate the marble freely without injuring it, remain uninjured by menstrua,

&c.; and that only calcination discharges them. Therefore it is probable that Mr. Chambers's method of staining or colouring marbles is extremely good.

Though acid menstrua work greatly on marble, yet it is observable that these colours are not discharged by them, but only by calcination; which, as it entirely and thoroughly destroys the compages of the stone, the substances of the colours must undoubtedly at the same time be exhaled by the force of the fire. We observe a like process in the works of nature; viz. in the dendritæ, or such as are on alkaline stones: for though the stones are utterly corroded by the acids, yet the dendritæ, however merely superficial, remain; but if calcined, the said dendritæ are immediately exhaled, and entirely disappear.

This art will not only give pleasure to the eye by regular paintings (whereas the natural colourings of marble are very irregular) but it may be very useful to blazon arms, and for inscriptions; as sculpture alone can never express colours, and chisseled inscriptions, &c. suffer much by age; for probably a monument of marble, rightly coloured by this method, will be preserved many ages from the injuries of the weather, though at the same time the stone itself will be somewhat hurt or corroded by the air.

VI. Observations on the Sea Scolopendre, or Sea Millepes. By John Andrew Peyssonel, M. D., F. R. S. Translated from the French.* p. 35.

This creature, in its figure, is like the land scolopendre, or, as Pliny says, to the hairy caterpillar, commonly called the milleped animal. It is of the same colour, has the same arrangement of circular rings; but whereas the land scolopendre is flat, this is square. Dr. P. counted 80 rings, which form the body and head, when brought to him. This sea insect was very small, and almost imperceptible. He was surprised, after having kept it some time, to see a round body, of a blackish green colour, like the glans virilis, pass out of it, which had a considerable opening, like the canal of the urethra. This gland was surrounded by two bodies or bowels, which appeared in form of a prepuce turned back; the one was yellowish, and the other whitish; each but a line thick, stronger and larger above, and terminating below like a ligature, filled with a matter like that contained in the intestines of fishes and insects. This may be what gave occasion to Pliny, and other naturalists, to think, that these insects, finding themselves taken, throw out their bowels in order to lessen their bulk.

The body of the animal is square; and the 4 sides are armed with such prickles as he never saw before. Also every ring has 4 bundles of prickles. The following is the manner of their being disposed: at the end of each ring, above the square, on each side we see a gland, near which a bundle of prickles arises

* The animal here described seems to belong to the genus *terebella.*

towards the squares below. This bundle seems to fill the whole ring, and to part as it were from the centre of the under side; where is a hollow or separation, which passes in a right line from the head to the tail. The bundles of prickles appear round at first, and terminate in points; but afterwards they are seen to spread out like a fan; so that their extremities are more than 4 times the breadth of their bases. They contain an infinite number of prickles, which are extremely fine, loose, and brilliant, like an aigrette of glass, but more free and loose. The range of prickles underneath spreads also like fans, serving the insect as feet; for it is on these he stands, and moves on them as the scolopendre does on his feet. Having put these sea scolopendres on his fingers, they thrust a great number of their prickles into the skin, and caused a sharp pain for some hours: it was like fire on the part. It was in vain that he rubbed and washed the part; and though the prickles were broken, yet the parts that stuck in the flesh produced their effect, and caused the pain he felt for some hours. Afterwards it all went off without any further ill consequence.

VII. On a Storm of Thunder and Lightning at Norwich, July 13, 1758. *By Mr. Samuel Cooper.** p. 38.

About 4 o'clock on Thursday afternoon, July 13, 1758, a short but severe thunder storm, with lightning, fell on the top of a house standing alone, and belonging to a common garden, on the causeway near Sandling's ferry, in the city of Norwich; it struck off the tiles of the roof at the east end, to the space of a yard or two; burnt a very small hole in the middle of a lath, in piercing into the chamber, and then darted to the north-east; ripped off the top of an old chair, without throwing it down; snapped the two heads of the bed-posts, rent the curtains, drove against the wall (the front of the house stands due north-east) forced out an upright of a window frame a yard long, 3 inches broad, and 2 thick; smote it in a right line into an opposite ditch, 10 or 12 yards distant: then struck down on the wall of the chamber, paring off half a foot's breadth of its plastered covering quite down to the floor; lifted up a board of the floor, and leaving a hole of half an inch diameter, pierced through by the side of the main beam into the kitchen, towards the west end of a pewter shelf; traversed the whole shelf to the east, and melted superficially, to the breadth of a shilling, 6 pewter dishes, 2 plates, and a pewter basin, all standing touching each other: 2 of the dishes were thrown down, the rest not displaced. Under this, a narrower shelf of pewter plates untouched. In its descent to the floor, knocked down, as she expressed it, an ancient woman sitting in the passage westward of

* This account is confirmed in almost every circumstance by another communicated to the Royal Society in a letter from Mr. William Arderon, F. R. S. to Mr. Henry Baker, F. R. S.—Ori

the shelves; who being presently taken up, her shoulders and back were found to be scorched all over, with the hind part of her left leg; the skin almost universally red and inflamed, rimpled in two or three places, but not broken: her shift burnt brown, stocking singed, with its colour of the inside discharged, and the outside unchanged: right foot very painful and bruised, with that shoe struck off, and its upper leather torn: her gown and other clothes without any damage. It passed through the same passage without injuring another old woman sitting knee to knee with her companion; but keeping its direction to the north-east, turned on a right angle on the outer door, split it, and passed through into the open air. On a right line with this passage to the west, and under the same roof, is the wash-house, where stood the master and his man. They saw the woman tumble down, and heard such a violent explosion, that made them both think the whole house must come down: and the man says, with such a blaze, as if all was on fire, but that was but for a moment. To the east of the pewter shelves, and under that part of the roof where it entered, it rushed into the kitchen closet, by tearing off a wooden button, that was nailed on, and there took some pieces from a Delft dish without throwing it down, broke a quart mug, and from a 4-ounce phial half full of oil cut off its empty half part without spilling a drop of the oil. The activity of the lightning was with abated violence to all other points of the compass; but not without some considerable degree of force; for it scraped the plaster off the wall in many different and distant places, both in the chamber and kitchen, and to the south-west of the chamber, where was the window, broke many panes of glass, and tore the lead outwardly, without melting it; and broke two panes of the kitchen window, with its lead, situated under the chamber window. Both kitchen and chamber smelt as strong of sulphur some hours after, as if fumigated with brimstone matches.

VIII. Experiments concerning the Encaustic Painting of the Ancients. By Mr. Josiah Colebrooke, F.R.S. p. 40.

The art of painting with burnt wax, as it is called, has long been lost to the world; the use of it to painters, in the infancy of the art of painting, was of the utmost consequence, drying oil being unknown, they had nothing to preserve their colours entire from the injury of damps, and the heat of the sun; a varnish of some sort was therefore necessary; but, being unacquainted with distilled spirits, they could not, as we now do, dissolve gums to make a transparent coat for their pictures; this invention therefore of burnt wax supplied that defect to them, and with this manner of painting, the chambers and other rooms in their houses were furnished; this Pliny calls encaustum, and we encaustic painting.

The following experiments were occasioned by the extract of a letter from the

Abbé Mazeas, translated by Dr. Parsons, and published in the 49th volume of the Philos. Trans. concerning the ancient method of painting with burnt wax, revived by Count Caylus. The Count's method was, First, To rub the cloth or board designed for the picture simply over with bees-wax. Secondly, To lay on the colours mixed with common water; but as the colours will not adhere to the wax, the whole picture was first rubbed over with Spanish whitening,* and then the colours are used. Thirdly, When the picture is dry, it is put near the fire, by which the wax melts, and absorbs all the colours.

Exper. 1.—A piece of oak board was rubbed over with bees-wax, first against the grain of the wood, and then with the grain, to fill up all the pores that remained after it had been planed, and afterwards was rubbed over with as much dry Spanish white, as could be made to stick on it, this, on being painted (the colours mixed with water only) so clogged the pencil, and mixed so unequally with the ground, that it was impossible to make even an outline, but what was so much thicker in one part than another, that it would not bear so much as the name of painting; neither had it any appearance of a picture; however, to pursue the experiment, this was put at a distance from the fire, on the hearth; and the wax melted by slow degrees; but the Spanish white (though laid as smooth as so soft a body would admit, before the colour was laid on) yet on melting the wax into it, was not sufficient to hide the grain of the wood, nor show the colours by a proper whiteness of the ground, the wax in rubbing on the board, was unavoidably thicker in some parts than others, and the Spanish white the same: on this he suspected there must be some mistake in the Spanish white, and made the inquiry mentioned in the note below.

To obviate the inequality of the ground in the first experiment.

Exper. 2.—A piece of old wainscoat (oak board) $\frac{1}{4}$ of an inch thick, which having been part of an old drawer, was not likely to shrink on being brought near the fire; this was smoothed with a fish-skin, made quite warm before the fire, and then with a brush dipped in white wax, melted in an earthen pipkin smeared all over, and applied to the fire again, that the wax might be equally thick on all parts of the board, a ground was laid on the waxed board with levigated chalk mixed with gum water, viz. gum Arabic dissolved in water; when dry, he painted it with a kind of landscape, and pursuing the method laid down by Count Caylus, brought it gradually to the fire by slow degrees, till it came within 1 foot of the fire, which made the wax swell and bloat up the picture;

* Spanish chalk is called by Dr. Parsons, in a note, Spanish white; this is a better kind of whitening than the common, and was the only white that had the name of Spanish annexed to it, that he could procure, though he inquired for it at most if not all the colour shops in town. Mr. Dacosta showed him a piece of Spanish chalk in his collection, which seemed more like a cimolia (tobacco pipe clay) and was the reason of his using that in one of the experiments.—Orig.

but as the chalk did not absorb the wax, the picture fell from the board and left it quite bare.

Exper. 3.—He mixed 3 parts white wax, and one part white resin, hoping the tenacity of the resin might preserve the picture. This was laid on a board heated with a brush, as in the former; and the ground was chalk, prepared as before. This was placed horizontally on an ironing box, charged with a hot heater, shifting it from time to time, that the wax and resin might penetrate the chalk; and hoping from this position that the ground bloated by melting the wax, would subside into its proper place: but this, like the other, came from the board, and would not at all adhere.

Exper. 4.—Prepared chalk 4 drams, white wax, white resin, of each a dram, burnt alabaster half a dram, were all powdered together and sifted, mixed with spirit of molosses instead of water, and put for a ground on a board smeared with wax and resin, as in Exp. 3. This was also placed horizontally on a box-iron, as the former: the picture blistered and was cracked all over; and though removed from the box-iron to an oven moderately heated, in the same horizontal position, it would not subside, nor become smooth. When cold, he took an iron spatula made warm, and moved it gently over the surface of the picture, as if to spread a plaster. This succeeded so well, as to reduce the surface to a tolerable degree of smoothness: but as the ground was broken off in many places, he repaired it with flake white, mixed up with the yolk of an egg and milk, and repainted it with molosses spirit, instead of water; and then put it into an oven with a moderate degree of heat. In this he found the colours fixed, but darker than when it was first painted; and it would bear being washed with water, not rubbed with a wet cloth.

Exper. 5.—A board that had been used in a former experiment, was smeared with wax and resin, of each equal parts; was wetted with molosses spirit, to make whitening (or Spanish white) mixed with gum-water adhere. This, when dry, was scraped with a knife, to make it equally thick in all places. It was put into a warm oven, to make the varnish incorporate partly with the whitening before it was painted; and it had only a small degree of heat: water only was used to mix the colours. This was again put into an oven with a greater degree of heat; but it flaked off from the board.

Exper. 6.—Having miscarried in these trials, he took a new board, planed smooth, but not polished, either with a fish-skin or rushes: he warmed it, and smeared it with wax only; then took cimolia (tobacco-pipe clay) divested of its sand, by being dissolved in water and poured off, leaving the coarse heavy parts behind. After this was dried and powdered, he mixed it with a small quantity of the yolk of an egg and cow's milk, and made a ground with this on the waxed board. When the ground was near dry, he smoothed it with a pallet-

knife, and washed with milk and egg where he had occasion to make it smooth and even: when dry he painted it, mixing the colours with common water; this on being placed horizontally in an oven, only warm enough to melt the wax, flaked from the board; but held so much better together than any of the former, that he pasted part of it on paper.

Exper. 7.—Flake white mixed with egg and milk, crumbled all to pieces in the oven when put on the waxed board, as in the last experiment. The bad success which had attended all the former experiments, led him to consider of what use the wax was in this kind of painting; and it occurred that it was only as a varnish to preserve the colours from fading. In order to try this,

Exper. 8.—He took what the bricklayers call fine stuff, or putty; to this he added a small quantity of burnt alabaster to make it dry: this it soon did in the open air; but before he put on any colours, he dried it gently by the fire, lest the colours should run. When it was painted, he warmed it gradually by the fire, to prevent the ground from cracking, till it was very hot. He then took white wax 3 parts, white resin one part, melted them in an earthen pipkin, and with a brush spread them all over the painted board, and kept it close to the fire in a perpendicular situation, that what wax and resin the plaster would not absorb might drop off. When cold, he found the colours were not altered, either from the heat of the fire or passing the brush over them. He then rubbed it with a soft linen cloth, and thus procured a kind of gloss, which he afterwards increased by rubbing it with a hard brush; which was so far from scratching or leaving any marks on the picture, that it became more smooth and polished by it.

After he had made all the foregoing experiments, in conversation with Dr. Kidby, who told him that there was a passage in Vitruvius de Architectura relative to that kind of painting; and which, when translated, runs thus: "But if any one is more wary, and would have the polishing [painting] with vermilion hold its colour, when the wall is painted and dry, let him take Carthaginian [Barbary] wax, melted with a little oil, and rub it on the wall with a hair pencil; and afterwards let him put live coals into an iron vessel (chafing dish,) and hold it close to the wax, when the wall, by being heated, begins to sweat; then let it be made smooth: afterwards let him rub it with a candle and clean linen rags, in the same manner as they do the naked marble statues. This the Greeks call καυσις. The coat of Carthaginian wax, thus put on is so strong, that it neither suffers the moon by night, nor the sun-beams by day to destroy the colour." Being satisfied, from this passage in Vitruvius, that the manner of using wax in Exper. 8 was right, Mr. C. was now to find if the wax-varnish, thus burnt into the picture would bear washing: but here he was a little disappointed; for rubbing one corner with a wet linen cloth, some of the colour

came off; but washing with a soft hair pencil dipped in water, and letting it dry without wiping, the colours stood very well, A board, painted as in Exp. 8, was hung in the most smoaky part of a chimney for a day, and exposed to the open air in a very foggy night. In the morning the board was seemingly wet through, and the water ran off the picture. This was suffered to dry without wiping, and the picture had not suffered at all from the smoke of the dew, either in the ground or the colours: but when dry, by rubbing it first with a soft cloth, and afterwards with a brush, it recovered its former gloss.

Suspecting that some tallow might have been mixed with the white wax he had used, which might cause the colours to come off on being rubbed with a wet cloth, he took yellow wax which had been melted from the honeycomb in a private family, and consequently not at all adulterated; to 3 parts of this he added one part resin, and melted them together.

Exper. 9.—Spanish white, mixed with fish glue, was put for a ground on a board and painted with water colours only. The board was made warm; and then the wax and resin were put on with a brush, and kept close to the fire till the picture had imbibed all the varnish, and looked dry. When cold, he rubbed it first with a linen cloth, and then polished it with a hard brush.

IX. On the Success of the preceding Experiment. In a Letter from Mr. Josiah Colebrook, F. R. S. p. 53.

This short letter accompanies a specimen of the encaustic on paper, being a bird drawn by Mr. George Edwards, on paper prepared with a ground of whitening and fish-glue, painted with water colours, and then the wax, &c. burned in. This will roll up as easily as common paper, without cracking the varnish. There are also 2 landscapes, painted by a young lady, after the same manner, on wood.

X. Of a Particular Species of Cocoon, or Silk Pod, from America. By the Rev. Samuel Pullein, M. A.* p. 54.

Having lately seen the aurelia of a particular species of caterpillar, Mr. P. judged, from its texture and consistence, that there might be procured from it a silk not inferior to that of the common silk-worm in its quality, and in its quantity much superior. He made some experiments on this new species of silk pod, which strengthen this opinion. This pod is about $3\frac{1}{4}$ inches in length, and above one inch in diameter; its outward form not so regular an oval as that of the common silk worm; its consistence somewhat like that of a dried bladder, when not fully blown; its colour of a reddish brown; its weight 21 grains.

* Several of the large moths of the Linnean division *attaci* form similar pods, in which they undergo their chrysalis state, as the phalæna Atlas, Hesperus, &c.

On cutting open this outer integument, there appeared a pod completely oval, as that of the silk-worm. It was covered with some floss-silk, by which it was connected to the outer coat, being of the same colour. Its length was 2 inches; its diameter nearly one inch; and its weight 9 grains. The pod could not be easily unwound, because it was perforated by the moth: but on putting it in hot water, he reeled off so much as sufficed to form a judgment of the strength and staple of its silk. The single thread winded off the pod in the same manner as that of the common silk-worm; seeming in all respects as fine and tough. He doubled this thread so often as to contain 20 in thickness; and the compound thread was as smooth as elastic, and as glossy as that of the common silk-worm. He tried what weight it would bear; and it bore 15½ oz., and broke with somewhat less than 16, on several trials. He then tried a thread of the common silk-worm, which was also composed of 20 (in thickness it rather exceeded the other); and it broke always with 15 oz. He boiled a part of the cocoon in water for 4 hours, that he might know whether it was composed of a gum in any sort mucilaginous; and he found that it was as indissoluble as that of the common silk-worm. The common silk-pod, with all its floss, weighs usually but 3 grains: and here is a pod which weighs 7 times as much. If the outer coat, which weighed 12 grains, were all to be used only as floss-silk, there remain 9 grains capable of being reeled; which is above 3 times as much as can be reeled from the common cocoon. But he is of opinion, that when the pod is fresh, and not hardened by age, the whole outer coat may be reeled off: for the pod on which he made these trials was 7 or 8 years old.

On inquiry, he found that the moth of this pod is called the isinglass by Marian. It is a very large moth, being 5 inches from the tip of each wing extended. It differs from the silk moth in having a proboscis, which intimates that it feeds in its papilio state, whereas the silk-moth never eats. The caterpillar which produces this pod is a native of America. It was found in Pennsylvania: the pod was fixed to the small branch of a tree, which seemed to be either of the crab or hawthorn species. The leaf of the tree had also helped to support the pod; for the mark of its ribs was apparent on the surface of the pod.

XI. A Thermometrical Account of the Weather, for One Year, beginning September 1753. *Kept in Maryland, by Rich. Brooke, M. D. Communicated by Mr. Henry Baker, F. R. S.* p. 58.

Merely a journal of the thermometer, with the weather, as to winds, rains, snow, &c.

XII. A Thermometrical Account of the Weather, for Three Years, beginning September 1754, *as observed in Maryland. By Rich. Brooke, M. D.* p. 70.

This is of the same nature as the preceding article.

XIII. Electrical Experiments and Observations. By Edward Delaval, M. A. p. 83.

Mr. D. filled several small glass tubes with the dry powders of calcined metals, viz. ceruss, lead ashes, minium, calx of antimony, &c. Into each end of every tube he put a piece of iron wire, which communicated with the calx, and fastened them with wax: so that the electric fluid, not being able to escape by means of the glass, must either pass through the calx, or not at all. On hanging one of the wires, bent for the purpose, to the electrified bar, and holding the other in his hand, he observed that no electric matter passed the calx, the snaps issuing all the while from the bar, or from that wire which was in contact with the bar. Animal and vegetable solids also, when reduced to ashes, and interposed in the same manner between two pieces of wire, he found as effectually intercept the electric stream, as the metallic calces. From these experiments we see, that animal, vegetable, and metallic bodies, though such known conductors of the electric fluid while in their entire state, are easily changed into resisters, or non-conductors of it.

Mr. D. was led to attempt this change from its having been observed, that dry mould would not conduct the electric fluid; and thence he suspected, that one class of the non-conductors must owe its property to an electrical virtue that would be found to reside in the calx, or earth of the chymists, after it is divested of the unctuous inflammable matter, which constitutes another of the chymical principles called sulphur; in like manner as this sulphur is constantly found highly electrical in all bodies where it abounds in a solid form, viz. resins, wax, &c. It must be remembered, that there is a remarkable and well-known opposition to the electrical effects of these two classes; the earthy one (as glass and stones) electrifying plus, and the sulphureous one minus. Does it not seem then a thing to be expected, in a body compounded of both, that the opposite powers of these ingredients should counterbalance and destroy the effects of each other, and the body in which the positive and negative ones equally prevail, become neutral, or non-electric?

There is another process, natural and without fire, which is supposed to destroy the sulphureous substance of metals, viz. when they are corroded, and moulder in the open air. Accordingly, with the same apparatus in which I tried the calcinations by fire, I examined the common rust of iron, and flake-white, which is the rust of lead, and find them equally converted into non-conductors in the open air. That this change, in metals particularly, is not owing to, or promoted by, the circumstance of mere pulverization, is evident, not only because the above-mentioned calces are equally strong electrics when formed into hard masses with a thin paste of flour and water, and after-

wards dried, but most clearly because the finest filings or powders of metals conduct as readily as the entire substances do.

But though this change will not succeed in metallic substances on mere pulverization, yet it seems to follow in most other hard bodies. Having dried a piece of Portland stone, he found it conducted perfectly well; but on powdering, and sealing it up in one of the tubes with the wire ends, as above, it became a perfect resister, or non-conductor, like the metallic calces. He tried the same experiment on a variety of other bodies, particularly gum arabic and alum; and had reason to believe it will succeed in all bodies that can be pulverized in the mortar.

Another very extraordinary means of making this change in bodies, which abound in calx or earth, is by fire, not by the intense one that calcines, but by a moderate heat; their most perfect resistance, or non-conducting property, being when their heat is just tolerable to our hands. Mr. D. had some of the same Portland stone, wrought into plates nearly as thin as window-glass, which he heated to a proper degree, and then coated on both sides with metal, in order to make the Leyden experiment. When the stone is hot enough to singe paper, it conducts as perfectly as when cold; but on cooling a little, it begins not to conduct, and affords small shocks, which gradually increase in strength for about 10 minutes; at which time it is about its most perfect state, and remains so near a quarter of an hour: after that time the shocks gradually decrease as the stone cools, till at last they quite cease, and it returns to its conducting state again; but this state appears before the stone is quite cold.

Experiments of this kind succeed in all bodies abounding in calx or earth, as stones dried clay, wood when rotten or burned in the fire till the surface becomes black. Among other substances, he tried a common tobacco-pipe, part of which near the middle he heated to a proper degree, and then applied one end of it to the electrified bar, while the other was held in the hand; and he observed that the electric fluid passed no farther along the pipe than to the heated part.

XIV. The Case of Wm. Carey, aged 19, *whose Tendons and Muscles were turning into Bones. By the Rev. William Henry, D. D., F. R. S. Dated Castle-Caldwell, near Enniskillen, March* 1, 1759. p. 89.

Wm. Carey was born in an island in Lough Melvill, a large lake in the northern point of the county of Leitrim in Ireland, and had continued there, or in the adjacent lands ever after. He was bred up to work as a labourer, and continued in very good health from his birth until 2 years before the above date. About

that time he felt an unusual pain in his right wrist, which in August 1757 began to swell: this obliged him to cease from his usual labour. In the space of a month more, this swelling became a hardness, like a bony substance; and continually shooting on, in Dec. reached up as far as the elbow; all the muscles continually growing into a bony substance, and dilating so, that his wrist and arm were twice as thick and broad as in the beginning. About a week after the pain began in his right wrist, he was seized with the like pain and swelling in the left wrist. This had proceeded in all respects in the same manner as in his right arm. The whole substance of each arm, from the elbow down to the wrists, felt as if it were one solid bone. The ossification was shooting downwards into the fingers, and upwards into the elbows; so as to prevent the bending of the fingers or elbow of the left arm. It had likewise shot upwards, so as to seize the great muscles of each arm between the elbows and shoulders. The continual pain and dilatation of the arms occasioned a bursting of the skin and fleshy parts about each elbow in Nov. 1758; out of which oozed a thin yellowish humour, with a little digested pus. Some of these breaches healed up of themselves. One small orifice in each elbow then continued to run.

In March 1758, he was seized with the like pain and swelling in his right ancle, whence such another bony substance soon grew as in his arms. This bony substance shot up from his ancle, both in the inward and outward side of the right leg, half way up to the knee; and the like bony substance in the inward side, shot downward from the pan of the knee 8 inches along the shinbone and was daily increasing; so that he walked with much pain and difficulty, and after resting in his walk, grew very lame. This person was of a very thin habit of body, and in size 5 feet 9 inches; somewhat inclined to an hectic, though he had no cough.

XV. A further Account of the same Case. By the Rev. William Henry, D. D., F. R. S. Dated Dublin, May 24th, 1759.

In this letter Dr. H. states, that he had sent Wm. Carey up in March preceding, to Mercer's hospital in Dublin; that after examining his case, the physicians and surgeons concluded, that the only probable chance to prevent the progress of the ossification, and to remove the evil already effected, was putting him into a mercurial course. This they tried; and after some slighter mercurial medicines, they, in the latter end of April, laid him down in a salivation, through which he passed with safety. This dried up the running sores at his elbows, occasioned by the bursting of the skin, through the ossification. Some lighter callus, which was shooting into the bones, seemed to be softened: in consequence of which he could move his elbows, and the joints of his fingers, with

more ease; and he had a little more clearness and vivacity in his countenance: but none of the ossified parts were reduced, nor was there any appearance of their reduction, and he continued to wear a hectic look. To reduce the ossified parts, they had applied to them mercurial plasters; the effect of which time would show.

As he was discharged out of the hospital, they had directed him to bathe continually in the ocean, which happened to be very convenient to his habitation; and had directed him to anoint his limbs with the soapy juice of the quercus marina, which lies in plenty along the shore.

XVI. An Account of the Comet seen in May 1759. *By J. Bevis, M. D.* p. 93.

Dr. B. had acquainted some of his friends, that it was his opinion a comet would hardly arise above our horizon of London Sunday April the 29th; but that probably we might see one April 30th. Sunday was very clear; but he could not see it. Monday, not so clear: however, he saw what he took for the comet through the smoke of the town, about 10, near the horizon.

Tuesday, May 1 saw it very plainly, at Mr. Short's, from 9 to 11. They compared it by means of the equatorial instrument with α, β, and ε corvi; whence its right ascension, at 8 h. 45 m. mean time, came out $159^{\circ}\ 55'\ 9''$, and its south declination $25^{\circ}\ 52'\ 14''$.

Wednesday, May 2d, observed it again at Mr. Sisson's in the Strand, with a sector of 5 feet radius, and compared it with β corvi; whence, at 9 h. 6 m. mean time, its right ascension $158^{\circ}\ 47'\ 37''$, and its south declination $22^{\circ}\ 19'\ 23''$. The increasing moon had now much weakened the light of the comet, so that the tail and nucleus could not be distinguished as last night.

I think, says Dr. B., I may now venture to pronounce this to be the same as the comet of 1682; and am about making out its future track. It is at this time about 4 times nearer to the earth than the sun is.

An Account of the Same Comet. By Nicolas Munckley, Esq. p. 94.

The first certain view Mr. M. had of any appearance, which could be the expected comet, was on the evening of the 30th of April, about s. s. w. a little lower than the middle of the Hydra. The following evening, May 1st, its place about 10 o'clock was, right ascension about 160 deg. declination a little more than 25 deg. s. It is a luminous appearance, very evident to the naked eye (notwithstanding the light of the moon, within 2 or 3 days of her quadrature), yet rather dim than splendid; large, but very ill defined. The telescope, at the same time it magnifies it, seems to render it more obscure. The nucleus appears to be rather surrounded with a circular haziness, than to have a tail in any particular direction, especially as seen through a telescope.

May 2, he saw the comet again very distinctly with the naked eye; but being then in London, without either globe or planisphere, he did not pretend to settle its place. The cloudiness of the evenings prevented his seeing the comet any more till the 5th and 6th of May: and on these days partly thin clouds, and partly the increasing light of the moon, rendered it much less easily discernible, both by the naked eye and in the telescope.

XVII. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1758, *pursuant to the Direction of Sir Hans Sloane, Baronet, Med. Reg. & Soc. Reg. nuper Præses, by John Wilmer, M.D. clariss. Societatis Pharmaceut. Lond. Socius, Hort. Chelsean. Præfectus & Prælector Botanic.* p. 96.

This is the 37th presentation of this kind, completing to the number of 1850 different plants.

XVIII. An Experimental Enquiry concerning the Natural Powers of Water and Wind to turn Mills, and other Machines, depending on a Circular Motion. By Mr. J. Smeaton, F. R. S. p. 100.

What Mr. S. communicates on this subject was originally deduced from experiments made on working models, which he thinks the best means of obtaining the outlines in mechanical inquiries. But in this case it is very necessary to distinguish the circumstances in which a model differs from a machine in large; otherwise a model is more apt to lead us from the truth than towards it. Hence the common observation, that a thing may do very well in a model, that will not answer in large. And indeed, though the utmost circumspection be used in this way, the best structure of machines cannot be fully ascertained, but by making trials with them, when made of their proper size. It is for this reason, that, though the models referred to, and the greatest part of the following experiments were made in the years 1752 and 1753, yet he deferred offering them to the Society, till he had an opportunity of putting the deductions from them in real practice, in a variety of cases, and for various purposes; so as to be able to assure the Society, that he had found them to answer.

PART I.—*Concerning Undershot Water-Wheels.*

Plate 13, fig. 9, is a perspective view of the machine for experiments on water-wheels; wherein ABCD is the lower cistern, or magazine, for receiving the water after it has quitted the wheel; and for supplying DE the upper cistern or head; where the water being raised to any height required, by a pump, that height is shown by FG, a small rod, divided into inches and parts; with a float at the bottom, to move the rod up and down, as the surface of the water rises

and falls.—HI is a rod by which the sluice is drawn, and stopped at any height required, by means of K a pin or peg, which fits several holes, placed in the manner of a diagonal scale, on the face of the rod HI.—GL is the upper part of the rod of the pump, for drawing the water out of the lower cistern, in order to raise and keep up its surface at its desired height, in the head DE; to supply the water expended by the aperture of the sluice.—MM is the arch and handle for working the pump, which is limited in its stroke by N, a piece for stopping the handle from raising the piston too high; that also being prevented from going too low, by meeting the bottom of the barrel.—O is the cylinder, on which a cord winds, and which being conducted over the pulley P and Q, raises R, the scale into which the weights are put, for trying the power of the water.—ST the two standards, which support the wheel, are made to slide up and down, to adjust the wheel as near as possible to the floor of the conduit.—W the beam which supports the scale and pulleys; this is represented as but little higher than the machine, for the sake of bringing the figure into a moderate compass, but in reality is placed 15 or 16 feet higher than the wheel.

Plate 13, fig. 10, is a section of the same machine, where the same parts are marked with the same letters as in fig. 9. Besides which, XX is the pump barrel being 5 inches diameter, and 11 inches long.—Y is the piston; and Z the fixed valve. GV is a cylinder of wood, fixed on the pump-rod, and reaches above the surface of the water; this piece of wood being of such a thickness, that its section is half the area of that of the pump-barrel, and will cause the surface of water to rise in the head, as much while the piston is descending, as while it is rising: and will thereby keep the gauge-rod FG more equally to its height. aa shows one of the two wires which serve as directors to the float, that the gauge rod FG may be kept perpendicular; for the same purpose also serves w, a piece of wood with a hole to receive the gauge-rod, and keep it upright; b is the aperture of the sluice; cc a kant-board, for throwing the water more directly down the opening cd, into the lower cistern: and ce is a sloping board, for bringing back the water that is thrown up by the floats of the wheel.

Fig. 11. represents one end of the main axis, with a section of the moveable cylinder, marked O in the preceding figures.—ABCD is the end of the axis; of which the parts B and D are covered with ferrules or hoops of brass. E is a cylinder of metal; the part marked F being the pivot or gudgeon. cc is the section of a hollow cylinder of wood, the diameter of the interior part being somewhat larger than the cylindrical ferrule B. aa is the section of a ferrule of brass, driven into the end of the hollow cylinder, and which is adjusted to that marked B, so as to slide freely on it, but with as little shake as possible. bb, dd, gg, represent the section of a brass ferrule, plate, and socket, fixed on the other end of the hollow cylinder; the socket dd being adjusted to slide freely on

the cylinder E, in the same manner as the ferrule aa slides on the cylinder B; the outer end of the socket at gg is formed into a sort of button; by pushing it, the hollow cylinder will move backwards and forwards, or turn round at pleasure on the cylindrical parts of the axis B and E. ee, ii, oo, represent the section of a brass ferrule, also fixed on the hollow cylinder: the edge of this ferrule ee is cut into teeth, in the manner of a contrate wheel; and its edge oo is cut in the manner of a ratchet.

Of consequence, when the plate bddb is pushed close to the ferrule D, the teeth of the ferrule ee will lay hold of G, a pin fixed into the axis; by which means the hollow cylinder is made to turn along with the wheel and axis: but being drawn back by the button gg, the hollow cylinder is disengaged from the pin G, and ceases turning. Note. The weight in the scale is prevented from running back, by a catch that plays in and lays hold of the ratchet oo. By this means the hollow cylinder on which the cord winds, and raises the weight, is put in action and discharged from it instantaneously, while the wheel is in motion: for without some contrivance of this kind, it would not be easy to make this sort of experiments with any tolerable degree of exactness.

The use of the apparatus now described will be rendered more intelligible, by giving a general idea of what I had in view; but as I shall be obliged, says Mr. S., to make use of a term which has heretofore been the cause of disputation, I think it necessary to assign the sense in which I would be understood to use it; and in which I apprehend it is used by practical mechanics. The word *Power*,* as used in practical mechanics, I apprehend to signify the exertion of strength, gravitation, impulse, or pressure, so as to produce motion: and by means of strength, gravitation, impulse, or pressure, compounded with motion, to be capable of producing an effect: and that no effect is properly mechanical, but what requires such a kind of power to produce it. The raising of a weight, relative to the height to which it can be raised in a given time, is the most proper measure of power; or, in other words, if the weight raised is multiplied by the height to which it can be raised in a given time, the product is the measure of the power raising it;* and consequently, all those powers are equal, whose products, made by such multiplication, are equal: for if a power can raise twice the weight to the same height; or the same weight to twice the height, in the same time that another power can, the first power is double the second:*

* All these preliminary remarks about power, evince a considerable degree of confusion in Mr. S.'s mind on mechanical power or force, and the measure of the same. With some good practical notions on machinery, he contrived to make some useful experiments; but unfortunately, for want of some more precise ideas on the theory of physics, he has vitiated many of the conclusions, often making them appear to be very different from those determined by theory, while, rightly deduced, they are found nearly to agree.

and if a power can raise half the weight to double the height; or double the weight to half the height, in the same time that another can, those two powers are equal.* But note, all this is to be understood in case of slow or equable motion of the body raised; for in quick, accelerated, or retarded motions, the vis inertiæ of the matter moved will make a variation.

In comparing the effects produced by water-wheels, with the powers producing them; or, in other words, to know what part of the original power is necessarily lost in the application, we must previously know how much of the power is spent in overcoming the friction of the machinery, and the resistance of the air; also what is the real velocity of the water at the instant that it strikes the wheel; and the real quantity of water expended in a given time. From the velocity of the water, at the instant that it strikes the wheel, given; the height of head productive of such velocity can be deduced, from acknowledged and experimented principles of hydrostatics; so that by multiplying the quantity, or weight of water, really expended in a given time, by the height of head so obtained; which must be considered as the height from which that weight of water had descended in that given time; we shall have a product, equal to the original power of the water; and clear of all uncertainty, that would arise from the friction of the water, in passing small apertures; and from all doubts, arising from the different measure of spouting waters, assigned by different authors. On the other hand, the sum of the weights raised by the action of this water, and of the weight required to overcome the friction and resistance of the machine, multiplied by the height to which the weight can be raised in the time given, the product will be equal to the effect of that power; and the proportion of the two products will be the proportion of the power to the effect; so that by loading the wheel with different weights successively, we shall be able to determine at what particular load, and velocity of the wheel, the effect is a maximum.

The manner of finding the real velocity of the water, at the instant of its striking the wheel; the manner of finding the value of the friction, resistance, &c. in any given case; and the manner of finding the real expence of water, so far as concerns the following experiments, without having recourse to theory; being matters on which the following determinations depend, it will be necessary to explain them.

To determine the Velocity of the Water striking the Wheel.

It has already been mentioned, in the references to the figures, that weights are raised by a cord winding round a cylindrical part of the axis. First, then, let the wheel be put in motion by the water, but without any weights in the scale; and let the number of turns in a minute be 60; now it is evident, that were the wheel free from friction and resistance; that 60 times the circumference of the wheel would be the space through which the water would have moved in

a minute; with that velocity wherewith it struck the wheel: but the wheel being incumbered by friction and resistance, and yet moving 60 turns in a minute, it is plain, that the velocity of the water must have been greater than 60 circumferences before it met with the wheel. Let now the cord be wound round the cylinder, but contrary to the usual way, and put a weight in the scale; the weight so disposed, which may be called the counter-weight, will endeavour to assist the wheel in turning the same away, as it would have been turned by the water: put therefore as much weight into the scale as, without any water, will cause it to turn somewhat faster than at the rate of 60 turns in a minute: suppose 63; let it now be tried again by the water, assisted by the weight; the wheel therefore will now make more than 60 turns; suppose 64; hence we conclude the water still exerts some power in giving motion to the wheel. Let the weight be again increased, so as to make $64\frac{1}{2}$ turns in a minute without water: let it once more be tried with water as before; and suppose it now to make the same number of turns with water as without, viz. $64\frac{1}{2}$; hence it is evident, that in this case the wheel makes the same number of turns in a minute, as it would do if the wheel had no friction or resistance at all; because the weight is equivalent thereto; for were it too little, the water would accelerate the wheel beyond the weight; and if too great, retard it; so that the water now becomes a regulator of the wheel's motion; and the velocity of its circumference becomes a measure of the velocity of the water.

In like manner, in seeking the greatest product, or maximum of effect; having found by trials what weight gives the greatest product, by simply multiplying the weight in the scale by the number of turns of the wheel, find what weight in the scale, when the cord is on the contrary side of the cylinder, will cause the wheel to make the same number of turns the same way, without water; it is evident that this weight will be nearly equal to all friction and resistance taken together; and consequently, that the weight in the scale, with twice* the weight of the scale, added to the back or counter-weight, will be equal to the weight that could have been raised, supposing the machine had been without friction or resistance; and which multiplied by the height to which it was raised, the product will be the greatest effect of that power.

The Quantity of Water expended is found thus:

The pump made use of for replenishing the head with water was so carefully made, that no water escaping back by the leathers, it delivered the same quantity of water at every stroke, whether worked quick or slow; and as the length of the stroke was limited, consequently the value of one stroke (or on account of more exactness 12 strokes) was known, by the height to which the water was

* The weight of the scale makes part of the weight both ways.—Orig.

thereby raised in the head; which being of a regular figure was easily measured. The sluice by which the water was drawn upon the wheel, was made to stop at certain heights by a peg; so that when the peg was in the same hole, the aperture for the effluent water was the same. Hence the quantity of water expended by any given head, and opening of the sluice, may be obtained: for by observing how many strokes a minute was sufficient to keep up the surface of the water at the given height, and multiplying the number of strokes by the value of each, the water expended by any given aperture and head in a given time will be given.

These things will be further illustrated by going over the calculus of one set of experiments.

Specimen of a Set of Experiments.

The sluice drawn to the 1st hole.

The water above the floor of the sluice.................. 30 Inches.
Strokes of the pump in a minute.......................... $39\frac{1}{2}$
The head raised by 12 strokes............................ 21
The wheel raised the empty scale, and made turns in a minute 80
With a counter-weight of 1 lb. 8 oz. it made............. 85
Ditto tried with water................................... 86

Nº	Weight. lb.	oz.	Turns in a min.	Product.
1	4	0	45	180
2	5	0	42	210
3	6	0	$36\frac{1}{4}$	$217\frac{1}{2}$
4	7	0	$33\frac{3}{4}$	$236\frac{1}{4}$
5	8	0	30	240 maximum
6	9	0	$26\frac{1}{2}$	$238\frac{1}{2}$
7	10	0	22	220
8	11	0	$16\frac{1}{2}$	$181\frac{1}{2}$
9	12	* ceased working.		

Counter-weight for 30 turns without water, 2 oz. in the scale.

N. B. The area of the head was 105.8 square inches.

Weight of the empty scale and pulley, 10 oz.

Circumference of the cylinder, 9 inches.

Circumference of the water-wheel, 75 ditto.

Reduction of the above Set of Experiments.

The circumference of the wheel 75 inches, multiplied by 86 turns, gives 6450 inches for the velocity of the water in a minute; $\frac{1}{60}$ of which will be the velocity,

* N. B. When the wheel moves so slow as not to rid the water so fast as supplied by the sluice the accumulated water falls back upon the aperture, and the wheel immediately ceases moving.—Orig.

in a second, equal to 107.5 inches, or 8.96 feet, which is due to a head of 15 inches;* and this we call the virtual or effective head.

The area of the head being 105.8 inches, this multiplied by the weight of the water of the inch cubic, equal to the decimal .579 of the ounce avoirdupois, gives 61.26 ounces for the weight of as much water as is contained in the head, on 1 inch in depth, $\frac{1}{16}$ of which is 3.83 pounds; this multiplied by the depth 21 inches, gives 80.43 lb. for the value of 12 strokes; and by proportion, $39\frac{1}{2}$, the number made in a minute, will give 264.7 lb. the weight of water expended in a minute.

Now as 264.7 lb. of water may be considered as having descended through a space of 15 inches in a minute, the product of these two numbers 3970 will express the power of the water to produce mechanical effects; which were as follows.

The velocity of the wheel at the maximum, as appears above, was 30 turns a minute; which multiplied by 9 inches, the circumference of the cylinder makes 270 inches; but as the scale was hung by a pulley and double line, the weight was only raised half of this, viz. 135 inches.

The weight in the scale at the maximum........	8 lb.	0 oz.
Weight of the scale and pulley...............	0	10
Counterweight, scale, and pulley..............	0	12
Sum of the resistance........................	9	6
	or lb. 9.375.	

Now as 9.375 lb. is raised 135 inches, these two numbers being multiplied together, the product is 1266, which expresses the effect produced at a maximum: so that the proportion of the power to the effect is as 3970 : 1266, or as 10 : 3.18.

But though this is the greatest single effect producible from the power mentioned, by the impulse of the water on an undershot wheel; yet as the whole power of the water is not exhausted by it, this will not be the true ratio between the power of the water, and the sum of all the effects producible from it: for as the water must necessarily leave the wheel with a velocity equal to the wheel's circumference, it is plain that some part of the power of the water must remain after quitting the wheel.

The velocity of the wheel at the maximum is 30 turns a minute; and consequently its circumference moves at the rate of 3.123 feet a second, which answers to a head 1.82 inches; this being multiplied by the expence of water in a

* This is determined on the common maxim of hydrostatics, that the velocity of spouting waters, is equal to the velocity that a heavy body would acquire in falling from the height of the reservoir; and is proved by the rising of jets to the height of their reservoirs nearly.—Orig.

minute, viz. 264.7 lb. produces 481 for the power remaining in the water after it has passed the wheel: this being therefore deducted from the original power 3970, leaves 3489, which is that part of the power which is spent in producing the effect 1266; and consequently the part of the power spent in producing the effect, is to the greatest effect producible by it, as 3489 : 1266 : : 10 : 3.62, or as 11 to 4.

The velocity of the water striking the wheel has been determined to be equal to 86 circumferences of the wheel per minute, and the velocity of the wheel at the maximum to be 30; the velocity of the water will therefore be to that of the wheel, as 86 to 30, or as 10 to 3.5, or as 20 to 7.

The load at the maximum has been shown to be equal to 9 lb. 6 oz. and that the wheel ceased moving with 12 lb. in the scale: to which if the weight of the scale be added, viz. 10 ounces,* the proportion will be nearly as 3 to 4 between the load at the maximum and that by which the wheel is stopped.

It is somewhat remarkable, that though the velocity of the wheel, in relation to the water, turns out greater than $\frac{1}{3}$ of the velocity of the water, yet the impulse of the water in the case of a maximum is more than double of what is assigned by theory; that is, instead of $\frac{4}{9}$ of the column, it is nearly equal to the whole column.

It must be remembered therefore, that in the present case, the wheel was not placed in an open river, where the natural current, after it has communicated its impulse to the float, has room on all sides to escape, as the theory supposes; but in a conduit or race to which the float being adapted, the water cannot otherwise escape than by moving along with the wheel. It is observable, that a wheel working in this manner, as soon as the water meets the float, receiving a sudden check, it rises up against the float, like a wave against a fixed object; insomuch that when the sheet of water is not a quarter of an inch thick before it meets the float, yet this sheet will act on the whole surface of a float, whose height is 3 inches; and consequently were the float no higher than the thickness of the sheet of water, as the theory also supposes, a great part of the force would have been lost, by the water's dashing over the float.†

* The resistance of the air in this case ceases, and the friction is not added, as 12 lb. in the scale was sufficient to stop the wheel after it had been in full motion; and therefore somewhat more than a counterbalance to the impulse of the water.—Orig.

† Since the above was written, I find that Professor Euler, in the Berlin Acts for the year 1748, in a memoire intitled, Maximes pour arranger le plus avantageusement les machines destinées à elever de l'eau par le moyen de pompes, page 192. § 9. has the following passage; which seems to be the more remarkable, as I don't find he has given any demonstration of the principle therein contained, either from theory or experiment; or has made any use of it in his calculations on this subject.——" Cependant dans ce cas puisque l'eau est reflechie, & qu'elle decoule sur les aubes vers les cotés, elle y exerce encore une force particuliere, dont l'effet de l'impulsion sera augmenté; & experience

In further confirmation of what is already delivered, the following table is adjoined, containing the result of 27 sets of experiments, made and reduced as above. What remains of the theory of undershot wheels, will naturally follow from a comparison of the different experiments together.

TABLE I.

N°.	Height of the water in the cistern.	Turns of the wheel unloaded.	Virtual heat thence deduced.	Turns at the maximum.	Load at the equilibrium.	Load at the maximum.	Water expended in a minute.	Power.	Effect.	Ratio of the power and effect.	Ratio of the velocity of the water and wheel.	Ratio of the load at the equilibrium, to the load at the maximum.	Experiments.
	In.		In.		lb. oz.	lb. oz							
1	33	88	15.85	30	13 10	10 9	275	4358	1411	10:3.24	10:3.4	10:7.75	At the 1st hole.
2	30	86	15.0	30	12 10	9 6	264.7	3970	1266	10:3.2	10:3.[illegible]	10:7.4	
3	27	82	13 7	28	11 2	8 6	243	3329	1044	10:3.15	10:3.4	10:7.5	
4	24	78	12.3	27.7	9 10	7 5	235	2890	901.4	10:3.12	10:3.55	10:7.53	
5	21	75	11.4	25.9	8 10	6 5	214	2439	735.7	10:3.02	10:3.45	10:7.32	
6	18	70	9 95	23.5	6 10	5 5	199	1970	561.8	10:2.85	10:3.36	10:8.02	
7	15	65	8.54	23.4	5 2	4 4	178 5	1524	442.5	10:2 9	10:3 6	10:8.3	
8	12	60	7.29	22.	3 10	3 5	161	1173	328	10:2 8	10:3.77	0:9.1	
9	9	52	5.47	19.	2 12	2 8	134	733	213.7	10:2.9	10:[illegible].6	10:9.1	
10	6	42	3.55	16.	1 12	1 10	114	404.7	117	0.2.82	10:3 8	10:9.3	
11	24	84	14.2	30.75	13 10	10 14	342	4890	505	10:3.075	10:3 66	10:[illegible].9	At the 2d.
12	21	81	13.5	29	11 10	9 6	297	4009	1223	10:3. 01	10:3.[illegible]	10:8.05	
13	18	72	10 5	26	9 10	8 7	285	2993	975	10:3. 25	10:3.6	10:8 75	
14	15	69	9.6	25	7 10	6 14	277	2659	774	10:2 9[illegible]	10:3 6[illegible]	10:9	
15	12	63	8.0	25	5 10	4 14	234	1872	549	10:2. 9[illegible]	10:3 9[illegible]	10:8.7	
16	9	56	6.37	23	4 0	3 13	201	1280	390	10:3. 05	10:4.1	10:9 5	
17	6	46	4.25	21	2 8	2 4	167.5	712	212	10:2. 98	10:4.55	10:9.	
18	15	72	10.5	29	11 10	9 6	357	3748	12[illegible]0	10:3.23	10:4.02	10:8 05	The 3d.
19	12	66	8.75	26.75	8 10	7 6	3[illegible]0	2887	878	10:3 05	10:4.05	10:8.1	
20	9	58	6.8	24.5	5 8	5 0	255	1734	541	10:3 01	10:4.22	10:9.1	
21	6	48	4.7	23.5	3 2	3 0	228	1064	317	10:2.99	10:4.9	10:9.6	
22	12	68	9.3	27	9 2	8 6	359	3338	1006	0:3.02	10:3.97	10:9.17	4th.
23	9	58	6 8	26.25	6 2	5 13	332	2257	686	10:3.04	10:4.52	10:9.5	
24	6	48	4.7	24.5	3 12	3 8	262	1231	3[illegible]5	10:3.13	10:5.1	10:9 35	
25	9	60	7.29	27.3	6 12	6 6	3[illegible]5	2588	783	10:3.03	10:4 55	10:9.[illegible]5	5th.
26	6	50	5.03	24.6	4 6	4 1	307	1544	450	10:2.92	10:4 9	10.9.3	
27	6	50	5.03	26	4 15	4 9	360	1811	534	10:2.95	10:5.2	10:9.[illegible]5	6th.
1	2	3	4	5	6	7	8	9	10	11	12	13	

jointe a la theorie a fait voir que dans ce cas, la force est presque double : de sorte qu'il faut prendre le double de la section du fil d'eau pour ce qui repond dans ce cas a le surface des aubes, poûrvu qu'elles soient assez larges pour recevoir ce supplement de force. Car si les aubes n'étoient plus larges que le fil ou trait d'eau, on ne devroit prendre que la simple section, tout comme dans le premier cas, ou l'aube toute entière est pappee par l'eau."—Orig.

Maxims and Observations deduced from the foregoing Table of Experiments.

Maxim 1. That the virtual or effective head being the same, the effect will be nearly as the quantity of water expended.—This will appear by comparing the contents of the columns 4, 8, and 10, in the foregoing sets of experiments.

Maxim 2. That the expence of water being the same, the effect will be nearly as the height of the virtual or effective head.—This also will appear by comparing the contents of columns 4, 8, and 10, in any of the sets of experiments.

Maxim 3. That the quantity of water expended being the same, the effect is nearly as the square of its velocity.—This will appear by comparing the contents of columns 3, 8, and 10, in any of the sets of experiments.

Maxim 4. The aperture being the same, the effect will be nearly as the cube of the velocity of the water.—This also will appear by comparing the contents of columns 3, 8, and 10.

Obs. 1. On comparing column 2d and 4th, tab. i. it is evident, that the virtual head bears no certain proportion to the head of water: but that when the aperture is greater, or the velocity of the water issuing therefrom less, they approach nearer to a coincidence: and consequently in the large opening of mills and sluices, where great quantities of water are discharged from moderate heads, the head of water, and virtual head determined from the velocity, will nearly agree, as experience confirms.

Obs. 2. On comparing the several proportions between the power and effect in column 11, the most general is that of 10 to 3; the extremes 10 to 3.2 and 10 to 2.8; but as it is observable, that where the quantity of water, or its velocity, that is, where the power is greatest, the 2d ratio is greatest also: we may therefore well allow the proportion subsisting in large works, as 3 to 1.

Obs. 3. The proportions of velocities between the water and wheel in column 12, are contained within the limits of 3 to 1 and 2 to 1; but as the greater velocities approach the limit of 3 to 1, and the greater quantity of water approach to that of 2 to 1, the best general proportion will be that of 5 to 2.

After the experiments above mentioned were tried, the wheel, which had originally 24 floats, was reduced to 12; which caused a diminution in the effect, on account of a greater quantity of water escaping between the floats and the floor; but a circular sweep being adapted thereto, of such a length, that one float entered the curve before the preceding one quitted it, the effect came so near to the former, as not to give hopes of advancing it by increasing the number of floats beyond 24 in this particular wheel.

PART II. *Concerning Overshot Wheels.*

In the former part of this essay, we have considered the impulse of a confined

stream, acting on undershot wheels. We now proceed to examine the power and application of water, when acting by its gravity on overshot wheels.

In reasoning without experiment, one might be led to imagine, that however different the mode of application is; yet that whenever the same quantity of water descends through the same perpendicular space, that the natural effective power would be equal; supposing the machinery free from friction, equally calculated to receive the full effect of the power, and to make the most of it: for if we suppose the height of a column of water to be 30 inches, and resting on a base or aperture of one inch square; every cubic inch of water that issues will acquire the same velocity or momentum, from the uniform pressure of 30 cubic inches above it, that one cubic inch let fall from the top will acquire in falling down to the level of the aperture; viz. such a velocity as in a contrary direction would carry it to the level from which it fell;* one would therefore suppose, that a cubic inch of water, let fall through a space of 30 inches, and there striking another body, would be capable of producing an equal effect by collision, as if the same cubic inch had descended through the same space with a slower motion, and produced its effects gradually: for in both cases gravity acts on an equal quantity of matter through an equal space;† and consequently, that whatever was the ratio between the power and effect in undershot wheels, the same would obtain in overshot, and indeed in all others: yet however conclusive this reasoning may seem, it will appear, in the course of the following deductions, that the effect of the gravity of descending bodies is very different from the effect of the stroke of such as are non-elastic, though generated by an equal mechanical power.

The alterations in the machinery already described, to accommodate the same for experiments on overshot wheels, were principally as follow:

Pl. 13, fig. 10, the sluice IB being shut down, the rod HI was unscrewed and taken off. The undershot water-wheel was taken off the axis, and instead of it an overshot wheel of the same diameter was put in its place. This wheel was 2 inches in the shroud or depth of the bucket: the number of the buckets was 36. The standards S and T, fig. 9, were raised half an inch, so that the bottom of the wheel might be clear of stagnant water. A trunk for bringing the water on the wheel was fixed according to the dotted lines fg, fig. 10. The aperture was

* This is a consequence of the rising of jetts to the height of the reservoirs nearly.

† Gravity, it is true, acts a longer space of time on the body that descends slow, than on that which falls quick; but this cannot occasion the difference in the effect: for an elastic body falling through the same space in the same time, will, by collision on another elastic body, rebound nearly to the height from which it fell; or by communicating its motion, cause an equal one to ascend to the same height.—Orig.

adjusted by a shuttle hi, which also closed up the outer end of the trunk, when the water was to be stopped.

Fig. 11, the ratchet oo, not being of one piece of metal with the ferrule ee, ii (though so described before, to prevent unnecessary distinctions,) was with its catch turned the contrary side; consequently the moveable barrel would do its office equally, notwithstanding the water wheel, when at work, moved the contrary way.

Specimen of a Set of Experiments.

Head 6 inches. 14 $\frac{1}{2}$ strokes of the pump in a minute, 12 ditto = 80 lb.*

Weight of the scale (being wet) 10 $\frac{1}{2}$ oz.

Counterweight for 20 turns, besides the scale, 3 oz.

No.	Weight in the Scale.	Turns.	Product.	Observations.
1	0 lb.	60		Threw most part of the water out of the wheel.
2	1	56		
3	2	52		
4	3	49	147	Received the water more quietly.
5	4	47	188	
6	5	45	225	
7	6	42$\frac{1}{2}$	255	
8	7	41	287	
9	8	38$\frac{1}{2}$	308	
10	9	36$\frac{1}{2}$	328$\frac{1}{2}$	
11	10	35$\frac{1}{2}$	355	
12	11	32$\frac{3}{4}$	360$\frac{1}{2}$	
13	12	31$\frac{1}{4}$	375	
14	13	28$\frac{1}{2}$	370$\frac{1}{2}$	
15	14	27$\frac{1}{2}$	385	
16	15	26	390	
17	16	24$\frac{1}{2}$	392	
18	17	22$\frac{3}{4}$	386$\frac{3}{4}$	
19	18	21$\frac{3}{4}$	391$\frac{1}{2}$	
20	19	20$\frac{3}{4}$	394$\frac{1}{4}$	Maximum.
21	20	19$\frac{3}{4}$	395	
22	21	18$\frac{1}{4}$	388$\frac{1}{4}$	
23	22	18	396	Worked irregular.
24	23	Overset by its load.		

In these experiments, the head being 6 inches, and the height of the wheel 24 inches, the whole descent will be 30 inches: the expence of water was 14$\frac{1}{2}$ strokes of the pump in a minute, 12 containing 80lb.: therefore the water expended in a minute was 96$\frac{2}{3}$ lb. which, multiplied by 30 inches, gives the power = 2900.

If we take the 20th experiment for the maximum, we shall have 20$\frac{3}{4}$ turns in a minute, each of which raised the weight 4$\frac{1}{2}$ inches, that is, 93,37 inches in a minute. The weight in the scale was 19 lb. the weight of the scale 10$\frac{1}{2}$ oz.; the counter-weight 3 oz. in the scale, which, with the weight of the scale 10$\frac{1}{2}$

* The small difference, in the value of 12 strokes of the pump, from the former experiments, was owing to a small difference in the length of the stroke, occasioned by the warping of the wood.

oz. makes in the whole $20\frac{1}{4}$ lb. which is the whole resistance or load: this, multiplied by 93,37 inches, makes 1914 for the effect.

The ratio therefore of the power and effect will be as 2900 : 1914, or as 10 : 6.6, or as 3 : 2 nearly. But if we compute the power from the height of the wheel only, we shall have $96\frac{2}{3}$ lb. multiplied by 24 inches = 2320 for the power, and this will be to the effect as 2320 : 1914, or as 10 : 8,2, or as 5 : 4 nearly.

TABLE II. *containing the Result of 16 Sets of Experiments on Overshot Wheels.*

No.	Whole descent.	Water expended in a minute.	Turns at the maximum in a min.	Weight raised at the maximum.	Power of the whole descent.	Power of the wheel.	Effect.	Ratio of the whole power and effect.	Ratio of power of the wheel and effect.	Mean ratio.
	Inch	lb.		lb.						
1	27	30	19	$6\frac{1}{2}$	810	720	556	10 : 6.9	10 : 7.7	Medium 10 : 8.1
2	27	$56\frac{2}{3}$	$16\frac{1}{4}$	$14\frac{1}{2}$	1530	1360	1060	10 : 6.9	10 : 7.8	
3	27	$56\frac{2}{3}$	$20\frac{3}{4}$	$12\frac{1}{2}$	1530	1360	1167	10 : 7.6	10 : 8.4	
4	27	$63\frac{1}{3}$	$20\frac{1}{2}$	$13\frac{1}{2}$	1710	1524	1245	10 : 7.3	10 : 8.[illegible]	
5	27	$76\frac{2}{3}$	$21\frac{1}{2}$	$15\frac{1}{2}$	2070	1840	1500	10 : 7.3	10 : 8.2	
6	$28\frac{1}{2}$	$73\frac{1}{3}$	$18\frac{3}{4}$	$17\frac{1}{2}$	2090	1764	1476	10 : 7.	10 : 8.4	10 : 8.2
7	$28\frac{1}{2}$	$96\frac{2}{3}$	$20\frac{1}{4}$	$20\frac{1}{2}$	2755	2320	1868	10 : 6.8	10 : 8.1	
8	30	90	20	$19\frac{1}{2}$	2700	2160	1755	10 : 6.5	10 : 8.1	10 : 8.2
9	30	$96\frac{2}{3}$	$20\frac{3}{4}$	$20\frac{1}{2}$	2900	2320	1914	10 : 6.6	10 : 8.2	
10	30	$113\frac{1}{3}$	21	$23\frac{1}{2}$	3400	2720	222[illegible]	10 : 6.5	10 : 8.2	
11	33	$56\frac{2}{3}$	2[illegible] $\frac{1}{4}$	$13\frac{1}{2}$	1870	1360	1230	10 : 6.6	10 : 9.	10 : 8.5
12	33	$106\frac{2}{3}$	$22\frac{1}{4}$	$21\frac{1}{2}$	3520	2560	2153	10 : 6.1	10 : 8.4	
13	33	$146\frac{2}{3}$	23	$27\frac{1}{2}$	4840	3520	2846	10 : 5.9	10 : 8.1	
14	35	65	$19\frac{3}{4}$	$16\frac{1}{2}$	2275	1560	1466	10 : 6.5	10 : 9.1	10 : 8.5
15	35	120	2[illegible] $\frac{1}{2}$	$25\frac{1}{2}$	4200	288	2467	10 : 5.9	10 : 8 6	
16	35	$163\frac{1}{2}$	25	$26\frac{1}{2}$	5728	2	29[illegible]1	10 : 5.2	10 : 7.6	
1	2	3	4	5	6	7	8	9	10	11

Observations and Deductions from the foregoing Experiments.

I. Concerning the Ratio between the Power and Effect of Overshot Wheels.

The effective power of the water must be reckoned on the whole descent; because it must be raised that height, in order to be in a condition of producing the same effect a second time.

The ratios between the powers so estimated, and the effects at the maximum deduced from the several sets of experiments, are exhibited at one view in column 9 of Table II. and hence it appears, that those ratios differ from that of 10 to 7.6

to that of 10 : 5.2, that is, nearly from 4 : 3 to 4 : 2. In those experiments where the heads of water and quantities expended are least, the proportion is nearly as 4 : 3; but where the heads and quantities are greatest, it approaches nearer to that of 4 : 2; and by a medium of the whole, the ratio is that of 3 : 2 nearly. We have seen before, in the observations on the effects of undershot wheels, that the general ratio of the power to the effect, when greatest, was 3 : 1; the effect therefore of overshot wheels, under the same circumstances of quantity and fall, is at a medium double to that of the undershot:" and, as a consequence thereof, "that nonelastic bodies, when acting by their impulse or collision, communicate only a part of their original power;" the other part being spent in changing their figure, in consequence of the stroke.

The powers of water computed from the height of the wheel only, compared with the effects, as in column 10, appear to observe a more constant ratio: for if we take the medium of each class, which is set down in column 11, we shall find the extremes to differ no more than from the ratio of 10 : 8,1 to that of 10:8.5; and as the 2d term of the ratio gradually increases from 8.1 to 8.5, by an increase of head from 3 inches to 11, the excess of 8.5 above 8.1, is to be imputed to the superior impulse of the water at the head of 11 inches above that of 3 inches: so that if we reduce 8.1 to 8, on account of the impulse of the 3-inch head, 'we shall have the ratio of the power, computed on the height of the wheel only, to the effect at a maximum as 10 : 8, or as 5 : 4 nearly:' and from the equality of the ratio between power and effect, subsisting where the constructions are similar, we must infer, that the effects, as well as the powers, are as the quantities of water and perpendicular heights multiplied together respectively.

II. Concerning the Most Proper Height of the Wheel in Proportion to the Whole Descent.

We have already seen, from the preceding observation, that the effect of the same quantity of water, descending through the same perpendicular space, is double, when acting by its gravity on an overshot wheel, to what the same produces when acting by its impulse on an undershot. It also appears, that by increasing the head from 3 inches to 11, that is, the whole descent, from 27 inches to 35, or in the ratio of 7 to 9 nearly, the effect is advanced no more than in the ratio of 8.1 to 8.4, that is, as 7 : 7.26; and consequently the increase of effect as not 1-7th of the increase of perpendicular height. Hence it follows, that the higher the wheel is in proportion to the whole descent, the greater will be the effect;' because it depends less on the impulse of the head, and more on the gravity of the water in the buckets: and if we consider how obliquely the water issuing from the head must strike the buckets, we shall not be at a loss to

account for the little advantage that arises from its impulse; and shall immediately see of how little consequence this impulse is to the effect of an overshot wheel. However, as every thing has its limits, so has this; for thus much is desirable, 'that the water should have somewhat greater velocity, than the circumference of the wheel, in coming on it;' otherwise the wheel will not only be retarded, by the buckets striking the water, but thence dashing a part of it over, so much of the power is lost. The velocity that the circumference of the wheel ought to have, being known by the following deductions, the head requisite to give the water its proper velocity is easily computed from the common rules of hydrostatics; and will be found much less than what is generally practised.

III. Concerning the Velocity of the Circumference of the Wheel, in order to produce the Greatest Effect.

If a body is let fall freely from the surface of the head to the bottom of the descent, it will take a certain time in falling; and in this case the whole action of gravity is spent in giving the body a certain velocity: but if this body in falling be made to act on some other body, so as to produce a mechanical effect, the falling body will be retarded; because a part of the action of gravity is then spent in producing the effect, and the remainder only giving motion to the falling body: and therefore 'the slower a body descends, the greater will be the portion of the action of gravity applicable to the producing a mechanical effect;' and in consequence the greater that effect may be.

If a stream of water fall into the bucket of an overshot wheel, it is there retained till the wheel by moving round discharges it: of consequence the slower the wheel moves, the more water each bucket will receive: so that what is lost in speed, is gained by the pressure of a greater quantity of water acting in the buckets at once: and, if considered only in this light, the mechanical power of an overshot wheel to produce effects will be equal, whether it moves quick or slow: but if we attend to what has been just now observed of the falling body, it will appear that so much of the action of gravity, as is employed in giving the wheel and water in it a great velocity, must be subtracted from its pressure on the buckets; so that, though the product made by multiplying the number of cubic inches of water acting in the wheel at once by its velocity, will be the same in all cases; yet, as each cubic inch, when the velocity is greater, does not press so much on the bucket as when it is less, the power of the water to produce effects will be greater in the less velocity than in the greater: and hence we are led to this general rule, "That, cæteris paribus, the less the velocity of the wheel, the greater will be the effect of it." A confirmation of this doctrine, together with the limits it is subject to in practice, may be deduced from the foregoing specimen of a set of experiments.

From these experiments it appears, that when the wheel made about 20 turns in a minute, the effect was, nearly at the greatest. When it made 30 turns, the effect was diminished about $\frac{1}{20}$ part; but that when it made 40, it was diminished about $\frac{1}{4}$; when it made less than $18\frac{1}{4}$, its motion was irregular; and when it was loaded so as not to admit its making 18 turns, the wheel was overpowered by its load.

It is an advantage in practice, that the velocity of the wheel should not be diminished further than what will procure some solid advantage in point of power; because, cæteris paribus, as the motion is slower, the buckets must be made larger; and the wheel being more loaded with water, the stress upon every part of the work will be increased in proportion; "The best velocity for practice therefore will be such, as when the wheel here used made about 30 turns in a minute; that is, when the velocity of the circumference is a little more than 3 feet in a second."

Experience confirms, that this velocity of 3 feet in a second is applicable to the highest overshot wheels, as well as the lowest; and, all other parts of the work being properly adapted, will produce very nearly the greatest effect possible: however this also is certain from experience, that high wheels may deviate further from this rule, before they will lose their power, by a given aliquot part of the whole, than low ones can be admitted to do; for a wheel of 24 feet high may move at the rate of 6 feet per second without losing any considerable part of its power;* and, on the other hand, I have seen a wheel of 33 feet high, that has moved very steadily and well with a velocity but little exceeding 2 feet.

IV. Concerning the Load for an Overshot Wheel, in order that it may produce a Maximum.

The maximum load for an overshot wheel, is that which reduces the circumference of the wheel to its proper velocity; and this will be known, by dividing the effect it ought to produce in a given time by the space intended to be described by the circumference of the wheel in the same time: the quotient will be the resistance overcome at the circumference of the wheel; and is equal to the load required, the friction and resistance of the machinery included.

V. On the Greatest Possible Velocity of an Overshot Wheel.

The greatest velocity that the circumference of an overshot wheel is capable of, depends jointly on the diameter or height of the wheel, and the velocity of falling bodies; for it is plain, that the velocity of the circumference can never be

* The 24-feet wheel going at 6 feet in a second seems owing to the small proportion that the head (requisite to give the water the proper velocity of the wheel) bears to the whole height.

greater than to describe a semi-circumference, while a body let fall from the top of the wheel will descend through its diameter; nor indeed quite so great, as a body descending through the same perpendicular space cannot perform the same in so small a time when passing through a semi-circle, as would be done in a perpendicular line. Thus, if a wheel be 16 feet 1 inch high, a body will fall through the diameter in one second: this wheel therefore can never arrive at a velocity equal to the making one turn in 2 seconds; but, in reality, an overshot wheel can never come near this velocity; for when it acquires a certain speed, the greatest part of the water is prevented from entering the buckets; and the rest, at a certain point of its descent, is thrown out again by the centrifugal force. This appears to have been the case in the first 3 experiments of the foregoing specimen; but as the velocity, when this begins to happen, depends on the form of the buckets, as well as other circumstances, the utmost velocity of overshot wheels is not to be determined generally: and indeed it is the less necessary in practice, as it is in this circumstance incapable of producing any mechanical effect, for reasons already given.

VI. On the Greatest Load that an Overshot Wheel can Overcome.

The greatest load an overshot wheel will overcome, considered abstractly, is unlimited or infinite: for as the buckets may be of any given capacity, the more the wheel is loaded, the slower it turns; but the slower it turns, the more will the buckets be filled with water; and consequently, though the diameter of the wheel, and quantity of water expended, are both limited, yet no resistance can be assigned, which it is not able to overcome: but in practice we always meet with something that prevents our getting into infinitesimals; for when we really go to work to build a wheel, the buckets must necessarily be of some given capacity; and consequently such a resistance will stop the wheel, as is equal to the effort of all the buckets in one semi-circumference filled with water.

The structure of the buckets being given, the quantity of this effort may be assigned; but is not of much consequence to the practice, as in this case also the wheel loses its power; for though here is the exertion of gravity on a given quantity of water, yet being prevented by a counterbalance from moving, is capable of producing no mechanical effect, according to our definition. But, in reality, an overshot wheel generally ceases to be useful before it is loaded to that pitch; for when it meets with such a resistance as to diminish its velocity to a certain degree, its motion becomes irregular; yet this never happens till the velocity of the circumference is less than 2 feet per second, where the resistance is equable, as appears not only from the preceding specimen, but from experiments on larger wheels.

SCHOLIUM. Having now examined the different effects of the power of

water, when acting by its impulse, and by its weight, under the titles of undershot and overshot wheels; we might naturally proceed to examine the effects when the impulse and weight are combined, as in the several kinds of breast-wheels, &c. but, what has been already delivered being carefully attended to, the application of the same principles in these mixed cases will be easy, and reduce what is to be said on this head into a narrow compass: for all kinds of wheels where the water cannot descend through a given space, unless the wheel moves with it, are to be considered of the nature of an overshot wheel, according to the perpendicular height that the water descends from; and all those that receive the impulse or shock of the water, whether in an horizontal, perpendicular, or oblique direction, are to be considered as undershots. And therefore a wheel, which the water strikes at a certain point below the surface of the head, and after that descends in the arch of a circle, pressing by its gravity on the wheel; "the effect of such a wheel will be equal to the effect of an undershot, whose head is equal to the difference of level between the surface of the water in the reservoir and the point where it strikes the wheel, added to that of an overshot, whose height is equal to the difference of level, between the point where it strikes the wheel and the level of the tail-water." It is here supposed, that the wheel receives the shock of the water at right angles to its radii; and that the velocity of its circumference is properly adapted to receive the utmost advantage of both these powers; otherwise a reduction must be made on that account.

PART III.—*On the Construction and Effects of Windmill Sails.*

In trying experiments on windmill-sails, the wind itself is too uncertain to answer the purpose: we must therefore have recourse to an artificial wind. This may be done two ways; either by causing the air to move against the machine, or the machine to move against the air. To cause the air to move against the machine in a sufficient volume, with steadiness and the requisite velocity, is not easily practised: to carry the machine forward in a right line against the air, would require a larger room than could conveniently be met with. What is found most practicable therefore, is to carry the axis of the sails progressively round in the circumference of a large circle. On this idea* a machine was constructed as follows.

* Some years ago Mr. Rouse, an ingenious gentleman of Harborough in Leicestershire, set about trying experiments on the velocity of the wind, and force thereof upon plain surfaces and windmill-sails: and much about the same time Mr. Ellicott contrived a machine for the use of the late celebrated Mr. B. Robins, for trying the resistance of plain surfaces moving through the air. The machines of both these gentlemen were much alike, though at that time totally unacquainted with each other's inquiries. But it often happens, that when two persons think justly upon the same subject, their experiments are alike. This machine was also built upon the same idea as the foregoing; but differed in having the hand for the first mover, with a pendulum for its regulator,

In plate 13, fig. 12, ABC is a pyramidical frame for supporting the moving parts. DE is an upright axis, on which is framed FG, an arm for carrying the sails at a proper distance from the centre of the upright axis. H is a barrel on the upright axis, on which is wound a cord; which, being drawn by the hand, gives a circular motion to the axis, and to the arm FG; and so carries the axis of the sails in the circumference of a circle, whose radius is DI, thus causing the sails to strike the air, and turn round on their own axis.

At L is fixed the end of a small line, which passing through the pulleys M, N, O, terminates on a small cylinder or barrel on the axis of the sails, and, by winding on it, raises P the scale, where the weights are placed for trying the power of the sails. This scale, moving up and down in the direction of the upright axis, receives no disturbance from the circular motion.

QR two parallel pillars standing on the arm FG, for supporting and keeping steady the scale P; which is kept from swinging by means of ST two small chains, which hang loosely round the two pillars. W is a weight, for bringing the centre of gravity of the moveable part of the machine into the centre of motion of the axis DE.

VX is a pendulum, composed of 2 balls of lead, which are moveable on a wooden rod, and thus can be so adjusted, as to vibrate in any time required. This pendulum hangs on a cylindrical wire, on which it vibrates, as on a rolling axis. Y is a perforated table for supporting the axis of the pendulum.

The pendulum being so adjusted as to make 2 vibrations in the time that the arm FG is intended to make one turn; the pendulum being set a vibrating, the experimenter pulls by the cord Z, with sufficient force to make each half revolution of the arm to correspond with each vibration, as equal as possible, during the number of vibrations that the experiment is intended to be continued. A little practice renders it easy to give this motion with all the regularity that is necessary.

Specimen of a Set of Experiments.

Radius of the sails	21	inches.
Length of ditto in the cloth	18	
Breadth of ditto	5.6	
* { Angle at the extremity	10	degrees.
* { Ditto at the greatest inclination	25	

instead of a weight, as in the former; which was certainly best for the purposes of measuring the impulse of the wind, or resistance of plains: but the latter is more applicable to experiments on windmill-sails; because every change of position of the same sails will occasion their meeting the air with a different velocity, though urged by the same weight.—Orig.

* In all the following experiments, the angle of the sails is counted from the plane of their motion; that is, when they stand at right angles to the axis, their angle is denoted 0°, this notation being agreeable to the language of practitioners, who call the angle so denoted, the weather of the sail; which they denominate greater or less, according to the quantity of this angle.—Orig.

20 turns of the sails raised the weight........................ 11.3 inches.

Velocity of the centre of the sails, in the circumference of the great circle, in a second .... } 6 feet.

Continuance of the experiment .... 52 seconds.

N°	Wt. in the scale.	Turns.	Product.
1	0 lb.	108	0
2	6	85	510
3	$6\frac{1}{2}$	81	$526\frac{1}{2}$
4	7	78	546
5	$7\frac{1}{2}$	73	$547\frac{1}{2}$ max.
6	8	65	520
7	9	0	0

The weight of the scale and pulley was 3 oz. and 1 oz. suspended on one of the radii, at $12\frac{1}{2}$ inches from the centre of the axis, just overcame the friction, scale, and load of $7\frac{1}{2}$ lb. and placed at $14\frac{1}{2}\frac{9}{0}$ inches, overcame the same resistance with 9 lb. in the scale.

Reduction of the preceding Specimen.

N° 5 being taken for the maximum, the weight in the scale was 7 lb. 8 oz. which, with the weight of the scale and pulley 3 oz. makes 7 lb. 11 oz. equal to 123 oz.; this added to the friction of the machinery, the sum is the whole resistance.* The friction of the machinery is thus deduced: since 20 turns of the sails raised the weight 11.3 inches, with a double line, the radius of the cylinder will be .18 of an inch; but had the weight been raised by a single line, the radius of the cylinder being half the former, viz. .09, the resistance would have been the same: we shall therefore have this analogy; as half the radius of the cylinder, is to the length of the arm where the small weight was applied; so is the weight applied to the arm, to a 4th weight, which is equivalent to the sum of the whole resistance together; that is, .09 : 12.5 :: 1 oz. : 139 oz.; this exceeds 123 oz. the weight in the scale, by 16 oz. or 1 lb. which is equivalent to the friction; and which, added to the above weight of 7 lb. 11 oz. makes 8 lb. 11 oz. = 8.69 lb. for the sum of the whole resistance; and this, multiplied by 73 turns, makes a product of 634, which may be called the representative of the effect produced.

In like manner, if the weight 9 lb. which caused the sails to rest after being in motion, be augmented by the weight of the scale and its relative friction, it will become 10.37 lb. The result of this specimen is set down in N° 12 of table 3, and the result of every other set of experiments there contained, were made and reduced in the same manner.

* The resistance of the air is not taken into the account of resistance, because it is inseparable from the application of the power.—Orig.

TABLE III. *Containing Nineteen Sets of Experiments on Windmill Sails of various Structures, Positions, and Quantities of Surfaces.*

The kind of sails made use of.	No.	Angle at the extremities.	Greatest angle.	Turns of the sails unloaded.	Turns of ditto at the maximum	Load at the maximum.	Greatest load.	Product.	Quntity of surface.	Ratio of greatest velocity to the velocity at a maximum.	Ratio of greatest load to the load at maximum.	Ratio of surface to the product.
Plain sails at an angle of 55°	1	35°	35°	66	42	7.56 lb.	12.59 lb.	318	404sq.in.	10:7	10:6	10:7.9
Plain sails weathered according to the common practice.	2	12	12		70	6.3	7 56	441	404		10:8.3	10:10.1
	3	15	15	105	69	6.72	8.12	464	404	10:6.6	10:8.3	10:10.15
	4	18	18	96	66	7.0	9.81	462	404	10:7	10:7.1	10:10.15
Weathered according to Maclaurin's theorem.	5	9	26½		66	7.0		462	404			10:11.4
	6	12	29½		70½	7.35		518	404			10:12.8
	7	15	32½		63½	8.3		527	404			10:13.
Sails weathered in the Dutch manner, tried in various positions.	8	0	15	120	93	4.75	5.31	442	404	10:7.7	10:8.9	10:11
	9	3	18	120	79	7.0	8.12	553	404	10:6.6	10:8.6	10:13.7
	10	5	20		78	7.5	8.12	585	404		10:9.2	10:14.5
	11	7½	22½	113	77	8.3	9 81	639	404	10:6.8	10:8 5	10:15.8
	12	10	25	108	73	8.69	10.37	634	404	10:6.8	10:8.4	10:15.7
	13	12	27	100	66	8.41	10.94	580	404	10:6.6	10:7.7	10:14.4
Sails weathered in the Dutch manner, but enlarged towards the extremities.	14	7½	22½	123	75	10.65	12.59	799	505	10:6.1	10:8.5	10:15.8
	15	10	25	117	74	11.08	13.69	820	505	10:6 3	10:8.1	10:16.2
	16	12	27	114	66	12.09	14.23	799	505	10:5.8	10:8.4	10:15.8
	17	15	30	96	63	12.09	14.78	762	505	10:6.6	10:8.2	10:15.1
8 sails being sectors of ellipses in their best positions.	18	12	22	105	64½	16.42	27.87	1059	854	10:6.1	10:5.9	10:12.4
	19	12	22	99	64½	18.06		1165	1146	10:5.9		10:10.1
	1	2	3	4	5	6	7	8	9	10	11	12

Observations and Deductions from the preceding Experiments.

I. Concerning the best Form and Position of Windmill Sails.—In table 3, N° 1, is contained the result of a set of experiments on sails set at the angle which the celebrated Mons. Parent, and succeeding geometricians for many years, held to be the best, viz. those whose planes make an angle of 55° nearly with the axis; the complement of which, or angle that the plane of the sail makes with the plane of their motion, will therefore be 35°, as set down in col. 2 and 3. Now if we multiply their number of turns by the weight they lifted when working to the greatest advantage, as set down in col. 5 and 6, and compare this product (col. 8) with the other products contained in the same column, instead of being the greatest, it turns out the least of all the rest. But if we set the angle of the same planes at somewhat less than half the former, or at any angle from 15° to 18°, as in N° 3 and 4, that is, from 72° to 75° with the axis, the product will be increased in the ratio of 31 : 45; and this is the angle most commonly made use of by practitioners, when the surfaces of the sails are planes.

If nothing more was intended than to determine the most efficacious angle to make a mill acquire motion from a state of rest, or to prevent it from passing into rest from a state of motion, we shall find the position of N° 1 the best; for if we consult col. 7, which contains the least weights that would make the sails pass from motion to rest, we shall find that of N° 1, (relative to the quantity of cloth) the greatest of all. But if the sails are intended, with given dimensions, to produce the greatest effect possible in a given time, we must entirely reject those of N° 1; and if we are confined to the use of planes, conform ourselves to some angle between N° 3 and 4, that is, not less than 72°, nor greater than 75°, with the axis.

The late celebrated Mr. Maclaurin has judiciously distinguished between the action of the wind on a sail at rest, and a sail in motion; and, in consequence, as the motion is more rapid near the extremities than towards the centre, that the angle of the different parts of the sail, as they recede from the centre, should be varied. For this purpose he has furnished us with the following theorem.* 'Suppose the velocity of the wind to be represented by a, and the velocity of any given part of the sail to be denoted by c; then the effort of the wind on that part of the sail will be greatest, when the tangent of the angle in which the wind strikes it, is to radius, as $\sqrt{2 + \frac{9cc}{4aa}} + \frac{3c}{2a}$ to 1.' This theorem then assigns the law, by which the angle is to be varied according to the velocity of each part of the sail to the wind: but as it is left undetermined what velocity any one given part of the sail ought to have in respect to the wind, the angle that any one part of the sail ought to have, is left undetermined also; so that we are still at a loss for the proper data to apply the theorem. However, Mr. S. being willing to avail himself of it, and considering that any angle from 15° to 18° was best suited to a plane, and of consequence the best mean angle, he made the sail, at the middle distance between the centre and the extremity, to stand at an angle of 15° 41′ with the plane of the motion; in which case the velocity of that part of the sail, when loaded to a maximum, would be equal to that of the wind, or $c = a$. This being determined, the rest were inclined according to the theorem, as follows:

			Angle with the axis.	Angle of weather.	
Parts of the radius from the centre.	$\frac{1}{6}$	$c = \frac{1}{3}a$	63° 26′	26° 34′	
	$\frac{2}{6}$	$c = \frac{2}{3}a$	69 54	20 6	
	$\frac{1}{2}$	$c = a$	74 19	15 41	middle.
	$\frac{2}{3}$	$c = 1\frac{1}{3}a$	77 20	12 40	
	$\frac{5}{6}$	$c = 1\frac{2}{3}a$	79 27	10 33	
	1	$c = 2a$	81 0	9 0	extremity.

* Maclaurin's Account of Sir Isaac Newton's Philosophical Discoveries, p. 176, art. 29.—Orig.

1

The result was according to N° 5, being nearly the same as the plane sails, in their best position: but being turned round in their sockets, so that every part of each sail stood at an angle of 3°, and afterwards of 6°, greater than before, that is, their extremities being moved from 9° to 12° and 15°, the products were advanced to 518 and 527 respectively. Now from the small difference between those two products, we may conclude, that they were nearly in their best position, according to N° 7, or some angle between that and N° 6: but from these, as well as the plane sails and others, we may also conclude, that a variation in the angle of a degree or two makes very little difference in the effect, when the angle is near upon the best.

It is to be observed, that a sail inclined by the preceding rule will expose a convex surface to the wind: whereas the Dutch, and all our modern mill-builders, though they make the angle to diminish, in receding from the centre towards the extremity, yet constantly do it in such manner, as that the surface of the sail may be concave towards the wind. In this manner the sails made use of in N° 8, 9, 10, 11, 12, and 13, were constructed; the middle of the sail making an angle with the extreme bar of 12°; and the greatest angle (which was about $\frac{1}{3}$ of the radius from the centre) of 15° with it. Those sails being tried in various positions, the best appears to be that of N° 11, where the extremities stood at an angle of $7\frac{1}{2}$° with the plane of motion, the product being 639: greater than that of those made by the theorem in the ratio of 9 : 11, and double to that of N° 1; and this was the greatest product that could be procured without an augmentation of surface. Hence it appears, that "when the wind falls on a concave surface, it is an advantage to the power of the whole, though every part, taken separately, should not be disposed to the best advantage."

Having thus obtained the best position of the sails, or manner of weathering, as it is called by workmen, the next point was to try what advantage could be made by an addition of surface on the same radius. For this purpose, the sails made use of had the same weather as those N° 8 to 13, with an addition to the leading side of each of a triangular cloth, whose height was equal to the height of the sail, and whose base was equal to half the breadth: of consequence the increase of surface on the whole was a 4th part, or as 4 : 5. Those sails, by being turned round in their sockets, were tried in 4 different positions, specified in N° 14, 15, 16, and 17; whence it appears that the best was when every part of the sail made a greater angle by $2\frac{1}{2}$°, with the plane of the motion, than those without the addition, as appears by N° 15, the product being 820: this exceeds 639 more than in the ratio of 4 : 5, or that of the increase of cloth. Hence it appears, that "a broader sail requires a greater angle; and that when

the sail is broader at the extremity, than near the centre, this shape is more advantageous than that of a parallelogram.*

Many have imagined, that the more sail the greater the advantage, and have therefore proposed to fill up the whole area: and by making each sail a sector of an ellipsis, according to M. Parent, to intercept the whole cylinder of wind, and so to produce the greatest effect possible. We have therefore proceeded to inquire how far the effect could be increased by a further enlargement of the surface, on the same radius of which N° 18 and 19 are specimens. The surfaces indeed were not made planes, and set at an angle of 35°, as Parent proposed; because, from N° 1 we learn, that this position has nothing to do, when we intend them to work to the greatest advantage. We therefore gave them such an angle as the preceding experiments indicated for such sort of sails, viz. 12° at the extremity, and 22° for the greatest weather. By N° 18 we have the product 1059, greater than N° 15, in the ratio of 7 : 9; but then the augmentation of cloth is almost 7 : 12. By N° 19 we have the product 1165, that is greater than N° 15 as 7 : 10; but the augmentation of cloth is nearly as 7 : 16; consequently had the same quantity of cloth as in N° 18 been disposed in a figure similar to that of N° 15, instead of the product 1059, we should have had the product 1386; and in N° 19, instead of the product 1165, we should have had a product of 1860; as will be further made appear in the course of the following deductions. Hence it appears, that beyond a certain degree, the more the area is crowded with sail, the less effect is produced in proportion to the surface: and by pursuing the experiments still further, it was found, that though in N° 19 the surface of all the sails together were not more than $\frac{7}{8}$ of the circular area containing them, yet a further addition rather diminished than increased the effect. "So that when the whole cylinder of wind is intercepted, it does not then produce the greatest effect, for want of proper interstices to escape." It is certainly desirable, that the sails of windmills should be as short as possible; but at the same time it is equally desirable, that the quantity of cloth should be the least that may be, to avoid damage by sudden squalls of wind. The best structure therefore for large mills, is that where the quantity of cloth is the greatest, in a given circle, that can be: on this condition, that the effect holds out in proportion to the quantity of cloth; for otherwise the effect can be augmented in a given degree by a less increase of cloth on a larger radius,

* The figure and proportion of the enlarged sails, which Mr. S. found best to answer at large, are represented in the figure, where the extreme bar is $\frac{1}{3}$ of the radius (or whip, as it is called by the workmen,) and is divided by the whip in the proportion of 3 to 5. The triangular or leading sail is covered with board from the point downwards $\frac{1}{3}$ of its height, the rest with cloth as usual. The angles of weather in the preceding note are best for the enlarged sails also; for in practice it is found, that the sails had better have too little than too much weather.—Orig.

than would be required, if the cloth was increased on the same radius. The most useful figure therefore for practice, is that of N° 9 or 10, as has been experienced on several mills in large.

TABLE IV. *Containing the Result of 6 Sets of Experiments, made for determining the Difference of Effect, according to the Different Velocity of the wind.*

N. B. The sails were of the same size and kind as those of No. 10, 11, 12, Tab. IV. Continuance of the experiment one minute.

No.	Angle at the extremity.	Velocity of the wind in a second.	Turns of the sails unloaded.	Turns of the sails at maximum.	Load at the maximum.	Greatest load.	Product.	Maximum load for the half velocity.	Turns of the sails therewith.	Product of lesser load and greater velocity.	Ratio of the two products.	Ratio of the greatest velocity to the velocity at a maximum.	Ratio of the greatest load to the load at a maximum.
		l. m.			lb.	lb.							
1	5°	4 4½	96	66	4.47	5.37	295	—	—	—	— —	10 : 6.9	10 : 8.3
2	5	8 9	207	122	16.42	18.06	2003	4.47	180	805	10 : 27.3	10 : 5.9	10 : 9.1
3	7½	4 4½	—	65	4.62	—	300	—	—	—	— —	—	—
4	7½	8 9	—	130	17.52	—	2278	4.62	180	832	10 : 27.8	—	—
5	10	4 4½	91	61	5.03	5.87	307	—	—	—	— —	10 : 6.7	10 : 8.5
6	10	8 9	178	110	18.61	21.34	2047	5.03	158	795	10 : 26.	10 : 6.2	10 : 8.7
1	2	3	4	5	6	7	8	9	10	11	12	13	14

II. Concerning the Ratio between the Velocity of Windmill Sails Unloaded, and their Velocity when Loaded to a Maximum.

Those ratios, as they turned out in experiments on different kinds of sails, and with different inclinations, the velocity of the wind being the same, are contained in column 10 of tab. 3, where the extremes differ from the ratio of 10 : 7.7 to that of 10 : 5.8; but the most general ratio of the whole will be nearly as 3 : 2. This ratio also agrees sufficiently near with experiments where the velocity of the wind was different, as in those contained in tab. 4, col. 13, in which the ratios differ from 10 : 6.9 to that of 10 : 5.9. However, it appears in general, that where the power is greater, whether by an enlargement of surface, or a greater velocity of the wind, that the 2d term of the ratio is less.

III. Concerning the Ratio between the Greatest Load that the Sails will bear without Stopping, or, what is nearly the same thing, between the Least Load that will Stop the sails, and the Load at the Maximum.

Those ratios for different kinds of sails and inclinations, are collected in col. 11, tab. 3, where the extremes differ from the ratio of 10 : 6 to that of 10 : 9.2; but taking in those sets of experiments only, where the sails respectively an-

swered best, the ratios will be confined between that of 10 : 8 and of 10 : 9; and at a medium about 10 : 8.3 or of 6 : 5. This ratio also agrees nearly with those in col. 14 of tab. 4. However it appears, on the whole, that in those instances, where the angle of the sails or quantity of cloth was greatest, that the 2d term of the ratio was less.

IV. On the Effects of Sails, according to the Different Velocity of the Wind.

Maxim 1. The velocity of windmill sails, whether unloaded, or loaded so as to produce a maximum, is nearly as the velocity of the wind; their shape and position being the same.—This appears by comparing together the respective numbers of columns 4 and 5, tab. 4.

Maxim 2. The load at the maximum is nearly, but somewhat less than, as the square of the velocity of the wind; the shape and position of the sails being the same.—This appears by comparing together the numbers in col. 6 tab. 4.

Maxim 3. The effects of the same sails at a maximum are nearly, but somewhat less than, as the cubes of the velocity of the wind.—It has already been proved, Maxim 1st, that the velocity of sails at the maximum is nearly as the velocity of the wind; and by Maxim 2d, that the load at the maximum is nearly as the square of the same velocity: if those two maximums would hold precisely, it would be a consequence that the effect would be in a triplicate ratio thereof: how this agrees with experiment will appear by comparing together the products in col. 8 of tab. 4, wherein those of N° 2, 4, and 6, (the velocity of the wind being double) ought to be octuple of those of N° 1, 3, and 5, instead of which they fall short, N° 2 by $\frac{1}{7}$, N° 4 by $\frac{1}{20}$, and N° 6 by $\frac{1}{6}$ part of the whole.

Maxim 4. The load of the same sails at the maximum is nearly as the squares, and their effect as the cubes, of their number of turns in a given time.—This maxim may be esteemed a consequence of the three preceding.

Maxim 5. When sails are loaded so as to produce a maximum at a given velocity, and the velocity of the wind increases, the load continuing the same; 1st, The increase of effect, when the increase of the velocity of the wind is small, will be nearly as the squares of those velocities: 2dly, When the velocity of the wind is double, the effects will be nearly as 10 : $27\frac{1}{2}$: but, 3dly, When the velocities compared, are more than double of that where the given load produces a maximum, the effects increase nearly in a simple ratio of the velocity of the wind.

V. On the Effects of Sails of Different Magnitudes: the structure and position being similar, and the velocity of the wind the same.

Maxim 6. In sails of a similar figure and position, the number of turns in a given time will be reciprocally as the radius or length of the sail.

Maxim 7. The load at a maximum that sails of a similar figure and position will overcome, at a given distance from the centre of motion, will be as the cube of the radius.

Maxim 8. The effect of sails of similar figure and position, are as the square of the radius.

Corol. 1. Hence it follows, that augmenting the length of the sail, without augmenting the quantity of the cloth, does not increase the power; because what is gained by the length of the lever, is lost by the slowness of the rotation.

Corol. 2. If sails be increased in length, the breadth remaining the same, the effect will be as the radius.

VI. Concerning the velocity of the Extremities of Windmill Sails, in Respect to the Velocity of the Wind.

Maxim 9. The velocity of the extremities of Dutch sails, as well as of the enlarged sails, in all their usual positions when unloaded, or even loaded to a maximum, are considerably quicker than the velocity of the wind.

The Dutch sails unloaded, as in tab. 3, No. 8, made 120 revolutions in 52″: the diameter of the sails being 3 feet 6 inches, the velocity of their extremities will be 25.4 feet in a second; but the velocity of the wind producing it being 6 feet in the same time, we shall have 6 : 25.4 :: 1 : 4.2; in this case therefore, the velocity of their extremities was 4.2 times greater than that of the wind. In like manner, the relative velocity of the wind, to the extremities of the same sails, when loaded to a maximum, making then 93 turns in 52″, will be found to be as 1 : 3.3; or 3.3 times quicker than that of the wind.

The following table contains 6 examples of Dutch sails, and 4 examples of the enlarged sails, in different positions, but with the constant velocity of the wind of 6 feet in a second, from table 3: and also 6 examples of Dutch sails in different positions, with different velocities of the wind, from table 4.

TABLE V. *Containing the Ratio of the Velocity of the Extremities of Windmill sails to the Velocity of the Wind.*

N°	N° of tab. 3 and 4.	Angle at the extremity.	Velocity of the wind in a second.	Ratio of the velocity of the wind and extremities of the sails. unloaded.	loaded.	
1	8	0°	6^{f} 0^{in}	1:4.2	1:3.3	From table 3.
2	9	3	6 0	1:4.2	1:2.8	
3	10	5	6 0	— — —	1:2.75	
4	11	$7\frac{1}{2}$	6 0	1:4	1:2.7	
5	12	10	6 0	1:3.8	1:2.6	
6	13	12	6 0	1:3.5	1:2.3	
7	14	$7\frac{1}{2}$	6 0	1:4.3	1:2.6	
8	15	10	6 0	1:4.1	1:2.6	
9	16	12	6 0	1:4	1:2.3	
10	17	15	6 0	1:3.35	1:2.2	
11	1	5	4 $4\frac{1}{2}$	1:4.	1:2.8	From table 4.
12	2	5	8 9	1:4.3	1:2.6	
13	3	$7\frac{1}{2}$	4 $4\frac{1}{2}$	— — —	1:2.8	
14	4	$7\frac{1}{2}$	8 9	— — —	1:2.7	
15	5	10	4 $4\frac{1}{2}$	1:3.8	1:2.6	
16	6	10	8 9	1:3.4	1:2.3	
1	2	3	4	5	6	

It appears from the preceding collection of examples, that when the extremities of the Dutch sails are parallel to the plane of motion, or at right angles to the wind, and to the axis, as they are made according to the common practice in England, that their velocity, unloaded, is above 4 times, and loaded to a maximum, above 3 times greater than that of the wind: but that when the Dutch sails, or enlarged sails, are in their best positions, their velocity unloaded is 4 times, and loaded to a maximum, at a medium the Dutch sails are 2.7, and the enlarged sails 2.6 times greater than the velocity of the wind. Hence we are furnished with a method of knowing the velocity of the wind, from observing the velocity of the windmill sails; for knowing the radius, and the number of turns in a minute, we shall have the velocity of the extremities; which, divided by the following divisors, give the velocity of the wind.

Dutch sails in their common position unloaded 4.2, loaded 3.3.

Dutch sails in their best position unloaded 4.0, loaded 2.7.

Enlarged sails in their best position unloaded 4.0, loaded 2.6.

From the above divisors there arise the following compendiums; supposing the radius to be 30 feet, which is the most usual length in this country, and the mill to be loaded to a maximum, as is usually the case with corn-mills; for every 3 turns in a minute, of the Dutch sails in their common position, the wind will move at the rate of 2 miles an hour; for every 5 turns in a minute, of

the Dutch sails in their best position, the wind moves 4 miles an hour; and for every 6 turns in a minute, of the enlarged sails in their best position, the wind will move 5 miles an hour.

The annexed table, which was communicated by Mr. Rouse, and which appears to have been constructed with great care, from a considerable number of facts and experiments, and which having relation to the subject of this article, is here inserted as he sent it; but at the same time it must be observed, that the evidence for those numbers where the velocity of the wind exceeds 50 miles an hour, do not seem of equal authority with those of 50 miles an hour and under. It is also to be observed, that the numbers in col. 3 are calculated according to the square of the velocity of the wind, which, in moderate velocities, from what has been before observed, will hold very nearly.

TABLE VI. *Containing the Velocity and Force of Wind, according to their common Appellations.*

Velocity of the wind. Miles in one hour.	Velocity of the wind. Feet in one second.	Perpendicular force on one foot area in pounds avoirdupois.	Common appellations of the force of winds.
1	1.47	.005	Hardly perceptible.
2	2.93	.020	Just perceptible.
3	4.40	.044	
4	5.87	.079	Gentle pleasant wind.
5	7.33	.123	
10	14.67	.492	Pleasant brisk gale.
15	22.00	1.107	
20	29.34	1.968	Very brisk.
25	36.67	3.075	
30	44.01	4.429	High winds.
35	51.34	6.027	
40	58.68	7.873	Very high.
45	66.01	9.963	
50	73.35	12.300	A storm or tempest.
60	88.02	17.715	A great storm.
80	117.36	31.490	A hurricane.
100	146.70	49.200	A hurricane that tears up trees, carries buildings before it, &c.
1	2	3	

VII. Concerning the Absolute Effect, produced by a Given Velocity of the Wind, on Sails of a Given Magnitude and Construction.

It has been observed by practitioners, that in mills with Dutch sails in the common position, that when they make about 13 turns in a minute, they then work at a mean rate, that is, by the compendiums in the last article, when the velocity of the wind is $8\frac{2}{3}$ miles an hour, or $12\frac{2}{3}$ feet in a second; which, in common phrase, would be called a fresh gale. The experiments in tab. 4, N° 4, were tried with a wind, whose velocity was $8\frac{3}{4}$ feet in a second; consequently had those experiments been tried with a wind, whose velocity was $12\frac{2}{3}$ feet in a second, the effect, by maxim 3d, would have been 3 times greater; because the cube of $12\frac{2}{3}$ is 3 times greater than that of $8\frac{3}{4}$.

From tab. 4, N° 4, we find, that the sails, when the velocity of the wind was $8\frac{3}{4}$ feet in a second, made 130 revolutions in a minute, with a load of 17.25 lb. From the measures of the machine, preceding the specimen of a set of experi-

ments, we find, that 20 revolutions of the sails raised the scale and weight 11.3 inches; 130 revolutions will therefore raise the scale 73.45 inches, which multiplied by 17.52 lb. makes a product of 1287, for the effect of the Dutch sails in their best position; that is, when the velocity of the wind is $8\frac{3}{4}$ feet in a second; this product therefore multiplied by 3, gives 3861 for the effect of the same sails, when the velocity of the wind is $12\frac{2}{3}$ feet in a second.

Desaguliers makes the utmost power of a man, when working so as to be able to hold it for some hours, to be equal to that of raising a hogshead of water 10 feet high in a minute. Now, a hogshead consisting of 63 ale gallons, being reduced into pounds avoirdupois, and the height into inches; the product made by multiplying those two numbers will be 76800; which is 19 times greater than the product of the sails last-mentioned, at $12\frac{1}{2}$ feet in a second: therefore, by maxim 8th, if we multiply the square root of 19, that is 4.46, by 21 inches, the length of the sail producing the effect 3861, we shall have 93.66 inches, or 7 feet $9\frac{2}{3}$ inches, for the radius of a Dutch sail in its best position, whose mean power shall be equal to that of a man: but if they are in their common position, their length must be increased in the ratio of the square root of 442 to that of 639, as thus appears:

The ratio of the maximum products of N° 8 and 11, tab. 3, are as 442 : 639; but by maxim 8, the effects of sails of different radii are as the square of the radii: consequently the square roots of the products or effects, are as the radii simply: and therefore as the square root of 442 is to that of 639, so is 93.66 to 112.66, or 9 feet $4\frac{2}{3}$ inches.

If the sails be of the enlarged kind, then from tab. 3, N° 11 and 15, we shall have the square root of 820 to that of 639 :: 93.66 : 82.8 inches, or 6 feet $10\frac{3}{4}$ inches: so that in round numbers we shall have the radius of a sail, of a similar figure to their respective models, whose mean power shall be equal to that of a man, viz.

The Dutch sails in their common position.......... $9\frac{1}{2}$ feet.
The Dutch sails in their best position 8
The enlarged sails in their best position............ 7

Suppose now the radius of a sail to be 30 feet, and to be constructed on the model of the enlarged sails, N° 14 or 15, tab. 3; dividing 30 by 7, we have 4.28, the square of which is 18.3; and this, according to maxim 7, will be the relative power of a sail of 30 feet, to one of 7 feet; that is, when working at a mean rate, the 30 feet sail will be equal to the power of 18.3 men, or of $3\frac{2}{3}$ horses: reckoning 5 men to a horse; whereas the effect of the common Dutch sails, of the same length, being less in the proportion of 820 : 442, will be scarcely equal to the power of 10 men, or of 2 horses.

That these computations are not merely speculative, but will nearly hold good

when applied to works in large, I have had an opportunity of verifying: for in a mill with the enlarged sails of 30 feet, applied to the crushing of rape seed, by means of two runners on the edge, for making oil; I observed, that when the sails made 11 turns in a minute, in which case the velocity of the wind was about 13 feet in a second, according to article 6th, that the runners then made 7 turns in a minute: whereas 2 horses, applied to the same 2 runners, scarcely worked them at the rate of $3\frac{1}{2}$ turns in the same time. Lastly, with regard to the real superiority of the enlarged sails, above the Dutch sails, as commonly made, it has sufficiently appeared, not only in those cases where they have been applied to new mills, but where they have been substituted instead of the others.

VIII. On Horizontal Windmills and Water-wheels, with Oblique Vanes.

Observations on the effects of common windmills with oblique vanes, have led many to imagine, that could the vanes be brought to receive the direct impulse, like a ship sailing before the wind, it would be a very great improvement in point of power; while others, attending to the extraordinary and even unexpected effects of oblique vanes, have been led to imagine, that oblique vanes applied to water-mills, would as much exceed the common water-wheels, as the vertical windmills are found to have exceeded all attempts towards a horizontal one. Both these notions, but especially the first, have so plausible an appearance, that of late years there has seldom been wanting those, who have assiduously employed themselves to bring to bear designs of this kind: it may not therefore be unacceptable to endeavour to set this matter in a clear light.

In pl. 13, fig. 13, let AB be the section of a plane, on which let the wind blow in the direction CD, with such a velocity as to describe a given space BE, in a given time, suppose 1 second; and let AB be moved parallel to itself, in the direction CD. Now, if the plane AB moves with the same velocity as the wind, that is, if the point B moves through the space BE in the same time that a particle of air would move through the same space; it is plain that in this case there can be no pressure or impulse of the wind on the plane: but if the plane moves slower than the wind, in the same direction, so that the point B may move to F, while a particle of air, setting out from B at the same instant, would move to E, then BF will express the velocity of the plane, and the relative velocity of the wind and plane will be expressed by the line FE. Let the ratio of FE to BE be given, suppose 2 : 3; let the line AB represent the impulse of the wind on the plane AB, when acting with its whole velocity BE; but when acting with its relative velocity FE, let its impulse be denoted by some aliquot part of AB, as for instance $\frac{4}{9}$AB: then will $\frac{4}{9}$ of the parallelogram AF represent the mechanical power of the plane; that is, $\frac{4}{9}\text{AB} \times \frac{1}{3}\text{BE}$.

2dly, Let IN be the section of a plane, inclined in such a manner, that the

base IK, of the rectangle triangle IKN, may be equal to AB; and the perpendicular NK = BE; let the plane IN be struck by the wind, in the direction LM, perpendicular to IK: then, according to the known rules of oblique forces, the impulse of the wind on the plane IN, tending to move it according to the direction LM, or NK, will be denoted by the base IK; and that part of the impulse, tending to move it according to the direction IK, will be expressed by the perpendicular NK. Let the plane IN be moveable in the direction of IK only; that is, the point I in the direction of IK, and the point N in the parallel direction NQ. Now it is evident, that if the point I moves through the line IK, while a particle of air, setting forwards at the same time from the point N, moves through the line NK, they will both arrive at the point K at the same time; and consequently, in this case also, there can be no pressure or impulse of the particle of the air on the plane IN. Now let IO be to IK as BF to BE; and let the plane IN move at such a rate, that the point I may arrive at O, and acquire the position OQ, in the same time that a particle of wind would move through the space NK: as OQ is parallel to IN, by the properties of similar triangles, it will cut NK in the point P, in such a manner, that NP = BF, and PK = FE: hence it appears, that the plane IN, by acquiring the position OQ, withdraws itself from the action of the wind, by the same space NP, that the plane AB does by acquiring the position FG; and consequently, from the equality of PK to FE, the relative impulse of the wind PK, on the plane OQ, will be equal to the relative impulse of the wind FE, on the plane FG: and since the impulse of the wind on AB, with the relative velocity FE, in the direction BE, is represented by $\frac{4}{9}$AB; the relative impulse of the wind on the plane IN, in the direction NK, will in like manner be represented by $\frac{4}{9}$IK; and the impulse of the wind on the plane IN, with the relative velocity PK, in the direction IK, will be represented by $\frac{4}{9}$NK; consequently the mechanical power of the plane IN, in the direction IK, will be $\frac{4}{9}$ the parallelogram IQ: that is $\frac{1}{3}$IK × $\frac{4}{9}$NK: that is, from the equality of IK = AB and NK = BE, we shall have $\frac{4}{9}$IQ = $\frac{1}{3}$AB × $\frac{4}{9}$BE = $\frac{4}{9}$AB × $\frac{1}{3}$BE = $\frac{4}{9}$ of the area of the parallelogram AF. Hence we deduce this.

General Proposition.—“ That all planes, however situated, that intercept the same section of the wind, and having the same relative velocity, in regard to the wind, when reduced into the same direction, have equal powers to produce mechanical effects.”—For what is lost by the obliquity of the impulse is gained by the velocity of the motion. Hence it appears, that an oblique sail is under no disadvantage in respect of power, compared with a direct one; except what arises from a diminution of its breadth, in respect to the section of the wind: the breadth IN being by obliquity reduced to IK.

The disadvantage of horizontal windmills therefore does not consist in this; that each sail; when directly exposed to the wind, is capable of a less power,

than an oblique one of the same dimensions; but that in a horizontal windmill, little more than one sail can be acting at once: whereas in the common windmill, all the 4 act together: and therefore, supposing each vane of a horizontal windmill, of the same dimensions as each vane of the vertical, it is manifest that the power of a vertical mill with 4 sails, will be 4 times greater than the power of the horizontal one, let its number of vanes be what it will: this disadvantage arises from the nature of the thing; but if we consider the further disadvantage, that arises from the difficulty of getting the sails back again against the wind, &c. we need not wonder if this kind of mill is in reality found to have not above $\frac{1}{8}$ or $\frac{1}{10}$ of the power of the common sort; as has appeared in some attempts of this kind.

In like manner, as little improvement is to be expected from water mills with oblique vanes: for the power of the same section of a stream of water, is not greater when acting on an oblique vane, than when acting on a direct one: and any advantage that can be made by intercepting a greater section, which sometimes may be done in the case of an open river, will be counterbalanced by the superior resistance that such vanes would meet with, by moving at right angles to the current: whereas the common floats always move with the water nearly in the same direction.

Here it may reasonably be asked, that since our geometrical demonstration is general, and proves that one angle of obliquity is as good as another; why in our experiments it appears, that there is a certain angle which is to be preferred to all the rest? It is to be observed, that if the breadth of the sail IN be given, the greater the angle KIN, the less will be the base IK: that is, the section of wind intersected, will be less: on the other hand, the more acute the angle KIN, the less will be the perpendicular KN: that is, the impulse of the wind, in the direction IK being less, and the velocity of the sail greater; the resistance of the medium will be greater also. Hence therefore, as there is a diminution of the section of the wind intercepted on one hand, and an increase of resistance on the other, there is some angle, where the disadvantage arising from these causes upon the whole is the least of all: but as the disadvantage arising from resistance is more of a physical than geometrical consideration, the true angle will best be assigned by experiment.

XIX. On the Remarkable Alteration of Colour in a Negro Woman. By Mr. James Bate, Surgeon in Maryland. p. 175.

Frank, a cook-maid in Col. Barnes's family, a native of Virginia, about 40 years of age, remarkably healthy, of a strong and robust constitution, had her skin originally as dark as that of the most swarthy African; but about 15 years before, that membrane in the parts next adjoining to the finger nails, became

white. Her mouth soon underwent the same change, and the phenomenon had since continued gradually to extend itself over the whole body; so that every part of its surface became more or less the subject of this surprising alteration. At the above date, 4 parts in 5 of the skin were white, smooth, and transparent, as in a fair European, elegantly showing the ramifications of the subjacent blood-vessels: the parts remaining sooty, daily lost their blackness, and in some measure partook of the prevailing colour; so that a very few years would, in all probability, induce a total change. The neck and back, along the course of the vertebræ, maintained their pristine hue the most, and in some spots proclaim their original state: the head, face, and breast, with belly, legs, arms, and thighs, were almost wholly white; the pudenda and axillæ party-coloured; the the skin of these parts, as far as white, being covered with white hair; where dark, with black. Her face and breast, as often as the passions anger, shame, &c. had been excited in her, had been immediately observed to glow with blushes; as also when, in pursuance of her business, she had been exposed to the action of the fire on these parts, some freckles had made their appearance.

The young woman declared, that excepting about 17 years before when she was delivered of a child, she had never been afflicted by any complaint of 24 hours continuance, and that she never remembered the catamenia to have been either irregular or obstructed, only during this pregnancy: she had never been subject to any cutaneous disorders, nor made use of any external applications, by which this phenomenon might be produced. The effects of the bile on the skin are well known to physicians, and had given rise to an opinion, that its colour was determined by it: but for his part he could not believe it had any thing to do here, since, from all the circumstances he had been able to collect, he could not find the least reason to suspect that this fluid, whether cystic or hepatic, had undergone any alteration. As ustion is known to make the skin of negroes become white, and as she was daily employed in the businesses of cookery, it might perhaps be supposed the effect of heat: but this could never be the case, as she had ever been well clad; and the change was as obvious in the parts protected from the action of that element, as in those most exposed to it. As an emunctory, the skin seemed to perform its office as well as possible, the sweat with the greatest freedom indifferently pervading the black and white parts. The effects of a blister he was yet a stranger to, as that which he applied on the outside of the arm did not answer the intended purpose: whether this was owing to its being laid on a part too much exposed, or that the corpus reticulare being destroyed, there might be such an adhesion of the cuticle to the cutis, as might render them inseparable, a 2d experiment must determine.

XX. Of a Paralytic Patient cured by an Electrical Application, in a Letter from Dr. Himsel, at Riga, to Jacob de Castro Sarmento, M. D., F. R. S. Translated from the French. p. 179.

Dr. H. here states, that a young man aged 20 recovered, by means of electricity, the use of his right arm, which had been affected with palsy for the space of 15 years, all the fingers of the paralytic hand were disabled, and the hand was so bent towards the elbow, as to form a right angle. But after being subjected to electrical shocks and sparks from the 10th of March to the 27th of April, he could extend and contract his fingers at pleasure, could move his arm backwards and forwards, and raise a 40 lb. weight to the height of three feet from the ground. He could also write his name, which he had not been capable of doing for 15 years before.

XXI. On some Observations relating to the Production of the Terra Tripolitana, or Tripoli. By Martin Hubner, F. R. S. Professor of History in the University of Copenhagen, and Member of the Royal Academy of Inscriptions and Belles Lettres of Paris. Translated from the French, by Emanuel Mendes Da Costa, F.R.S. p. 186.

Mr. H. endeavours to prove that the terra tripolitana or tripoli, is only a wood wholly petrified, and afterwards calcined by subterraneous fire.

Remarks on the preceding Paper. By Mr. Emanuel Mendes Da Costa, F. R. S. p. 192.

Mr. Da Costa remarks, that it is not improbable but that some of Mr. Hubner's tripoli might have been produced from the petrified wood he found in the mountain de Poligné, in Brittany ; and the whole account is then reduced to this only circumstance, that the layers of fossil wood in this mountain, having been saturated with the tripoline particles, which likewise abound in the same mountain, thereby composed a stone, or third body ; and that afterwards, these tripoline particles were again reduced, by the effects of a subterraneous fire, to their pristine state ; the force of the fire destroying the compages of the third body, or stone.

XXII. A Remarkable Case of an Empyema. By Mr. Joseph Warner, F. R. S., and Surgeon to Guy's Hospital. p. 194.

May be consulted in the collection of this author's surgical works.

XXIII. Extracts of some Letters from Signor Abbate de Venuti, F. R. S. to J. Nixon, A. M. and F. R. S., relating to several Antiquities lately discovered in Italy. p. 201.

[In these letters there is nothing sufficiently interesting for republication or abridgement.]

XXIV. *Experiments relating to the Preservation of Seeds. By John Ellis, Esq., F.R.S.* p. 206.

In order to send abroad a supply of cork acorns in a growing state, Mr. E. tried the following experiments on them to preserve them sound. He tried the very same experiments, at the same time, on a parcel of fresh oak acorns, which he collected at Sydenham in Kent, the latter end of last October, and had since kept them by him in a box in a warm room, it may give some insight into what may be the fate of those sent abroad. The experiments were made between the 25th and 30th of October 1758; and the acorns cut open to see the effects, Jan. 17, 1759.

Exper. 1.—Acorns of the English oak smeared over several times with a strong solution of gum arabic; and also they had been dried in a window, folded in a piece of paper, and put into a deal box. When these were cut open, they appeared hard, dry, and inclining to black, being quite perished.

Exper. 2.—Some acorns treated as in the first experiment, were wrapped up in papers, soaked in a strong solution of gum arabic, each in a separate paper: after they had been dried, they were put in the box with the rest. These were somewhat softer than the first, but decayed.

Exper. 3.—Some of them were smeared several times over with gum senega; and when they were dried in the window, and well hardened, were put in a paper into the deal box. These looked rather better than the two former parcels; but unfit for vegetation.

Exper. 4.—Some of the same acorns were put into the middle of a cake of plasterers stiff loam, or such as the brewers use to stop their beer barrels, and covered over near an inch on every side. This soon became dry, without any cracks: it was about 2½ inches thick; and was placed with the rest, wrapped up in a paper, in the box. The kernels of these were shrivelled up, and grown quite dry and hard, like horn, the loam proving a strong absorbent.

Exper. 5.—Some were rolled up separately in thin flakes of bees-wax warmed, to make it pliable, and put it in paper in the box. These looked very well when they were cut asunder, and appeared likely to grow; but were a little shrunk.

Exper. 6.—Some were rolled separately in rosin, made pliable with warmth. These cut quite fresh.

Exper. 7.—Some of them were rolled, each in a thin covering of a mixture of pitch, rosin, and bees-wax, called mummy by the gardeners. These cut as well, and looked as fresh as if they had just fallen from the tree.

The cork acorns, sent to Georgia, were inclosed in the same substances with the foregoing, and put into a box filled with dry sand, quite full, and well fastened: this was put into a tight cask, among papers and wearing apparel, and stowed in the upper part of the hold of the ship.

While making these experiments, Mr. E. wrote to Dr. Linneus, of Upsal, for his opinion of them, and for his method of preserving seeds in long voyages, His answer considers the great danger that attends seeds in warm voyages, in the same light with governor Ellis, and he communicated a very probable method of preserving seeds in long voyages, which, he says, has never failed. The following is an extract of his letter, dated the 8th of Dec. 1758, from Upsal. "Seeds may be brought from abroad in a growing state, if we attend to the following method:" "Put the seeds into a cylindrical glass bottle, and fill up the interstices with dry sand, to prevent their lying too close together, and that they may perspire freely through the sand; then cork the bottle, or tie a bladder over the mouth of it. Prepare a glass vessel, so much larger than that which contains the seeds, that when it is suspended in it, there may be a vacant space on all sides of about 2 inches distance between both glasses, for the following mixture; 4 parts of nitre, and one-5th part of equal parts, of common salt, and sal ammoniac: these must be well pounded, and mixed together, and the spaces all round between the outer and inner glasses well filled with it. This saline mass, which should be rather moist, will always be so cold, that the seeds in the inner glass will never suffer, during their voyage, from the heat of the air. This experiment has been tried, and has not failed."

As to the acorns before-mentioned, sent to Georgia, on the 27th of Nov. 1758, Mr. E. prepared 7 parcels of the acorns of the cork-bearing oak or ilex, in the following manner:

N° 1, 15 acorns, each covered over singly with a stiff solution of gum arabic, and afterwards rolled up in gumed paper. N° 2, 13 do. each rolled up in a thin cover of common yellow bees-wax, softened before the fire, and rolled up afterwards separately, in white paper. N° 3, 10 do. each rolled up as before, in wax, and afterwards each covered with a coat of brewers loam moistened with a thick solution of gum arabic. N° 4, 5 do. each coated with gum arabic, and afterwards with whiting moistened with a thick solution of gum arabic. N° 5, 25 do. each coated with gum arabic, and afterwards with brewers loam moistened with a thick solution of gum arabic. N° 6, 3 do. each covered with gardeners grafting mummy, consisting of a mixture of bees-wax, rosin, and pitch. N° 7, 10 do. each covered with fullers earth made into a paste, with a stiff solution of gum arabic.

These 7 parcels were all put into chip boxes, filled with dry house-sand, and afterwards put into a tight cask; and they arrived in Georgia in April following. Governor Ellis, in his letter, dated from thence, May 6, 1759, says, of all these experiments, none succeeded but the parcel N° 3. which had first been covered with bees-wax, and afterwards with a paste made of loam and dissolved gum arabic. We even find, that those that were covered with a thin coat of

bees-wax, and afterwards with paper, did not succeed; as their covering was not thick enough to keep in their perspiration.

The chestnut, next to the acorn, being the most difficult to preserve sound during the course of one season, or a whole year, on the 23d of February last, 1759, Mr. E. procured a parcel of Spanish chestnuts, just as they were imported, many of which were sounder than they generally are so late in the season: these he divided into 4 parcels, and put each parcel into a small earthen jar, involving them in the following substances:

Jar N° 1. 12 Chestnuts in mutton suet.
2. 12 do. . . . . . in bees-wax and mutton suet, equal quantities.
3. 12 do. . . . . . in bees-wax.
4. 12 do. . . . . . in bees-wax and yellow rosin, equal quantities.

These substances he melted: but did not pour them among the chestnuts, till he could bear his finger in them without the least sensible uneasiness, which he considered as the proper test not to effect the kernels by their heat, and immediately immersed the jar to the brim in cold water.

To examine the effects of these experiments, and to lay before the Society a fair account of them, Mr. E. broke all the jars on the 22d of November last, and found, that jar N° 1, which contained the chestnuts immersed in mutton suet, proved all rotten, attended with a very disagreeable putrid smell. Those in jar N° 2, were most of them sound and fresh, and their kernels as white and sweet-tasted, as when fresh gathered. These were inclosed in half bees-wax, and half mutton suet, melted together. Those in jar N° 3, were equally sound and well tasted, and had been inclosed in bees-wax only. Those in jar N° 4, which were inclosed in half bees-wax and half yellow rosin, were all turned soft and spongy, of a brown colour, and a most disagreeable taste and smell, from the resinous steams they had imbibed.

On the 24th of November last, Mr. E. planted 6 of the chestnuts preserved in wax and suet, N° 2, and 6 of those preserved in wax only, N° 3, in two garden pots, and placed them in a very spacious conservatory, where many of them are already germinating; which proves this method of preserving the larger seeds a very proper one to recommend to gentlemen that go to China, and other parts of the East Indies, to preserve many kinds of valuable seeds in a state of vegetation during a voyage of a whole year, till they arrive here; and probably till they are carried to our settlements in the American colonies.

It remains then for gentlemen who go to the East Indies, to place the seeds they preserve in bees-wax, or bees-wax and suet, in the coolest part of the ship, to prevent these substances being affected with the heat of those parts, which far exceeds ours. Perhaps Linneus's method of inclosing them in a larger vessel, and surrounding them with a mixture of salts will answer this end.

Small seeds, in their pods, may be preserved by being placed thinly on pieces of paper, cotton or linen cloth, that have been dipped in wax, then rolled up tight, and well secured from air by a further covering of wax.

XXV. Of a Very Long Suppression of Urine. By A. Dawson, M.D. p. 215.

R. W. aged 23 years, tall and well made, was seized, in the year 1755, with a weakness of one side, which soon went off, leaving only one knee weak and swelled; for which she was admitted into St. George's Hospital. April 4, 1756, she had a stoppage of urine, and felt no disposition to make any for 2 days. During the whole month of April, the discharge of urine was very irregular, it having ceased at one time for 5 days, and at another time for 9 days. When the catheter was introduced, little or no urine was found in the bladder.

In order to relieve her, she had been directed to use both the warm and cold bath, bleeding, purging, turpentine clysters, and a strong infusion of pareira brava, without any success: cantharides had likewise been given her to the quantity of 3 grs. in a dose; and this dose had been repeated 4 times in 24 hours; but without any other effect than that of occasioning pains in her throat, stomach, and bowels, with vomiting and purging. A wet towel put round her waist seemed to do the most good, and brought away some water 2 or 3 times; but afterwards ceased to have this effect.

From April 26, 1756, she made no water, and felt no want of making it for many months; yet all this time she could eat once or twice a day, and was able to walk and ride. She was sparing in the use of liquids; she had but little sleep, no sweats, and her skin had no urinous smell: her breathing was often very laborious, with a dry cough: the catamenia were irregular: there were oedematous swellings in her limbs, abdomen, hips, and face; but by the help of purges, and spontaneous vomitings, which began in the 3d month of the suppression, these swellings were tolerably kept under. She vomited sometimes every day, and sometimes only every 3d or 4th day; and though these vomitings usually came on presently after dinner, yet what she vomited seemed to be mere urine, without any thing which she had eaten mixed with it. In the beginning of June 1757, the nipples of her breasts chopped, and discharged some times a watery humour, sometimes a thick matter, streaked with blood, and sometimes a humour approaching to the colour of urine: all these discharges had a urinous scent, and seemed to lessen her urinous vomitings, which from this time became less frequent: but her legs, and especially her body, swelled to an extraordinary size, and she breathed with the utmost difficulty.

August 1 and 2, 1757, after a total suppression of urine for above a year and 3 months, she felt uncommon pricking pains, with great heat all down her back and loins, and about the belly and groin. On the 2d day she voided about 3 oz.

of thick, slimy matter, attended with sharp pains in the urinary passages: this water was not high coloured. The next day, she made water of a truly urinous kind with less pain; and continued to make a little water every day, the pain daily decreasing; and on the 7th she voided about a pint. Afterwards she had often a suppression of urine for 10 or 14 days; and once for 2 months, during which she had no vomiting; but her body was very much swelled. In July 1759, according to her own account, she did not usually make above half a pint in 24 hours; and sometimes scarcely so much in 2 days. The catamenia were then irregular; her sleep short, and disturbed; she had very little appetite; her legs swelled; and the rest of her body wasted.

XXVI. Several Accounts of the Fiery Meteor, which appeared on Sunday Nov. 26, 1758, between 8 and 9 at Night. Collected by John Pringle, M.D., F.R.S. p. 218.

Dr. Pringle first states the collection of observations as sent to him, and then makes his remarks on them. The observations abridged are as follow:

1. Mr. Mudge, of Plymouth, says he thinks he can venture positively to say the meteor was not seen at Plymouth. Besides a very minute and particular scrutiny among the people of the town, as he was apprehensive the narrowness of the streets, and height of the houses, might have been the cause of their not observing it, the lieutenant-governor was so obliging, at his request, to send a serjeant to inquire of every soldier in the garrison: and as some of them must have been on centinel duty that evening, if the meteor had appeared above their horizon, it could not have escaped them, as the garrison is situated on an eminence, and the prospect bounded by the sky only. Dr. Huxham also stated that he did not believe the meteor had been seen at Plymouth.

2. The Rev. Dr. Shipley, minister of Silchester in Hampshire, a parish about 45 miles w.s.w. of London, said, ‘that he had not a view of the meteor himself, but had conversed with 3 countrymen, his parishioners, who had seen it: that they had all agreed in observing the light to be greater than that of moonshine; and one of them in particular said it was so great that he could easily have seen a pin lying on the ground: that the body at first was like a large shooting star, but with a slower motion: that its direction was northerly: that during its progress it increased in size, leaving a stream of light behind; and at last, as it declined to the horizon, its lower part became in appearance as broad as his hand, while the length of the whole seemed to be about 5 feet, of a conical figure, ending in a point upwards: that before it reached the horizon, it burst into a flame, resembling a flash of lightning, and then immediately disappeared. Dr. Shipley going to the spot where the observer had stood, and making him point to some trees at a distance, over which he said the meteor disappeared, the Dr.

found, by taking the altitude with an instrument, that it had been extinguished about $1^{\circ}\ 15'$ above the horizon, then bearing 35° westward of the north.*

3. As for London, and the parts adjacent, all that could be learnt was 'that the meteor was seen by 3 gentlemen in Chelsea-fields.' It is probable that on that evening the air was foggy hereabouts, or that there was no wind to carry off the smoke; for these circumstances will easily enough account for there being no notice at all taken of that body in London, and that it was so little heard of in the neighbourhood.

4. A person near Colchester saw the meteor, and said its direction was towards the south-east; that the apparent diameter of the body was about 5 or 6 inches, not so large as the moon when at the highest, but more bright; that the meteor left a train of light behind it; that its progress was extremely swift; that no explosion was heard when it disappeared; and that he did not perceive it to break into stars in the manner of a rocket.

Mr. Wigson, who sent this account, concluded with observing, that as this man was at that time on a journey from Thorp to Colchester, he might easily be deceived as to the points of the compass, by the windings of the road.

5. In tracing the progress of this body northwards, Dr. P. had the following from the Rev. John Michell, fellow of Queen's College Cambridge, as he collected them from a person who saw them near that place. The first appearance was at least 70° high, and it appeared to move directly perpendicularly, till it came down to the horizon, where it passed between two trees, which the man pointed out: this last place of its appearance was 23° west of the north from the place where he stood; and as we were at least a mile distant from the trees, we may depend on that bearing to a degree or 2 at most. The head, which went foremost, was by the description, of a bright white, like iron, when almost of a melting heat; but it emitted no sparks, as iron does in that state. The head was about half the diameter of the moon, and till it had descended to within about 14° of the horizon, was somewhat less in the vertical than in the horizontal diameter; but from 14° high, it was at its utmost splendor, and round, and continued so till it disappeared. The tail was about a 5th part of the breadth of the head, and when the head was about 27° high, was at the longest: the length then might be somewhat more than 8°, which was the mean length.

* This bearing carries the meteor about a point farther to the westward than what is consistent with the common maps, and several of the following observations. On the supposition that the observer was tolerably exact in pointing out the apparent altitude, at which the meteor disappeared, and that it was extinguished when nearly perpendicular to Fort William (in the highlands of Scotland), as shall be shown afterwards, then allowing 22 miles for the curvature of the earth, and a distance of 420 miles between Silchester and Fort William, the real height of this body, at its disappearance, was about 32 miles.—Orig.

The colour of the tail was a duskish red, about the colour of red-hot iron, all of a breadth, not pointed. When the head was about 6° or 7° high,* the tail burst, as the informer expressed it, and the brightness of the light dazzled his eyes; after which the tail disappeared, and in the room of it there were 3 stars, all contained within the compass of a little more than one degree from the head; and they, together with the head, descended, keeping their due distance, till below the horizon. The diameter of these stars was nearly the same with the diameter of the tail, viz. about 3′; but they were of the same colour as the head. The brightness of the light was so great, that one might see to pick up a pin, and some noise was heard; but of this latter at least Mr. M. was a little doubtful.

6. Pursuing the progress of the meteor northward, the next information was from Manchester. Mr. Lloyd of that place, F. R. S. wrote, that though it had been seen there by several, the only tolerable account he could send was from Lord Derby's head-gardener; who said, 'As he was returning from Liverpool to Knowsley (a place at 7 or 8 miles distance), about 8 in the evening, he was surprised by a sudden glare of light; and that he soon saw a ball of fire appearing, of half the breadth of the moon, moving horizontally eastward, a little inclined to the north, with a hissing noise: that a train of light, like a tail, followed it, which being soon collected into the body, it burst, and part seemed to fall down like stars, and the rest vanished. He thought the whole appearance continued about 2 minutes.—7. Mr. Lloyd added what follows from near Liverpool. 'On Sunday last was seen by several credible persons, between 9 and 10 o'clock at night, a ball of fire, which arose in the east, and appeared to increase in size for some time, and then burst, without noise. Its direction was to the northward.'

8. Cockermouth, in Cumberland, is about 86 miles north by west of Liverpool. Mr. Muncaster of that town says, 'that the meteor passed over that place about 9 in the evening, with a very great velocity, towards the N.W.; that it gave so strong a light, that the smallest thing might have been seen on the pavement; and that it disappeared in less than a minute: that the globe of fire appeared as large as the moon when she is high, but much brighter; and had a tail of a conical form; but that they did not observe any sparks or stars fall from it, like those which are seen on the bursting of a rocket; nor did they hear any explosion.'

9. From Carlisle, which lies about 26 miles N.E. of Cockermouth, the account received was more particular, and was from Mr. James Hewit, wine-merchant

* Supposing this angle of elevation just, the real height of the meteor, when the tail broke off, over the shire of Lanerk in Scotland, was about 42 miles, allowing for the curvature of the earth. —Orig.

of that city, who not only had a view of the meteor during part of its course, and heard a report, but measured the height at its apparent elevation when he saw it, and at its extinction, from the memory of another person, who had a sight of it to the last. The direction of the meteor was from south-east to north-west. It did not appear in a globular form as it passed over Carlisle; but tapered to a point. Its head seemed to be about 14 inches in diameter, and its length from head to tail about 5 yards. It appeared much brighter than the moon, and lighted the atmosphere to such a degree, that a person in the street could easily have distinguished the difference between a small needle and a pin, if they had been lying on the ground before him. It emitted several sparks as it went along, and continued in sight about 25 seconds. About a minute after it disappeared, there were two explosions immediately following each other, of a hollow noise, as loud as the report of a cannon at 2 or 3 miles distance; and immediately after the explosions there was heard a confused rumbling noise in the air, which continued at least 20 seconds. The greatest height was 32° above the horizon, on a vertical circle; and 41° from the north towards the west.* The height when it disappeared, was 8°.† In answer to what you require in your last (viz. whether the path of the meteor was to the eastward or westward of Carlisle?) Mr. H.'s situation, when he saw it, was near the centre of the town; and the bearing of that part of the house, over which he saw it, was 41° from the north towards the west; and as its progress appeared on the left side of him when facing the said house, the path was consequently west of him and of any part of this city.‡

10. Mr. Jonathan Ormiston, merchant at Newcastle, stated, from the Newcastle Journal, that a surprising large meteor was seen here, about 9 o'clock, which passed a little westward of the town, directly to the north, and illuminated the atmosphere to that degree for near a minute, that though it was dark before, one might have taken up a pin in the street. Its velocity was almost inconceivably great; and it seemed near the size of a man's head. It had a tail about 2 or 3 yards long; and as it passed some say they saw sparks of fire fall from it. It appeared low in the atmosphere; and we are advised from Edinburgh, that it

* It appears by this observation, that the meteor being so low as 32°, it must have passed the town a great way before Mr. Hewit got a sight of it.—Orig.

† The meteor being extinguished when perpendicular to Fort William (as will appear by a subsequent observation), at the apparent altitude of 8° at Carlisle, makes the real height at Fort William to have been between 26 and 27 miles, allowance being made for the curvature of the earth, of $3\frac{1}{2}$ miles, between these two places.—Orig.

‡ From this last circumstance, compared with observations 5 and 13, we are enabled to judge nearly of the true path, which must have run from Cambridge across the Solway-frith, between Carlisle and Dumfries, and by obs. 16 on to Fort William.—Orig.

2

passed over that city* just about the same time, had the same appearance, and moved in the like direction.

11. Mr. Martin Doubleday, near Durham, says that a quarter before 9, as he was sitting writing by candle-light, with his face towards a window fronting the north west, he was surprized by a sudden and extraordinary light, and stepping hastily to the window, saw the resemblance of a large sky-rocket, falling and bursting into sparks of fire, which became more scattered in its descent, and seemed to be quite spent by the time it reached the horizon (which it did, as near as he could guess, due north †), its path appearing luminous to a considerable distance from the scattered parts, which with it were not dispersed, but as if confined between 2 parallel straight lines. The greatest height of its luminous path, when he first saw it, was 25° above the horizon, N.W. by N. He heard no noise at the time, nor after; and he concluded from its appearance, that it must have begun to burst before he saw it.

12. Mr. Blake, F.R.S. sent the observation of the Rev. Wm. Henderson, vicar of Felton (a village about 24 miles N.N.W. of Newcastle); who says, that the night was dark and calm; that as he was going home a little after 9, the road was instantly so much enlightened, that he might have seen to take up a pin; that the globe to the eye was about the size of a cannon-ball of 6 or 7 pounds weight; that he could not guess at its distance from the earth, but during the short time he saw it, he imagined he heard it whiz over his head; that it had a tail like that of a comet, almost a yard in length, perpetually emitting sparks of fire; that the time of observation was very short, on account of a great hill that rises on the south side of the river Coquet, and of a clump of trees on the north side, which obstructed his view; that its velocity was great, for that it did not continue in sight above 5 or 6 seconds; and that its course, as near as he could judge, was to the north-west.

13. Dr. Gilchrist, physician at Dumfries (which lies a few miles north of the Solway frith, and about 30 miles N. W. by W. of Carlisle,) stated, that the best account he could get of the meteor (which he did not see himself,) was from a young man of that place, who, in common affairs, was sensible and distinct. This person being in a room on a first floor, which had the windows to the north-east, was surprized by an extraordinary light, and, running to one of the windows, saw a large fiery body, like red-hot iron. It appeared to him as large and as long, as a middle-sized man, the fore part broadest; its progress was from S. E. to N. W. part of the tail separated from the rest, but he still thought it followed:

* By the accounts I had from Edinburgh, it was not nearly vertical there.—Orig.

† The head of the meteor, seen from this gentleman's house, could not reach an unobstructed horizon, nor be seen due north by 3 or 4 points, consistently with most of the other observations. —Orig.

the body for a little space, and then it burst like gunpowder, though without noise, and fell down in sparks of fire, while the body kept on its course; but which he immediately lost sight of, by a house of 2 stories high that intervened Dr. Gilchrist added, 'that a young lady of his acquaintance, happening to be in the street near the same place, saw the meteor likewise and described it as a ball of fire, about the size of the sun, with a tail; and the length of the whole as longer than one's arm. She said it was almost over her head, higher than the town steeple; that it burst without noise, and was entirely dissipated into sparks of fire, which fell down, and, as she thought, almost reached the tops of the houses.'

14. The Rev. Wm. Turnbull, minister of Abbotrule (a parish about 46 miles N. by E. of Carlisle, 44 miles N. E. by E. of Dumfries, and 6 miles S. W. by W. of Jedburgh, in the shire of Roxburgh,) stated, that about 9 at night, sitting in his parlour, which had a south-west window, he very distinctly saw a light, which he took for a flash of lightning; but was surprized with the difference of its colour, being whiter, and giving a clearer view of every thing in the room, than what he could have expected from common lightning; that, however, he waited for a clap of thunder, and accordingly, at the end of 5 or 6 minutes, he heard a very great explosion, not indeed so like thunder, as the crashing noise of the fall of a house; and being persuaded that this was really the case, and that the gabel-end of his own house, farthest from the room he sat in, with the offices, had fallen, he ran out, but found no damage done, nor saw any clouds, it being clear star-light.'

15. Mr. Walter Pringle, sheriff-depute of the shire of Roxburgh, stated that a servant of the house, where he happened to be that night (about 20 miles S. S. E. of Edinburgh, and as far N. N. W. of Abbotrule,) came in about 9, and told him there had been some thunder and lightning; which he thought very improbable, as he had been out but a few minutes before, and had not seen a cloud in the sky.' Mr. Pringle added, that the Rev. John Smith, of Jedburgh, had written to him as follows: 'I am surprized, that, in all the accounts given of the meteor, one remarkable circumstance is omitted, namely, the horrid crack which I heard, being then on the confines of Cumberland, near Stonegarthside (about 15 miles N. by E. of Carlisle:) it was much louder than the report of any heavy cannon, and continued about 7 or 8 seconds. The people thought it a peal of thunder; but this, I imagined, could not be the case, as the sky was not clouded. I did not see the meteor myself, being then within doors.

16. In another letter, Mr. Pringle said, that he had conversed with one James Turnbull, a farmer at Ancram, a village about 3 miles north-west of Jedburgh, who had seen the meteor and heard the explosion. He said that,

about 9 at night he happened to be out, and on returning to his house, and just entering the threshold, the whole side of the house became suddenly enlightened and with a brightness as of sun-shine. His back being towards the place whence the light came, he quickly turned about to see what might be the cause of it, and then beheld a globe of fire about the size of the crown of his cap, directing its course towards him as he thought. He scarcely had time to think, when it passed by him to the north-west with a very great swiftness, and very high in the air, viz. 58° high, as afterwards measured by Mr. P. When it came opposite to the gabel end of the dwelling-house, he then discovered its true figure: it was perfectly round at the great end, which went foremost, and tapered 3 or 4 yards in length. Being resolved to see it as long as he could, and fearing the wall and roof of his house might intercept the view, he moved 6 or 7 yards farther off the house, keeping his eye fixed on the meteor, and observed that it had not gone above a quarter of a mile, when one-third towards the small end broke off; which third separated into sparks of fire, resembling stars, and immediately vanished. Soon after the remaining body vanished also, directly to the north-westward of his house, and the former darkness returned. The time of the meteor's appearance, during his observation, might be near a minute. After he had been in the house about 5 minutes, he heard a noise, like a clap of thunder, of some continuance; and, on his daughter's saying there is thunder, he said that could not be; for that he had seen no clouds when he was out. On this he went out again, and found no clouds, but clear star-light. Several of his neighbours in the village of Ancram (which lies about 300 or 400 yards from him a little westerly of the south, and over the middle of which the meteor passed, according to his imagination,) likewise saw the meteor, and heard the report. One of them in particular said, that the noise came from the fire as it went along. Another who heard the report, said it sounded to him like a crashing noise, and in such a manner as made him imagine that the gabel-end of his own and his neighbour's house, which were contiguous, had fallen down.

17. After making a survey of the place, Mr. Pringle got the farmer to point to that part of the heavens to which he referred the meteor, when opposite to the gabel-end of his house; and the observer seeming to be well assured of the place, Mr. Pringle took the altitude with an instrument, and found, after 3 trials, the height to be about 58°.* He concluded with saying that, in answer to some more queries, the farmer had told him, that he had observed little

* At this time the meteor must have been vertical, about 2 or 3 miles to the southward of *Lochmabin*, a town in the shire of Dumfries distant from the observer about 37 miles, and from that place where the tail afterwards broke off 31 miles. From the altitude here given, Dr. P. computed the real height, at this place, to have been about 9 miles.—Orig.

rising or falling of the meteor during its whole course; but that its motion, from the time he first saw it, to its extinction, seemed to be nearly in one straight line, at an equal height above the horizon; † and that the light was continued and uniform, without any fresh burstings of flames from either the head or the tail.

18. All the information received from that part of the country over which the meteor seemed to break, was from Lord Auchenleck, as follows: about a quarter after 9 that night, there appeared from the south-east a very great illumination or light, which instantly made such a splendor, that to a considerable distance one could most distinctly see houses, trees, water, stones, &c. but could not observe any particular body from which the light issued, nor that it ran farther westward; from which we may conclude, that it had then broken. No noise was heard. During the preceding part of the day, there was a strong and very cold south-east wind, with a little frost; but the evening was more calm.

From this letter it appears, that the sky in those parts, as about London, was then so much clouded, as to hide the body of the meteor, though the light of it was very manifest, and which was probably the brighter there for the bursting of the tail, and its dissolution into sparks of fire, when almost vertical to the observers.

19. Sir Robert Pringle, who was at Stitchill, (about 10 miles N. N. E. of Jedburgh, and about 60 miles, nearly in the same direction, from Carlisle,) wrote that he did not see the meteor nor met with any body that observed it, further than the great light with which it was attended, making every thing to be seen on the ground as distinctly as in sun-shine, and which continued, as they said, much longer than a common flash of lightning from thunder. At that time Sir R. happened to be sitting, with some of his family, in the parlour, and all of them heard a noise they could not account for, as sounding like a gun fired off in the garrets, or a cannon discharged about a quarter of a mile off; but the noise continuing like thunder at a distance, they concluded it was nothing else, till one of the maid-servants came in, and said she had seen a very surprizing flash of lightning, both for its clearness, which she compared to noon day, and for its continuance. Some of the Edinburgh newspapers describe the body of that meteor to have been like a large star coming from the southward, and ending in the northward, both points westward of the observer, with a train after it, in form like a cone; and with several sparks falling from it as it went along. These accounts say nothing of the length of this luminous appearance; but that it seemed to be about 10 or 12 inches broad at the head; nor do they mention any sound that was heard after it vanished.

† This remark must be corrected by the last paragraph of the last note of Obs. 16.—Orig.

20. Mr. Redpath, son of Mr. Redpath of Angelraw, (a place about 4 miles north-east of Stitchill, in the shire of Berwick,) said that he did not see the meteor himself; and that from all he could gather, its direction was from the south-east to the north-west, but nearer the south and north points than the east and west, with a tail of considerable length, pointing downwards, inclining to the east; that its course seemed to be very quick, and that sparks of fire fell from it as it moved along; that the whole was of a conical figure, and appeared to be about 5 inches at its basis; that a very strong light issued from it, which, in those houses where the candles were out, darted through the windows with such strength, that the rooms were wholly illuminated by it for 7 or 8 seconds; that its first appearance was not exactly at the horizon, but a little above it, and that, at its greatest height, it certainly did not exceed 40°; that it was extinguished before it reached the horizon, perhaps by about 8 or 10 degrees; that the colour of the meteor was at first nearly of a pure white, but in proportion as it advanced, it grew red, and seemed to go out all at once; that the light which issued from it seemed rather to consist of successive flashes from side to side, than of an uniform regular flame; that a few minutes after its disappearance (not above 3 or 4) was heard by several people a violent thunder-clap, or something very like it, and from the same point it disappeared, viz. rather nearer to the north than the north-west."

21. At Dalkeith (a market town 6 miles south-east of Edinburgh,) a gentleman, who happened to be walking eastward in the street, perceiving his right side and arm strongly illuminated, suddenly turned his face to the light, and saw the meteor, then in a direction at right angles with the street, having an altitude, as he conjectured, of about 45°; he observed that the figure was oval, the light great, and of a bluish cast; but he heard no sound.

22. By an article published in the Edinburgh news-papers, the meteor appeared there of a conic form, but about 5 or 6 inches broad at the basis, and lasted 5 or 6 seconds; its light was great, and sparks flew from it like those of a rocket, when its force is spent.

23. An article in the Glasgow paper was to this purpose. About 9 o'clock a globe of fire came over this city from the southward, in appearance as large as the full moon. It made the streets as light as at noon-day, lasted about a minute, and just before it vanished, it divided into three parts directly over the middle of the town, and then ascended through the atmosphere.

24. From Dunfermline (a town in the shire of Fife, about 14 miles north-west of Edinburgh,) Dr. Stedman stated, that he had only found two persons who had seen the meteor, a man and his wife, from whom he had the following particulars. That the first view they had of it was in the south-south-east, as it came from behind a building; and that it seemed to them to move westward;

that the hinder part or train emitted large sparks or globules of flame, such as are seen to fall from a sky-rocket, when it begins to break; that its altitude was about 24°, which Mr. S. had taken with an instrument afterwards; that they lost sight of it before it was extinguished, by a steeple that stood in the way; that its head or fore-part appeared somewhat broader than the full moon. When Dr. Stedman sent this account, he had omitted taking the bearings; but, in his next letter, he said, he had supplied that defect, and found, that the first appearance to the observers (when the meteor came from behind the building that intercepted the sight of it,) had been about south by east $\frac{1}{3}$ east; and that it had disappeared behind the steeple at about south by west $\frac{2}{3}$ west; that, during this short course, it neither seemed to them to ascend nor descend.

25. One Mr. Cairns (a young man then appointed surgeon's mate to one of the regiments at Gibraltar,) stated, that he had seen the meteor; that he was then in the shop of Mr. Oliphant, surgeon-apothecary at Culross (a town about 19 miles W. N. W. of Edinburgh;) that Mr. Oliphant and he were surprised by a sudden glare of light from the street, coming as it were, in successive flashes, but without any intervals of darkness; that they both ran out, and observed a ball of fire moving, with great velocity, in a direction nearly from the south-east to the north-west; that its height seemed to be considerable; but they had not seen it to the last, by reason of some houses on the north side of the street, which stood in their way; that it was somewhat of a less size than that of the full moon, when about the same height above the horizon; and of an oval figure, with the longest diameter in the course of its direction. He observed no tail, nor sparks of fire issuing from it; but said, that some people of the town had taken notice of the latter; that the meteor itself was of a reddish fiery colour, though the reflection of the light from the streets was of a yellowish cast; that he heard no explosion himself, and had met with none who pretended to have heard any noise, either during the appearance of the body, or after its extinction. The duration he made about 13 seconds; which gives its rate of motion from Cambridge to Fort William, a space of 400 miles, about 30 miles in 1 second of time.

26. Dr. P. wrote to Dr. Simson, professor of medicine in the university of St. Andrew's, (which lies about 31 miles N. E. by N. of Edinburgh,) who answered, that his family had been alarmed by the light, and that one of them cried out, the heavens were all on fire; that his son (a minister) happened, at the first appearance of the light, to be standing close by a south window, and saw the meteor like a ball of fire, but of an oval figure, with its longest axis in the direction of its course, of a size equal to that of the full moon at her greatest

height;* that it moved with great velocity from the south-east† (about which point he first saw it) towards the N. W. but that he had lost sight of it about the S. W. by the intervention of a building on the opposite side of the street, before it had fallen from its apparent height: that he observed no tail, nor sparks of fire issuing from it; and heard no noise after the return of darkness. Its apparent altitude was about $15^{\circ}\frac{1}{2}$; and its continuance 6 or 7 strokes of the pulse.

27. Believing there was a better chance for hearing of its course more to the westward, the following account was given by Dr. Alexander Mackenzie, physician in the shire of Ross. Dr. M. was at Flowerdale, a gentleman's house on the western coast of Rosshire, where the view of the heavens is extremely confined, being quite surrounded, except at one point, by very high and close-approaching hills; hence the meteor must have been high before it could be observed, and it quickly disappeared, as its progress was very rapid. Its light was most surprisingly splendent, but not in the least like that of the sun, except when it shines through a cloud, or a summer shower. Its magnitude was near to that of the full moon, when she is 3 or 4 hours high. Its colour not at all like that of the body of the sun, or an ignited globe, but resembled that of the flame of spirits. Its figure was quite spherical, without any tail; but it emitted, or, as it were, dropped, sparks of various colours and magnitudes. As for its height, it was nearly vertical; and its direction was from the west northerly, to the east southerly.

28. Mr. Cleghorn, author of the natural history of Minorca, writes from Dublin, that though the meteor did most certainly appear at Dublin, as well as in England, yet few people had observed it with attention, and none, that he could hear of, had committed any thing to writing, excepting one Mr. Garret, a good sensible man, with some mathematical learning, whose account he there-

* If Mr. Simson lost sight of the meteor duly S. W. of him, it must have then been perpendicularly over the southern part of the shire of Lanerk, about 66 miles from the observer, and about the nearest he could have seen it any where in its course. I shall therefore suppose, that it was at its greatest apparent diameter just before it disappeared; that is, equal to that of the full moon, according to his comparison; consequently its real diameter was about half a mile, on the like computation with that on Obs. 24. This is the most moderate; for the meteor might have been considerably larger even from this observation.

† Having omitted desiring Mr. Simson to take the bearings with a compass, he has not imagined that I required any greater precision than having the most common points; but as I find Cambridge laid down in all the maps nearly S. S. E. of St. Andrew's, and as we have no reason to believe the meteor was lighted to the eastward of Cambridge, it is probable Mr. Simson did not see it till it was nearer to the south than the S. S. E. But supposing this gentleman saw it at its first setting out, viz. over Cambridge, and duly S. S. E. then, from the angle of elevation of $15\frac{1}{2}$ deg. the distance between the two places, and an allowance made for the curvature of the earth, the perpendicular height at Cambridge must have been about 100 miles.

fore sent as follows, kept by that person; and to which was subjoined an answer to some queries that had been put to him concerning that body. Here is given the paper, of which the following is the substance.

Fifteen minutes past 8 in the evening, a globe of fire about 17° high, due east, moved from south to north, as large in appearance as the moon, but more of a golden colour; it broke and dispersed, like a starry rocket, in small bright sparkles, nearly before the wind, or as if they passed away with the wind.

XXVII. Remarks on the several Accounts of the Fiery Meteor (which appeared on Sunday the 26th of November, 1758,) and on other such Bodies. By John Pringle, M. D., F. R. S. p. 259.

First, as to the path of the meteor. This meteor seems to have been vertical at Cambridge, or nearly so, and to have taken fire about the zenith of that place; or at least to have appeared first there in a state of ignition. Thence it proceeded directly, almost N. W. by N. over several counties in England, over the Solway frith, which it crossed between Carlisle and the town of Dumfries; and in Scotland over the shires of Dumfries and Lanerk: but soon after its becoming vertical to the last, viz. a few miles to the southward of Douglas (or perhaps nearer to the borders of Lanerk and the shire of Air, about 10 or 12 miles to the eastward of Auchenleck,) part of the tail seemed to break off, and to disperse in bright sparks of fire; while the head, into which the remainder of the tail was instantly collected, moved on in the same direction, till coming over Fort William, in the shire of Inverness, after a course of about 400 miles, it there suddenly disappeared. But, notwithstanding the extinction of the meteor at this place, it seems still to have proceeded northwards; since it was seen again in a luminous state, in a globular form, but without a tail, about the 58° of latitude, on the western coast of the shire of Ross, almost vertical to the observer; moving then to the southward of the east; that is, in a direction almost contrary to the first: and in this last course, of which we know not the end, it possibly might have gone a great way to the eastward.

During the first part of its progress, viz. from Cambridge to Fort William, it went obliquely downwards in such a manner, that, by computation, it must have been from about 90 to 100 miles high at the first of these places, and between 26 and 32 miles at the last. But at what height it was afterwards seen in the shire of Ross, is not to be determined, since one observation only was transmitted from that country. As Dr. Mackenzie observed it nearly over his head, and yet of a smaller size than the full moon when some hours above the horizon, perhaps after its descent to Fort William, it had reascended at the time he saw it; because he probably would have described it as a larger body, if then not higher than when it first disappeared.

This dipping and rising in the course of a meteor, is not more extraordinary than its lateral deviation from a straight line.

In regard to the velocity, it seems almost incredible; as we have sufficient data for computing it at the rate of 30 miles in a second. But if we allow that it only moved through half the space in that time, we shall find the progression of this body to have been above 100 times swifter than the mean celerity of a cannon ball, and nearly equal to that of the earth in its orbit round the sun.

As to its real size, we cannot pretend to determine that point with any precision; since its dazzling brightness would occasion some deception, and the apparent magnitude has been so differently represented by the observers. If the meteor, when nearest to Dublin, appeared to Mr. Garret equal to the full moon, then we shall find that its true diameter was about 2 miles; and if the farmer at Ancram saw this body while it was vertical at Cambridge, of a size equal to the crown of his cap, or to about half that of the full moon, we cannot allow less than a mile for the real axis. On estimating from the observations made at St. Andrew's and Dunfermline, the diameter was at least half a mile, and perhaps much greater. However, as the imagination is so apt to enlarge such objects, we shall fix the size of the globe at the smallest, and reckon it only about a mile and a half round.

The body must have been of a considerable bulk to have yielded such a light, as that, when in the zenith of Cambridge, the farmer at Ancram, at the distance of above 260 miles, should, on entering his threshold, see the whole side of his house illuminated by it; and, to use his own expression, with a brightness as of sunshine. And indeed the greatness of the light is every where taken notice of, even at those places where the atmosphere was so thick as to hide the tail, nay, the whole meteor, as at Auchenleck, where it was nearly vertical.

As for the tail, it was a stream of light several miles in length; for this was no deception, like what we suppose the train of a shooting star to be, but was either a real flame, or, what is more probable, it consisted partly of flame, but mostly of smaller masses of fire (which the observers call sparks, when falling out of the lucid tract,) and of vapours or fuliginous particles, not heated red hot, but illuminated by the parts actually burning. Perhaps these vapours were the chief part of the composition, and which will account for its light being so much fainter than that of the head; since in some places where the air was less clear, or the distance greater, we find the whole meteor described either as a round ball, or a spheroid (with the largest axis in the direction of its motion,) but without a tail. In this last case, viz. that of the oval form, it is probable that, besides the head, the beginning of the tail was also visible, as consisting of flame, and therefore brighter than the rest; and that both together appeared oblong to

those observers. But such as were nearest, and had a clear atmosphere, saw the tail of a considerable length; that is, the flame, the sparks, and the illuminated vapour, in a train behind the head, as being lighter, and therefore meeting with more resistance from the air: in the same manner as the flame, the sparks, and smoke, of a torch are seen to follow it. All this is plain; but in regard to that separation of the third part of the tail from the rest, a circumstance clearly described by the farmer at Ancram, and seemingly confirmed by other observations, there may be some difficulty. Perhaps at this period, on a greater explosion in the ball, most of the combustible matter was thrown out at once, which falling behind, occasioned that appearance of the breaking off a part of the tail, while, for want of fuel, the remainder vanished, or, as the observer expresses it, was collected into the head. This account is rendered more probable by what is said of the emission of a greater light about this time, and by the loud report heard by the farmer 5 minutes after, and which, on computing the distance, ought to have reached him much about that time, had it been occasioned by this extraordinary bursting and dispersion of the inflammable matter.

The hissing noise, taken notice of by some while the meteor passed them, was a deception of that kind, which frequently connects sound with motion; and is the case of those who fancy they hear something, when they see the shootings of the aurora borealis; I say a deception, because if the meteor, during its course, really made any noise, so great was the distance of that body, and so short its continuance, that this sound could not have been heard till some minutes after the return of darkness. But the final report, so frequently mentioned, not only heard by those who saw the light, but by others who knew nothing of what had happened, was a real sound, and immensely greater than any we are acquainted with. For, at the distance of 70 miles and upwards, it was compared to loud thunder, the report of heavy artillery, the fall of the gable-end of the house the person was in, and to a musket fired off in the garret. If this noise was produced when the body threw out those masses of burning matter, by the observers called sparks of fire, the bursting of the tail, &c. we shall find that at this time the meteor, by being more than 41 miles high, was in a region where the air is 3000 times rarer than on the surface of the earth: that is, about 6 times rarer than in a common exhausted receiver, where sonorous bodies are not heard, and even where gunpowder and the pulvis fulminans take fire, and are exploded, but without noise. Hence Dr. P. infers, that the separation of the elastic matter must have been performed with a velocity exceeding all imagination, as the intensity of sound so much depends on the resistance of the air, and as this elastic matter could fly off with so much celerity, as to find so great an opposition from so thin a medium.*

Dr. P. also concludes from the great report, that the substance of the meteor

was of a firmer texture than what could arise from mere exhalations, whether formed into a sphere, and then burning, or disposed into a kind of train, and consumed by a running fire: for sounds, as far as we know, are either produced by the quick and violent percussions of hard bodies on the air; or by the sudden expansion of an elastic fluid, after being condensed within some solid substance. The noise occasioned by the motion of electrical matter is, perhaps, the only exception; but we have no reason to imagine, that this was at all concerned in the present case. There seems to be the more ground for believing this body was solid, at least that its surface was so, from finding, that after the violent explosion, it still retained its form; a circumstance that could hardly take place if the meteor had consisted of nothing but vapours. We may therefore presume, that the burning matter found vent through a hard crust by certain apertures, which either might have been there invisible, or unobserved. All I can say in support of this conjecture is, that, by the Memoirs of the Academy of Bologna, we find a meteor appeared in Italy in the year 1719, lower in the air than that we have now been treating of, and in which, it is pretended, four several chasms were distinguished, each emitting smoke.† To these arguments for the solidity of this body, we may add its extreme velocity, and the intensity of the light: which are likewise circumstances more conformable to a heavy and solid substance, than to one formed of exhalations only.

On the whole, Dr. P. believes it will appear, that these accounts are not favourable to the prevailing hypothesis about the formation of such bodies, which makes them to consist of certain sulphureous vapours arising from the earth: for, besides what has been urged above, Dr. Halley has shown, "That at the height of 41 miles, the air is so rarefied as to take up 3000 times the space it occupies on the surface of the earth; and that, at 53 miles high, it would be expanded above 30000 times: but thinks it is probable, that the utmost power of its spring cannot exert itself to so great an extension; and that no part of the atmosphere reaches above 45 miles."‡ This being the case, how can we suppose any such vapours to rise to the height of 90 or 100 miles, where the air must be so many millions of times rarer than what we breathe?

Some have been of opinion, that these fiery meteors are only a kind of lightning, at greater heights than common; forming their notion on the velocity of those balls of fire, and on the sound accompanying them, so much resembling

* M. Saluce has lately shown, by some curious experiments, that such substances as gunpowder and the pulvis fulminans, have a detonation in proportion to the rapidity with which the internal air is separated, and to the resistance of the external air. See Miscell. Taurinens, tom. 1.—Orig.

† Apparebant in eo hiatus seu voragines quatuor fumum exhalantes. Instit. et Acad. Bonon. tom. 1, p. 285.—Orig.

‡ Phil. Trans. N° 181, p. 104. Abridg. vol. iii. 300.

that of thunder. But this hypothesis having gained no credit, we need not employ time in refuting it; and the less now, as the nature of lightning is so much better understood than when this theory was first published. It may only be observed, that before the matter of lightning was discovered to be of the electrical kind, it was natural to suppose it to be formed of the sulphureous vapours arising from the earth; and if the earth was found proper for producing such exhalations, of course it was judged capable of furnishing materials for all the lucid phenomena in the ethereal regions. Thus, not a hundred years ago, the comets themselves were accounted for on no better principle; and therefore we are the less to wonder if these meteors have been hitherto almost constantly referred, even by the best naturalists, to the same origin.

If it is then probable that these balls of fire come from regions far beyond the reach of our vapours; if they approach often so near to the earth, and so seldom or never touch it; if they are moved with so much celerity, as in that respect to have the character of celestial bodies; if they are seen flying in all directions, and consequently have a motion of their own, independent of that of our globe; if they part with such quantities of an elastic fluid, a phlogistic matter, and probably an acid, surely we are not to consider them as indifferent to us, much less as fortuitous masses, or trains of terrestrial exhalations in the ethereal regions; but rather as bodies of a nobler origin, possibly revolving about some centre, formed and regulated by the Creator for wise and beneficent purposes, even with regard to our atmosphere; which, during their combustion, they may supply with some subtile and salutary matter, or remove from it such parts as begin to be superfluous, or noxious to the inhabitants of the earth.

XXVIII. Thoughts on the different Impregnation of Mineral Waters; more particularly on the Existence of Sulphur in some of them. By John Rutty, M. D. p. 275.

It would be useless in the present advanced state of chemical knowledge respecting the different impregnation of mineral waters, to reprint this paper.

XXIX. On the Effects of a Storm of Thunder and Lightning at Rickmansworth, in Hertfordshire, July 16, 1759. *In a Letter from Mrs. Ann Whitfeld. Communicated by Mr. John Van Rixtel, F. R. S.* p. 282.

One of those common effects of a house struck by lightning. A stack of chimneys and some tiles carried off the roof. Some windows forced in, several things within the house broken, the rooms filled with dust and sulphureous smell, &c.

XXX. *Extraordinary Effects of Lightning. By Mr. Wm. Mountaine, F.R.S.* p. 286.

One remarkable circumstance in this account is, that the stroke of lightning happened seemingly at the same moment as that mentioned in the foregoing article, as appears by comparing the dates in the accounts, the one being said to be about 8 o'clock, and the other about a quarter after 8, the same morning; though the one place was London, and the other in Hertfordshire.

During the morning of July the 16th last, says Mr. M., was much thunder and lightning: about 8 o'clock was heard an extraordinary loud crack, which seemed to be very near, as the large flash and sound were almost coincident. In a few minutes there was an alarm, that a house was on fire in Goat street in Southwark, Mr. M. readily suspected the cause, lightning, and soon after went to the place to inquire into particulars, and was informed that 3 houses were damaged in that place. The first house was almost untiled on the west side, and being of timber, was very much split and shattered: some of the weather-boards were thrown outwards to the bottom of the garden, to the distance of about 30 feet from the house, and the windows were forced inwards. Several small pieces of glass, in the leaded windows, were impelled with such force, as to stick very fast in a door which was opposite, and in the hard plastered partition; besides some bits of melted window lead.

The second house was actually set on fire, but soon extinguished. This house having bell-wires through several rooms, they were melted as by hot fusion, and dispersed in numerous bits all around; many particles being collected by magnets, and otherwise.

In a third house, many glasses and china ware were broken, and the bell-wires in like manner destroyed. In some rooms however, considerable parts of the wire remaining, they attempted to make these serve again, when repairing the bell-wiring, but found them so brittle as to be quite useless.

From these houses the lightning seems to have tended towards the N. E.; for in that direction, at the distance of about 200 yards, a person was struck, standing in his own entry, and rendered almost senseless and speechless for some hours, and for several days was much afflicted with a stupor, giddiness, and vomiting, and retained a constant and strong taste of sulphur in his mouth and throat. On the opposite side of the street, and somewhat oblique towards the N.E. at the distance of about 20 yards, another person had about 6 dozen of bottles of Port wine broken to pieces, which were in an arched vault, the bottles laid on their side within a large circular tub or cooler, as a bin, bound with iron hoops; every bottle being broken as if by a mallet.

Mr. M. declared he had been more particular in the examination of some of

the foregoing facts, as they seem to contradict an opinion generally received, that the solution of metals by lightning is effected by a kind of cold fusion; for it appears very evident that the melted iron-wire, in the several preceding cases, had all the marks of heat and ignition that usually attend the fusion of that metal, when brought about by common fire.

Some Remarks on the Preceding Letter. By Gowin Knight, M.B., F. R.S., and Principal Librarian of the British Museum. p. 294.

The facts contained in Mr. Mountaine's letter, says Mr. K., are an evident proof that the fusion of metals by lightning is, sometimes at least, attended with heat and ignition, as in the case of common fusion. And since the reading of those facts he had been more and more induced to suspect that the received opinion of a cold fusion is a vulgar error, though too generally adopted, and of very long standing. From some of the circumstances attending these facts, compared with what is to be found in authors relating to the same subject, he thinks it possible both to show whence this opinion first took its rise, and how it became so general; and at the same time to prove that there is no clear evidence for the truth of it from any relations hitherto published. Accordingly Mr. K. mentions several cases of both kinds, whence he draws that general conclusion.

XXXI. Of a Meteor seen at Shefford in Berkshire, on Saturday, October 20, 1759. *By Rd. Forster, M.A., Rector of Shefford.* p. 299.

About 6 in the evening a ball of fire fell nearly east from this place. His servant, who is a very sober honest fellow, says it was nearly of the same size with the moon, and full as bright as she ever shines: its motion was very swift, and as far as he could judge (for it was out in a moment) quite downright, i. e. perpendicular to the horizon.

XXXII. Of the same Meteor, seen at Bath. By Mr. J. Colebrooke, F.R.S. p. 301.

Between 5 and 6 in the afternoon, as Mr. C. walked over the north parade, a ball of fire, of the size of a tennis ball, of a very bright colour, with a train of 4 or 5 feet in length, darted from the N.W. and describing the arch of a great circle on his left hand, sunk behind the hills to the S.E.: just before it sunk, several large sparks of bright blue fire issued from it; but it did not seem to burst: it was not more than 2 seconds in its passage, and he could compare it to nothing but the most glorious sky-rocket he had ever seen.

Mr. Peers, a gentleman of London, was at Bath at the same time, and being in a room fronting the east, that looked over the meadow between Bath and Bathwick, he told Mr. C. with some surprize, the next day, that he saw the largest star he had ever seen fall into the meadow; and what was more particular,

that it fell perpendicularly; whereas all he had ever seen before shot obliquely in the sky.

XXXIII. Of the same Meteor seen at Chigwell Row in Essex. By Mr. Wm. Dutton, Watchmaker in Fleet-street. p. 302.

At the time he happened to be walking with his face eastward, with a companion, at Chigwell row, in Essex (which lies about 12 miles east of London, and on a pretty high hill), when they observed a meteor bearing northward of the east, in appearance not high up in the air, though with a considerable angle of elevation, perhaps of about 70 or 80 degrees, with its declination from the zenith eastwards. It moved with great velocity, in a direction from north to south, and seemingly in a curve line downwards; but vanished at the height of 4 or 5 degrees above the horizon, then bearing nearly south of them. It was of a round form, about the size of the planet Venus, when seen at the largest, of a light bluish cast, but very bright. At its vanishing, several particles, still brighter than itself, and somewhat like the stars that are seen on the breaking of a rocket, seemed to issue out of it. They perceived a faint light to follow it, like the tail of a comet, and about 2 feet in length. The whole time of the appearance did not exceed 3 or 4 seconds.

XXXIV. An Account of 2 Stones of remarkable Shapes and Sizes, which for the Space of 6 Years were firmly lodged in the Urethra of a Young Man, and at length successfully cut out. By Joseph Warner, F.R.S., and Surgeon to Guy's Hospital. p. 304.

Thomas Bingham, a very healthy young man, 20 years old, came from Yarmouth to London in Sept. 1759, and put himself under Mr. W.'s care, to be cured of a swelling, which he had in his urethra. On questioning the patient, he was informed by him, that he had little or no pain; that he had never perceived the least difficulty in voiding his urine, nor had he ever had the least involuntary efflux of it: he had not at any time suffered in the least but from the bulk and weight of something that grew in his urinary passage (urethra), which on exercise of late proved troublesome to him. On inspection he discovered a considerable prominence between the testicles and anus. On feeling the part with his fingers there appeared a very evident hardness and tumor.

By introducing a large, smooth, and ductile probe into the urethra, it was evident there was a stone or stones lodged in that passage. He advised the cutting the tumor out, which was complied with; and in the following manner he proceeded to the performance of the operation. The patient being supinely placed on a steady table, of a convenient height, covered with a double blanket, and a pillow put under his shoulders, Mr. W. caused his hands and feet to be

tied together; and by proper assistants he was held in the same position as is done in the operation of cutting for the stone in the urinary bladder. He then proceeded to divide the urethra longitudinally by incision. The extent of the incision was from one end of the swelling to the other: the length and size of the wound enabled him to take away the stones without any violence or difficulty. After the stones were removed, he brought the lips of the wound together, and with the twisted suture he retained them in that situation. By this method, and by occasionally passing a bougie of a proper size into the urethra, beyond the farther extent of the incision, the patient went happily on, till the cure of the wound was completed, which was effected in about 3 weeks; and there afterwards remained no inconvenience at all to the patient in voiding or retaining his urine.

Weight of the extracted Stones.

	In air.	In water.	Spec. gravity.
Large stone	308. 5	92.85	1.431
Small stone	44.35	10. 4	1.326
Both stones	350. 8	102. 9	1.415

XXXV. Experiments on the Tourmalin. By Mr. Benj. Wilson, F.R.S.* p. 308.

The first account met with of the tourmalin, and the remarkable properties belonging to it, was from a memoir in the Berlin acts, printed in the year 1758; where it appears that F. V. T. Æpinus, professor of natural philosophy, made several very curious and judicious experiments on it; the most material of which prove a plus electricity on one side of it, and at the same time a minus electricity on the other side; provided the tourmalin is moderately warmed, and even by hot water. These appearances are the more extraordinary, as the like means employed in the same manner upon diamonds, glass, and all other electric bodies hitherto tried, produce no such appearances.

The Duke de Noya, who visited this kingdom in 1758, wrote a small treatise on the subject, and published it at Paris on his way to Italy: in this work he mentions Æpinus's experiments, but does not admit of a plus and minus electricity belonging to the tourmalin when heated. On the contrary, he says that the two sides are electrified plus, but one of them is more so than the other; and that it is the difference between those degrees which has led Æpinus into the mistake.

Mr. W. repeated most of the experiments mentioned in the Berlin memoir,

* The tourmalin belongs to the genus of siliceous stones, and is a species of shorl (schorlus electricus) found at the Brasils, Ceylon, in the Tyrol, &c. and generally in a crystallized form. The crystals are truncated prisms with 3, 6, or 9 faces or sides, and are either of a green or blue colour. See the 3d vol. of Hauy's Traité de Mineralogie.

soon after it appeared in England, with the tourmalin belonging to Dr. Sharp, being the only one known of here at that time: and though but a small one, compared with Æpinus's, yet it was large enough to satisfy Mr. W. that his opinion was well founded.

The largest tourmalin Mr. W. had from Dr. Heberden, and with which he made the following experiments, weighed above 120 grs. It was of an oval form, and polished: the greatest diameter an inch and a quarter, and the least one inch. One side plain, the other convex, but cut into several small planes or facettes, something like a rose diamond; the thickest part of which is near one third of an inch. To make experiments with the tourmalin requires the greatest attention, as the appearances sometimes are scarcely sensible, so that he has been obliged to employ the tenderest kind of apparatus, and even interpose a sort of screen to prevent his breath, or other like motions of the air, from disturbing the experiment.

Mr. W.'s apparatus for making many of these experiments, consisted of 2 very small balls made of the pith of elder, and suspended by two linen threads of the finest kind: the ends of these threads were fastened to a slip of wood about 3 inches long, and half an inch broad: then on a stick of sealing wax, 9 inches long, fixed upright on a table, or any other convenient place, was fastened the slip of wood, from one end of which the threads &c. hang down 5 inches, so that the pith balls are about 4 inches from the table. These balls are always supposed to be electrified plus, except where the contrary is mentioned: but with no greater power than to make them recede from each other about 1 inch in every experiment.

Mr. W. prefers the wax stand to a glass one, as the latter when cold acquires moisture very soon, and therefore becomes a conductor: whereas wax, when it is once in good order, will continue a non-conductor for a long time. Before setting down my experiments, he mentions 3 truths that are commonly known, and which for the present he calls general laws, viz. 1. Two bodies equally electrified plus recede from each other, or are repelled. 2. Two bodies electrified minus recede also from each other, or are repelled. 3. One body electrified plus and another body electrified minus, to an equal degree, accede, or move towards each other, or as it is generally expressed, are attracted.

Experiments on the Tourmalin.

Exp. 1. One edge of the tourmalin being properly fastened to a long stick of sealing wax, Mr. W. dipped the stone into boiling water, and continued it there near one minute. On taking it out, and presenting the convex side near the pith balls, they immediately receded from it, but not very strongly. On turning the plain side towards the balls, it caused them to accede to it, but rather at a greater distance in this case, than they receded from it in the last. When the

stone was colder, these appearances were considerably stronger; but on cooling still more they were less and less.

Exp. 2. He repeated this last experiment, with the difference only of having the balls electrified minus instead of plus.* And in this case (as might be expected) the effects were reversed; the balls acceding towards the convex side, and receding from the plain side.

Exp. 3. He presented the convex side to the flame of a candle, but not so near as to touch the flame, and held it there about one minute, during which time the stone acquired a plus electricity on both sides, for the balls receded from them, but rather with a greater force from the convex side, than from the plain side.

Exp. 4. After a short time, the tourmalin being colder, and the remaining heat more equally diffused, it changed its last state to a plus and minus one; for the plain side made the balls accede, and the convex side made them recede.

Exp. 5. He now held the plain side as near the flame as he had done the convex one; and instead of both sides being electrified plus, they were electrified minus, for each side caused the balls to accede.

Exp. 6. After the same length of time as in the fourth experiment, the tourmalin being colder, it changed its state also; for the convex side made the balls recede, and the plain side continued to make them accede, as they had done before. He was naturally led next to examine at which surface of the tourmalin the fluid entered (if any did), while it continued heating.

Exp. 7. Flame being improper for the purpose, because the electric fluid is readily dissipated by its presence, he made choice of an iron rod, at the end of which was a round knob. This was heated, and afterwards brought to a certain distance from the balls, in order to see if they were moved by it: but not perceiving the least motion, he interposed the tourmalin, with the convex side next the balls. They acceded a little, and when he removed the heated iron, they returned to their place again. He then brought the iron nearer to the tourmalin than before: the balls in this case moved with vigour towards the stone, and continued in contact with it for a considerable time; and after that they receded from it. On examining the balls they had lost all their plus electricity, and were electrified minus. He also observed that the stone itself was minus on both sides. He gathered from this experiment, that the electric fluid flowed from the balls towards the stone; because they not only lost their own plus electricity, but were electrified minus; and as the tourmalin was minus also on both sides, a quantity of electric fluid must have flowed from it towards the iron.

* He did the same thing in every experiment where there was occasion to use these balls, to be more certain of the conclusions.—Orig.

Exp. 8. When he heated the convex side in the same manner as he did the plain one, the balls were not moved towards the tourmalin, but from it, and continued in that state. In this experiment they were electrified plus, and the stone also was plus on both sides.

Exp. 9. When the plain side of the tourmalin was exposed to the heated end of the glass, in like manner as it was to the knob of iron in the 7th experiment, he observed that about 3 inches of the heated part of the glass was electrified minus, and beyond that distance it was electrified plus, and continued so even when the glass was very nearly cold.

Exp. 10. He was now eager to try what would be the event, when the heated part of the glass was next the convex side of the tourmalin. On making the experiment he found that the tube was electrified minus above one foot in length, without the least appearance of a plus electricity beyond the minus one, as in the last experiment. And this minus appearance continued also when the tube was nearly cold. Now because the tourmalin was plus, the balls plus also, and the heated glass minus, the electric fluid must have flowed from the glass to produce a plus electricity in the stone and balls.

His next step was to put the tourmalin into its natural state (as Æpinus calls it), in order to make further experiments.

Exp. 11. For this purpose he separated the tourmalin from the wax, and placed it in boiling water for a short time, where it was surrounded on all sides with a conductor and an equal degree of heat: then taking it out of the water, he laid the convex side (after it was dry) on the slip of wood, supported by wax, to which the pith balls were suspended; but no appearance happened, for the balls continued at rest.

Exp. 12. But when the stone remained on the wood a little time longer, the balls separated to a considerable distance, sometimes near 2 inches; and remained so for more than one minute.

Exp. 13. If while the stone continued resting on the wood, he brought his finger near the plain side of the tourmalin, the balls receded farther from each other; and when he repeated the approach, it every time affected the balls and made them recede a little more, unless the tourmalin was become too cold; in which case they approached nearer, but still continued to be electrified plus.

Exp. 14. On removing the stone by degrees from off the wood, the balls approached nearer and nearer: but when it was taken away entirely they receded again, and in this case were electrified minus instead of plus.

Exp. 15. After heating the stone again in boiling water, he laid the plain side on the wood. In this case the balls continued at rest as they did in exp. 11, when the convex side laid upon the wood.

Exp. 16. But after a little time the balls separated an inch or more, and re-

mained so for some time. In this state they were electrified minus; for they receded from the amber.

Exp. 17. On bringing the finger near the convex side, the balls receded farther from each other, as they did in the 13th experiment: and on repeating the approach, the balls receded a little more, unless the stone was become too cold.

Exp. 18. On removing the tourmalin the least from the wood, the balls approached nearer each other, and continued to do so as the stone was removed farther off; yet they were electrified minus, though in a less degree.

Exp. 19. He then removed it entirely, and the balls receded, but to a greater distance than at any time before in the preceding experiments; and instead of being minus, they were now plus; for the amber caused them to accede.

Exp. 20. The tourmalin being again fixed to the wax, he gave the convex side one slight rub with the finger, and observed that both sides were electrified plus. When the tourmalin was put into its natural state, so as to electrify plus and minus, he gave the same side another slight rub; and in this case both sides were electrified plus. On repeating these last two experiments with the plain side instead of the other, the tourmalin was electrified plus on both sides likewise; but with this difference, that now they were considerably more electrified than before.

Exp. 21. To electrify glass minus, he made use of the same glass, and when it was a little warmed as before, held it within 2 feet of the prime conductor, which was electrified plus. By this method, that part of the glass, which was opposed to the conductor, became electrified minus on both sides; but beyond that a considerable part all round the minus, was electrified plus on both sides. This effect is of the same kind with that mentioned in the 9th experiment. In a few minutes, the minus electricity disappeared, and the plus continuing, diffused itself into the place of the other; so that now the whole was electrified plus.

Exp. 22. The experiment so far succeeding, induced him to make use of a less piece of glass, that he might have the whole electrified minus. On making the experiment, it answered accordingly.

Exp. 23. He exposed the small piece of glass to the prime conductor, at the distance of 2 feet, which was the same as before in the 21st experiment, and observed a minus electricity at both surfaces.

Exp. 24. As he moved the glass nearer, to a certain distance, it was more sensibly electrified minus; and after that, on moving it still nearer, the minus appearance was less and less sensible, till it came within the distance of about one inch, and then it was electrified plus on both sides.

Exp. 25. This plus electricity in the glass he found might be changed to a minus once again, by removing the glass, and holding it for a time at a greater distance: which is another proof of the repulsive power of this fluid.

Exp. 26. The tourmalin afforded like appearances when in the same circumstances; with this difference, that they were caused at greater distances than those of the glass; and particularly the plus electricity was acquired at the distance of one foot or more.

Exp. 27. Having by him a pane of glass, one side of which was rough, and the other smooth, he rubbed it slightly on the rough side; on which both sides were electrified minus.

Exp. 28. He treated the other side in the same manner; after which the minus electricity was changed to a plus one on both sides.

Exp. 29. He then tried whether he could not by rubbing make one side of this glass plus, and the other minus at the same time. This he effected, after both sides were made plus; for by rubbing the rough side of the glass less than the smooth side, that became minus, and the smooth side continued plus. He rubbed the rough side less, because he found from experience that rough glass required a less power to electrify it minus, than smooth glass did to electrify it plus; and therefore he concluded, that the medium on the different surfaces has different powers, the greatest belonging to the smooth, and the least to the rough surfaces; as Sir Isaac Newton has shown concerning light falling on polished and rough glass.

Mr. W. remembered an observation of the like kind, which Mr. Short made on having occasion to heat one of his metal speculums, behind which was fastened a wooden handle. This speculum he placed near a strong fire, with the polished surface towards the same, where it continued above an hour, without receiving the least degree of warmth. That power therefore which reflected the heat, must certainly be of the same nature with that which occasioned the knobs of light in vacuo, mentioned in the first experiment. Mr. W. had frequently endeavoured to cause alterations in that power, with a view to be better acquainted with its laws; and among other attempts he rubbed electrics against electrics.

Exp. 30. The first trial was with the tourmalin and amber, which produced a plus electricity on both sides of the stone, and a minus one in the amber. Afterwards he electrified the amber, and held it near the tourmalin; still both sides were plus: and if he rubbed the tourmalin while the amber was electrified, it continued plus. Then he rubbed the stone with glass: yet both sides of the tourmalin were plus, and the glass minus.

Exper. 31. But when the glass was electrified plus, and held near the tourmalin, as had been done before with the amber, in this case, both sides were electrified minus.

These experiments seem to show, that where electric appearances are produced by the rubbing of any two polished bodies together, that body whose substance is hardest, and electric power strongest, will be always plus, and the

softest and weakest always minus. It was from this theory, that he was desirous of trying to electrify the tourmalin minus by rubbing, not having at that time been able to do it. He fixed on a brilliant diamond for this purpose, as being the hardest body, and strongest electric, that he was acquainted with; and on rubbing the tourmalin with it, his expectations were answered: for both sides of the tourmalin were electrified minus, and the diamond plus.

Exper. 32. These experiments succeeding, he rubbed glass against glass, and found that they electrified each other: but one of them was plus, and the other minus.

Exper. 33. Two pieces of amber, treated in the same manner, were also electrified plus and minus.

Exper. 34. When he rubbed glass against amber, the former was plus, and the latter minus.

Exper. 35. Two tourmalins being rubbed against each other, one became plus, and the other minus.

As electrics rubbed against electrics, occasioned electrical appearances, he was encouraged to try what would be the effect of air, if he rubbed, or rather forced it against electrics; for he supposed the particles of air to be surrounded with a medium of the same kind as grosser bodies, which is the cause of their being so elastic.

Exper. 36. To do this, he only made use of a common pair of bellows, and having brought the tourmalin near to the end of the pipe, he found after it had received about 20 blasts, it was electrified plus on both sides. Air therefore seems to be less electric than the tourmalin.

Exper. 37. Into the place of the tourmalin he brought a pane of glass, and blew against it the same number of times as in the former experiment. When he examined both sides, they were electrified plus also, but less than the tourmalin.

Exper. 38. Amber treated in the same manner, was electrified less than the glass.

Exper. 39. He had recourse next to a smith's bellows. The difference these occasioned was only a much stronger electricity in the tourmalin. Amber was still weaker than the glass, and the glass weaker than the tourmalin.

Still having in view the medium on the surfaces of the particles of air, he considered that heat would rarify it: by which means, air having its resistance lessened, would more readily part with the electric fluid, and of consequence electrify more powerfully. *Exper.* 40. The pipe of the bellows being made red-hot, he blew against the tourmalin 12 times only. In this experiment likewise the tourmalin was electrified plus on both sides, but to a considerable degree more than was done in the 36th and 39th experiments. The hot air had

the same effect on glass; but electrified it less than the tourmalin: and amber, though like the other bodies, it suffered an increase of power by the same treatment, was electrified the least of all.

Exper. 41. When the tourmalin had received the same number of blasts against the plain side, while the finger touched the convex side, it afforded different appearances; for the plain side was electrified plus, and the other minus. After a short time, both sides were plus; and some time after this the stone recovered its natural state, the plain side being minus, and the convex plus.

Exper. 42. The convex side was now presented to the bellows in the same manner, and received an equal number of blasts. In this experiment both sides were plus, but weaker than in the last experiment; and, after a time, the stone returned to its natural state, affording a plus and minus appearance.

But it remains to be inquired into, whether the tourmalin is so disposed by nature, as to suffer the electric fluid to pass through it only in one direction, like magnetism through the loadstone, or is indifferent which way it flows.

Exper. 43. On examining a tourmalin which was flat on both sides, and polished, except on its edges, part of the edge appeared plus, and another part opposite to it minus; so that a line, drawn from the plus part through the centre of the stone to the other side, would pass through the minus part,

Exper. 44. Two smaller tourmalins, that were flat also and polished, like the other, exhibited the same appearances.

Exper. 45. Another tourmalin, which was also flat, but unpolished, afforded a 4th instance of this kind.

Exper. 46. The first of these tourmalins was afterwards polished, as well at the edges as the surface; in order to see, whether that would make any alteration; but it still retained its former electrical state.

Exper. 47. He experienced the same with another tourmalin, which had been rough likewise.

Exper. 48. He then, with a little emery, made that edge, which was plus, rough again, preserving all the rest smooth; but he could not perceive that any alteration was made by it. He did the like with the edge of the other polished tourmalin, without being able to observe any difference.

Exper. 49. As to the small tourmalin that was plain on one side, and a little convex on the other, it was plus on the plain side, and minus on the convex; which was contrary to the large tourmalin described in the beginning of this letter.

Exper. 50. He had an opportunity of trying another tourmalin, now in the British Museum, which afforded another instance of the singular disposition of this stone. This tourmalin is plain on one side, and concave on the other;

with a small flat border round it. It was square, but one of the corners had been broken off. When he examined it, the broken part was plus, and the corner opposite to the broken part minus: so that here also the electric current ran through the stone in a diagonal line.

Exper. 51. Every one of these tourmalins, except that in the British Museum, he greased all over, and, while they were warm enough to preserve the grease liquid, he tried each tourmalin separately, but found no alteration in the virtue of the stone, except weakening it a little; though it is well known, that moisture of any sort readily conducts the electric fluid; and therefore, if the tourmalin had not a fixed kind of electricity, the plus and minus observable on the two sides of the stone, must, by this treatment, have united and destroyed each other: the plus side parting with as much of the fluid as the minus, on the other side wanted to restore the equilibrium.

Upon the whole, all these experiments do most clearly prove, that the tourmalin suffers the electrical fluid to pass through it only in one direction, and so far it bears some analogy to the loadstone. And as the loadstone loses its virtue by being made red hot, he was desirous to see what would be the event in the tourmalin under the same treatment.

Exper. 52. He therefore put one of the flat tourmalins into a strong fire, for half an hour: but could not afterwards perceive the least alteration. He made the same experiment on another tourmalin, with the same success.

Exper. 53. Lastly, he heated the stone again, and, while it was red hot, he threw it into water; by which treatment the virtue of the tourmalin was entirely destroyed, and it had the appearance of being shivered in many parts, without breaking.

In regard to the internal frame of the tourmalin, we can say nothing; yet so much we have learned, by these experiments, that there are 3 different methods of heating the tourmalin, which produce different electric appearances; that different degrees of heat afford different appearances; that friction has the same effect on it, as on glass; and that the tourmalin, when it is heated properly, suffers a current of the electric fluid to pass through it in one direction only: so that the tourmalin hath, as it were, 2 electrical poles, which are not easy to be destroyed or altered; and further, that there is not any substance in nature, which we are acquainted with, that the electric fluid does not readily pass through; that there seems to be a constant flux and reflux of it in all bodies, as well in the air as in vacuo, occasioned by the alternate changes of heat and cold in every part of this globe.

XXXVI. New Experiments and Observations on Electricity. By Robert Symmer, Esq. F. R. S. p. 340.

Paper I.—*On the Electricity of the Human Body, and the Animal Substances Silk and Wool.*

Mr. S. had for some time observed, that on pulling off his stockings at evening, they frequently made a crackling or snapping noise; and in the dark he could perceive them to emit sparks of fire; and in weather favourable for electrical experiments, those appearances were more remarkable than at other times. He mentioned this observation to several friends, some of whom likewise had often perceived the snapping, and the emission of fire from their stockings on pulling them off, especially in the winter evenings: but he could not hear of any one who had taken this phenomenon into consideration in a philosophical way.

His first endeavour was to discover what sort of stockings was most proper to produce electricity. To determine this, he tried single stockings of different kinds, viz. thread, cotton, silk, and worsted, putting them on, and wearing them some time. On pulling them off, he could perceive nothing of electricity in the thread or cotton, and no remarkable degree of it in the silk and worsted. For whatever electricity these latter single stockings acquire by friction or otherwise, they immediately lose on being separated from the leg: if any electric virtue remains, it is no more than what belongs to it as an electric substance ceasing to be excited; and it is in so small a degree, as in the present case not to merit attention. In general, when Mr. S. speaks of the electricity in question, he means such a power of electricity as is obvious, and perceptible to the senses; so that the stocking, after being taken off, should appear more or less inflated; throw out an electrical wind to be felt by the bare leg; attract or repel another stocking visibly; and, on the touch, snap and emit, or receive electrical fire.

Mr. S. next proceeded to try the effect of 2 stockings on one leg. This he did with 2 of thread, cotton, worsted, and silk successively; but this produced no electrical appearance more than before. He then combined them one with another, and, running through all the different changes, found that none of those he then made use of exhibited visible proofs of electricity, except the silk and worsted together; and there indeed the electric power appeared remarkably strong. It seemed to be a matter of indifference whether the silk or the worsted was uppermost, the combination of the two was what he judged to be essential.

A circumstance to be carefully attended to, is the manner in which the stockings are to be taken off; for as to the putting of them on, it is a matter of indifference how that is performed. In taking them off, care must be had not

to separate them; for if that happens in pulling them off, all the electricity escapes. The best way is to put the hand between the leg and the stockings, and so push them off together. Nothing more remains to be done, than to pull them asunder; for then they both exhibit a degree of electricity, which when at the highest is really surprising.

Mr. S.'s first trials had by accident been made with *black* silk stockings; but afterwards making trial in like manner with *white* ones, he was surprized that they produced no electricity. He took a pair of white silk stockings, and having warmed them at the fire, put them both on the same leg. After remaining about 10 minutes, he took them off, and pulled them asunder, but discovered no signs of electricity in either. He did the same with a pair of black silk, but to no other effect. He then proceeded to the decisive trial: he put a black and white stocking on his leg, and wore them likewise 10 minutes; and in return had the satisfaction of observing, on their being pulled asunder, that each of them had acquired a stronger degree of electricity than he had before seen: they were inflated so much, that each of them showed the entire shape of the leg, and at the distance of a foot and a half they rushed to meet each other. He went through the same experiment with worsted stockings, and found that, as in silk, nothing but the combination of black and white produced electricity. As I had often experienced the power of electricity in the combination of black silk with white worsted stockings, there remained only to try that of white silk with black worsted, which answered as he expected, and seemed to complete the demonstration.

PAPER II.—*On the Electricity of black and white Silk.*

By a great number of experiments Mr. S. observed that the electricity produced between black and white silk, is stronger than that between silk and worsted of those different colours, and a great deal stronger than that between worsted and worsted: the last is so weak, except in time of frost, or when a sharp north-east wind blows, that though the effects are always of the same nature, yet they are sometimes so languid as to be scarcely perceptible. He therefore confines himself in this paper, to what is observable with regard to the electricity between black and white silk only. It is proper to mention another circumstance: having found it troublesome to electrify the stockings, by putting them as often on his leg as was requisite in making experiments, he quitted that method entirely; and satisfied himself with the degree of electricity which is excited in the stockings by drawing them on the hand: and this is to be understood with regard to all the following experiments and observations, unless when otherwise expressed. The electricity thus produced is not equally powerful with that which is excited by means of the leg; but it is sufficiently strong to answer all the purposes in view; and is attended with this advantage, that

the stockings continue longer fit for these experiments: for, like other electrical apparatuses, they must be kept clean, and free from all extraneous matter; and are therefore most to be depended on when new, or when newly washed.

After being a little aired at the fire, when the black silk is drawn single on the hand, a crackling noise is heard; and in the dark sparks of fire may be perceived, as passing between the hand and the stocking: while it is drawn backwards and forwards the crackling continues, and is most considerable on the separation of the stocking from the hand. Thus it appears, that black silk is highly susceptible of electricity; that it is produced almost instantaneously, or at least with very little friction; that most of it escapes, while the stocking is yet on the hand; and that, on the total separation, very little remains. This is similar to what happens with the glass tube, when the hand, after passing along it in one direction, repasses it in the other. But still the electricity that the stocking retains, after it is separated from the hand, is considerable enough to attract or repel little light bodies at the distance of 1 or 2 feet: some degree of inflation in the stocking is likewise perceivable; and when a non-electric is brought near it, a crackling is heard, and in the dark sparks may be seen. If 2 black stockings be drawn on the hand at a time, the appearances are much the same as before; only that the stockings, when taken off and separated, give smaller proofs of electricity, than if each of them had been single on the hand.

White silk differs much in electricity from black silk. When the white stocking is drawn separately on the hand, no crackling is heard, nor sparks of fire seen in the dark, though it be pulled backward and forward ever so often: when another white stocking is drawn on above it, nothing more appears: and, when separated from the hand, neither of them discovers any signs of electricity, excepting that, when brought within a few inches of Canton's pocket electrometer, they attract and repel the balls a little.

If, instead of 2 white or 2 black stockings, one white, and over that a black stocking be drawn on the hand, they discover not the least signs of electricity while they continue on the hand, even though they should be drawn backward and forward on it several times; nor, when taken together from the hand, and presented to the electrometer, do they appear to have acquired any more than a very small degree of electricity. They must be brought within the distance of a foot, nay, sometimes of a few inches, before they have any effect on the balls: but the moment they are separated, they are found to be both of them highly electrified, the white positively, and the black negatively. The circumstances that appear the most to merit observation, are as follow:

1°.—When the electrometer is placed on a non-electric, and the black stocking is presented to it at the distance of 3, 4, or 5 feet, according as it happens to be more or less powerfully electrified, the balls begin to be visibly attracted, and

3

when it is brought nearer, they are seen to be agitated in a violent manner. If, instead of the black, the white stocking be presented at the same distances, it is found to have precisely the same effects, attracting and agitating the balls in the very same manner; whence it appears, that whatever difference there was between the electricity of the black and the white, under other circumstances, they each of them acquire an equal degree of electricity, by being electrified together.

2°.—When the electrometer is supported by glass, and the white stocking is presented to it, it first attracts the balls, and afterwards repels them; when taken away, it leaves them in a repulsive state with regard to each other; when brought back, it repels them as before. If, instead of the white, the black be now presented, the balls are immediately attracted, soon after again repelled, and left once more in a repulsive state with regard to each other. If the white be again presented, the same train of effects takes place as before; and so on, alternately, as in the case of the clear and opaque glass tubes, when excited: the white stocking answering precisely to the clear, and the black to the opaque tube, and acting the one positively, the other negatively, at full as great a distance, and as forcibly, as the tubes.

3°.—Both the stockings, when held at a distance from one another, appear inflated to such a degree, that, when highly electrified, they give the entire shape of the leg; and when brought near the face, or any naked part of the body, there is a sensation felt as if a cool wind was blowing on that part. When the two white, or the two black, are held together by the extremities, they repel one another, and form an angle, seemingly of 30 or 35 degrees.

4°.—When a white and a black stocking are presented to each other, they mutually attract, with a force answerable to the degree of electricity they have acquired; when brought within the distance of 3 feet, they usually incline towards each other: within $2\frac{1}{2}$ or 2 feet, they catch hold of each other; and when brought nearer, they rush together with surprising violence. As they approach, their inflation gradually subsides; and their attraction of foreign objects diminishes: when they meet, they flatten, and join as close together, as if they were so many folds of silk; and then the balls of the electrometer are not affected at the distance of a foot, nor even of a few inches at certain times. But what appears most extraordinary is, that when they are separated, and removed at a sufficient distance from each other, their electricity does not appear to have been in the least impaired by the shock they had in meeting. They are again inflated, again attract, and repel, and are as ready to rush together as before. When this experiment is performed with two black stockings in one hand, and two white in the other, it exhibits a very curious spectacle; the repulsion of those of the same colour, and the attraction of those of different colours, throws them

into an agitation that is not unentertaining, and makes them catch each at that of its opposite colour, at a greater distance than one would expect. When allowed to come together, they all unite in one mass: when separated, they resume their former appearance, and admit of the repetition of the experiment as often as you please; till their electricity, gradually wasting, stands in need of being recruited.

5°.—When they are separated from each other, they lose their power very soon, much as the excited tube does; but when they are together, they retain it for an hour or two, and longer, when the air is in a state favourable for electricity. While they are asunder, and any non-electric is brought near them; if that non-electric is of a broad surface, it is with difficulty they are discharged of their electricity; but if the point of any, especially of a metallic, body, be presented, they are instantaneously deprived of their electrical virtue: but if they be in conjunction together, they retain their electricity with so much obstinacy, that even the sharpest point of metal cannot deprive them of it. In this, and in some other respects, there appears to be such a resemblance between the Leyden phial, or the electrical pane of glass, and the black stocking in conjunction with the white, especially when the one is within the other, that Mr. S. considers them both in the same light. In both cases, the positive electricity is on the one side, and the negative on the other; and the stockings, as well as the phial, and the pane of glass, are at once electrified positively and negatively. In both cases there is an accumulation of electricity, and a retention of it, far beyond what is to be met with in a simple body, electric or non-electric. There is however a very remarkable difference between them in two respects. In the phial, and in the pane of glass, an explosion is always obtained by carrying on a communication between the two sides by the interposition of a non-electric; but in the case of the black stocking and the white, he never could procure an explosion, nor so much as a speedy discharge, while the one was within the other. On the other hand, the phial and the pane of glass afford no opportunity of separating the positive from the negative electricity, so as to show them entire and distinct from each other; whereas we need only pull the stockings asunder, and then in the white we find the positive, and in the black the negative electricity.

6°.—When the stockings are separated, and in the dark, on presenting to them the point of one's figure, or any small metallic body, rounded at the end, they exhibit the appearance of electrical fire or light, according to the negative or positive state of the stocking the object is presented to. With the black, at the distance of 2 or 3 inches, there appears to dart from the end of the finger a sprig or pencil, as it were, of fiery sparks, which dilates in its progress, and strikes against the surface of the stocking: at the same time a crackling or snapping noise is heard. When the first discharge is made, on presenting the finger

to a fresh part of the stocking, the same phenomenon is repeated, till you have traversed the whole length of the stocking, which, when the finger moves slowly, usually yields 8 or 10 distinct discharges, before it is divested of its electricity. With regard to the white stocking, the same appearances hold, but with this difference, that instead of sparks of fire issuing from the finger, a little globule of white or bluish light is seen at the point of it; and when the electricity is strong, that little body of light seems to break in an explosion between the stocking and the finger; and rather a hissing than a crackling noise is heard.

7°.—The electrical phial may be charged by the stockings, either positively or negatively, according as the wire from the neck of the phial is presented to the white or the black; and in the one or the other case, the hissing, or the crackling noise, is louder than when any common wire, or non-electric body, is presented: but if the electricity of the white stocking be thrown into the phial, and on that the electricity of the black, or vice versâ; in that case, the phial will not be electrified at all.

The charging of the phial was among the first of Mr. S.'s experiments with electrified stockings. By some trials he made in the month of December last, he found it would succeed. One frosty evening in that month, having thrown into a small phial, filled with quicksilver, the electricity of one black stocking, he received from the explosion a smart blow on his finger. With the electricity of 2 stockings, the blow reached both his elbows; and by the means of 4, he kindled spirits of wine in a tea-spoon, which he held in his hand, and at the same time felt the blow from the elbows to the breast. It may not, however, be improper to observe, that the electricity, in this case, was excited by means of the leg.

From what has been said, it is evident that all the remarkable appearances of electricity, hitherto discovered, may be exhibited by a simple apparatus of black and white silk. But this is not all: in the course of the experiments something urious has occurred, of which no notice had been taken by others: viz. a strong cohesion produced by electricity.

Paper III.—*Of Electrical Cohesion.*

By experience Mr. S. found that the same pair of stockings did not always do equally well, even independently of the weather; and that, by being too frequently electrified at a time, their virtue appeared to diminish. He therefore judged it proper to be provided with changes of pairs; and that there might be the greater conformity between the experiments, he chose them as exactly as possible of the same size and substance. The sort he fixed on, was what is called half gauze; the weight of the white stocking, at an average, 18 dwt. 10 gr. but when dyed black, 1 oz. 1 dwt. the weight being increased, by the dying

of that colour, above 5 dwt. in the pair. When the white and the black stocking were warmed at the fire, so as to be prepared for electricity, they usually lost about a 20th part of their weight; so that in the course of the experiments he rated the white at $17\frac{1}{2}$ dwt. and the black at 1 oz. The scale, with the silk lines that belonged to it, and the hook, was adjusted precisely to the weight of 1 oz.; and as he commonly measured the strength of cohesion by fixing the hook to the black stocking, and taking hold of the white, he had only to make an allowance of 2 oz. more than the weights put into the scale, so as to take the precise weight the stockings could raise by the power of cohesion.

He measured this power two different ways; the first whilst the one stocking was still within the other; 2dly, when separated, and the one afterwards applied externally to the other. In the first of these cases, it may be thought that an allowance should be made for the friction in pulling the stockings asunder; but that appeared to be very inconsiderable; for when those of the same colour were put one within the other, and inverted, they dropped asunder of themselves; or if there was any entanglement about the heel, a little shaking disengaged and separated them: however, if it should be thought proper, the allowance of an ounce may be made, by deducting so much from the weight respectively found.

In the experiments made to measure the force of electrical cohesion, he always found it answerable to the degree of electricity at the time excited. When the stockings have been but weakly electrified, he found them unable to support the weight, the one of the other. When in a more powerful state of electricity, they would raise, respectively, from 1 to 12 ounces and upwards; nay, once he found the cohesion so strong as to move 17 ounces, including the scale and the black stocking.

The greatest weight he had been able to raise by the force of electrical cohesion, has been 17 ounces. Now the white stocking, which weighed but $17\frac{1}{2}$ dwt. bore all this weight: in this case therefore it raised, by the strength of its cohesion with the black, nearly 20 times its own weight. And if we consider that the force applied to separate them, acted in a direction parallel to the surfaces, by which they cohered; and that when the surfaces are smooth, a force acting in such direction, has much greater influence in separating bodies, by making them slide gently over one another, than if those bodies were rigid, and the force employed to separate them acted in a direction perpendicular to the cohering surfaces; when we consider this, it will be hard to determine how great the strength of their cohesion may be.

The force with which the black and the white stocking cohere, is not the only thing remarkable in their junction. The solution of that cohesion, and the different degrees of tenacity, according to different circumstances, afford some curious observations. When the black and the white stocking are in co-

hesion with each other, if another pair, more highly electrified, be separated, and presented to the former still in conjunction, the black to the white, and the white to the black; in that case, the cohesion of the first pair will be dissolved, and each stocking of the second, will carry off that of its opposite colour adhering to it. If the degree of electricity of both pairs be equal, the cohesion of the first pair will be weakened, but not dissolved; and all the 4 will cohere, forming as it were one mass. If the 2d pair be but weakly electrified, the cohesion of the first pair with one another will be but little impaired, and that of the stockings of the 2d with those of the 1st, will be weak in proportion. And lastly, if the 2d pair be not at all electrified, or if, in their stead, any other body not electrified be presented, there will be no effect produced on either hand.

White silk and black, when electrified, not only cohere with each other in the manner shown above, but when in a high degree of electricity, are found, both one and the other, to adhere to bodies of broad and even of polished surfaces, though those bodies be not electrified. This adhesion he discovered accidentally. While he was about some electrical experiments, having accidentally thrown a stocking, that was highly electrified, hastily out of his hand, he was surprized to find it some time after sticking against the paper-hangings of the room. This led him to make the following experiments.

He presented the white and the black silk, highly electrified, and in cohesion with each other, to the hangings; but no effect was produced. He then separated the black from the white, and presented them singly; in that case each of them readily adhered to the hangings, which they likewise did when flung from a little distance, and continued there for near an hour before they dropped. Having stuck up the black and the white, in the manner above mentioned, he came with another pair of stockings, highly electrified, and applying the white to the black, and the black to the white, he carried them off from the wall, hanging on those that had been applied to them. When the 2d pair were electrified, but to a moderate degree, on applying them as above, the former immediately quitted their hold of the hangings, and dropped to the ground. The same experiments held with the painted boards of the room; and likewise with the looking-glass; to the last of which, both the black and the white silk, appeared to adhere more tenaciously, than to either of the former.

The late Mons. Du Fay, an ingenious member of the Academy of Sciences at Paris, to whom we owe some valuable discoveries in electricity, gives an account of something of this nature, in a memoir, presented in the year 1733. Electricity was at that time in its infancy; Mr. Hauksbee had a little before published an account of his experiments; which brought such surprizing appearances of electricity to light, as could not but induce the curious to turn

their eyes on that subject. In the course of those experiments, he had taken notice of something remarkable with regard to colours. Mr. Gray succeeded, and having opened a new path, made still further discoveries in electricity: he likewise, in giving an account of what he had observed, hinted at something curious with regard to colours. But neither of them appear to have come to any determined point in this matter. Mons. Du Fay, who concurred with Mr. Gray, in carrying on electrical discoveries, with a candour and ingenuity that did honour to them both, having entered on an inquiry to determine what sort of bodies were most susceptible of electricity, thought proper, in consequence of what had fallen from Mr. Hauksbee and Mr. Gray, to examine what effect the different colours had in augmenting or diminishing the electricity of different substances.

Accordingly he ranged a number of ribbands, of all the primitive colours, hanging them in the same vertical plane; and to these he applied an excited glass tube, in a horizontal direction. On this he observed, that the black was first attracted; and, as he brought the tube nearer, the white next; and the rest successively, though not always in the same order. He made another experiment, in the same view, with gauzes of different colours, through which he tried the force of an excited tube, on light bodies placed at a proper distance behind them: and from the result he was of opinion, there was something in the influence of colours. But having afterwards tried some experiments with the coloured rays of the sun as refracted by a prism, with flowers of different colours, and with white ribbands rubbed over with differently coloured substances, he began to change his opinion. He likewise had recourse to what he calls a decisive experiment: he dipped his different coloured ribbands in water; and when they were all equally wetted, he applied his tube, and found they were all equally attracted. From this last-mentioned experiment, in particular, he concluded that colours, as colours, had no effect in electricity; but that all was owing to the ingredients of the dye imbibed by the coloured body.

It is not Mr. S.'s purpose here to inquire, whether Mons. Du Fay's conclusion is well or ill founded. Whatever may be the decision of that point, he apprehends the whole of this affair has very little concern with the subject of these papers, and could have been of little use to Mr. S. had he been acquainted with it before.

Paper IV. Part I.

Of Two Distinct Powers in Electricity.

With regard to positive and negative electricity, Mr. S.'s notion is, that the operations of electricity do not depend on one single positive power, according to the opinion generally received; but on two distinct, positive, and active

powers, which, by contrasting, and, as it were, counteracting each other, produce the various phenomena of electricity; and that, when a body is said to be positively electrified, it is not simply that it is possessed of a larger share of electric matter than in a natural state; nor, when it is said to be negatively electrified, of a less; but that, in the former case it is possessed of a larger portion of one of those active powers, and in the latter, of a larger portion of the other; while a body, in its natural state, remains unelectrified, from an equal balance of those two powers within it.

All who allow of positive and negative electricity, know, that the Leyden phial, when charged, exhibits electricity in those two states, the one within, the other on the outside; and that when a communication is made between the two, by the means of a non-electric touching the coating, and at the same time approaching the wire, or vice versa, the explosion is produced, and the phial discharged. This reduces the question to a narrow compass; for if, on the discharge of the phial, we meet with proofs not only of a power acting from within to the outside, but also of a power acting at the same instant from the outside to within, then we may fairly conclude, that what is called negative electricity is, in reality, a positive active power; and that electricity, in general, consists not of one alone, but of two distinct, positive powers, acting in contrary directions, and towards each other.

The proof he offers first, is founded on the following experiment. When the phial is electrified but a little, if we touch the coating of it with a finger of one hand, and at the same time approach a finger of the other hand to the wire we shall receive a pretty smart blow on the tip of each of the fingers, the sensation of which reaches no farther: if the phial be electrified a degree higher, we shall feel a stronger blow, reaching to the wrists, but no farther: when again it is electrified to a still higher degree, a severer blow will be received; but will not be felt beyond the elbows: lastly, when the phial is strongly charged, the stroke may be perceived in the wrists and elbows; but the principal shock is felt in the breast, as if a blow from each side met there. This plain and simple experiment seems obviously to suggest to observation, the existence of two distinct powers, acting in contrary directions: and I believe it would be held as a sufficient proof by any who should try the experiment, with a view to determine the question simply from their own perceptions.

But as Mr. S. was sensible that the proof of any important point in philosophy, ought not to depend on the perceptions of this or that particular person, he judged it necessary to have recourse to experiments, the result of which might admit of no ambiguity. The fortunate discovery of M. Muschenbroek and M. Allamand, with the improvements that have since been made on it, puts it in our power to increase electricity to what degree we please. He did not there-

fore despair of the means of bringing this matter to a fair decision. He expected, that if an electrical stroke should be made to pass through a solid body, with so much force as to pierce and tear the substance of it, such marks would be left, as might enable us, with certainty, to trace the course of the electrical power in its passage through the body.

Having no apparatus of his own capable of producing such effects, he had recourse to a worthy member of this Society, doctor Franklin, who was possessed of a very good one. He had communicated all his observations to this gentleman as they occurred, and, in return, met with an ingenuity and candour that render him as estimable in private life, as the improvements he has introduced into electricity, and particularly his discovery in relation to thunder and lightning, will render his reputation lasting in the learned world. They differed in opinion with regard to the point in question; yet Mr. S. found Dr. F. ready to give him all the assistance in his power, for bringing the matter to a fair decision. Mr. S. had seen him pierce a quire of paper with a stroke of electricity; and as it had been struck several times before, he desired it might be given him, that he might at leisure examine the effects of the sundry strokes.

When Mr. S. came to do so, he observed, that at every hole which had been made through the quire, the upper and the under leaf (for the quire had been laid in a horizontal position when it was struck) were ragged about the orifice. and those ragged edges pointed mostly outwards from the body of the quire. But, what was more material, when he came to turn over the leaves, he found that the edges of the holes were bent regularly two different ways (and more remarkably so about the middle of the quire,) one part of each hole upwards, and the other part downwards; so that, tracing any particular hole as it traversed the quire, he found on one side the fibres pointed one way, and on the other side the other way; much in such a manner, as if the hole had been made in the quire, by drawing two threads in contrary directions through it. This was not all: a piece of paper, covered on one side with Dutch gilding, had been accidentally left between two leaves in the quire, and had been pierced by two different strokes. This exhibited a very remarkable appearance: where each of the strokes had been given, the gold leaf was stripped off, and had left the paper bare for a little space, in an oblong form, rounded at the ends; in which, at the distance of about a quarter of an inch from each other, appeared two points, one of them a little round hole, the other only an indent or impression, such as might have been made by the point of a bodkin. In the leaf which fronted the gilding, two such points likewise appeared, corresponding to those above-mentioned; so that the hole in the one was opposite to the impression in the other, but surrounded with little black or blueish circles. When the hole, which had been struck in the quire, was traced from above down to the gilding

(for the gilt paper happened to lie with its gilded side uppermost,) it was found to terminate on the point in the gilt paper where the impression appeared, and there the impression pointed downwards. Again, when the hole in the lower part of the quire was traced from below upwards, it was found to terminate on the point in the leaf fronting the gilding, where the impression was, and there the impression pointed upwards. The facts above-mentioned seem to leave it without doubt, that the stroke had been given at the same instant, upwards and downwards; but that the electrical power from above, and from below, had seized on the gilding, dissipated part of it in vapour, and by that means become so weak, that each of them could afterwards only make an impression on the paper, marking the respective directions of their course.

Mr. S. communicated these observations to Dr. Franklin; but as no conclusion can, with certainty, be drawn but from facts, confirmed by repeated trials, he desired to have the satisfaction of making a few experiments with the Dr. in regard to this matter; to which he readily consented. For that purpose Mr. S. waited on him one morning about the middle of June; and the better to ascertain what was essential in the facts, he varied the circumstances a little from those above.

In the middle of a paper book of the thickness of a quire, he put a slip of tin foil; and in another of the same thickness he put 2 slips of the same sort of foil, including the 2 middle leaves of the book between them. On striking the two different books, the effects were answerable to what he expected. In the first, the leaves on each side of the foil were pierced, while the foil itself remained unpierced; but, at the same time, he could perceive an impression had been made on each of its surfaces, at a little distance one from another; and such impressions were still more visible on the paper, and might be traced as pointing different ways. In the 2d, all the leaves of the book were pierced, excepting the 2 that were between the slips of foil; and in these 2, instead of holes, the two impressions, in contrary directions, were very visible.

Mr. S. afterwards repeated these experiments with another similar instrument, and he met with nothing but what confirmed him in the opinion of 2 distinct counteracting powers. All the remarks he had been able to make in the repetition of experiments, that need to be added to what he had before observed, may be reduced to the 3 following.

1°. When a quire of paper, without any thing between the leaves, is pierced with a stroke of electricity, the 2 different powers keep in the same track, and make but one hole in their passage through the paper: not but that the power from above, or that from below, sometimes darts into the paper at two or more sundry points, making so many holes, which, however, generally unite before they go through the paper. What he means is, that he never yet could observe

the two powers to make different holes in the paper; but that they always keep the same common channel, rushing along it with inconceivable impetuosity, and in contrary directions. They seem to pass each other much about the middle of the quire; for there the edges are most visibly bent different ways: whereas in the leaves near the outside of the quire, the holes very often carry more the appearance of the passage of a power issuing out, and exploding into the air, than of one darting into the paper.

2°.—When any thin metallic substance, such as gilt-leaf, or tin-foil, is put between the leaves of the quire, and the whole is struck; in that case the counteracting powers deviate from the direct track, and leaving the path they would in common have taken through the paper only, make their way in different lines to the metallic body, and strike it in two different points, distant from each other about a quarter of an inch, more or less (the distance appearing to be least when the power is greatest); and whether they pierce, or only make impressions on it, in either case they leave evident marks of motion from two different parts, and in two contrary directions. It is this deviation from a common course, and the separation of the lines of direction consequent on it, that affords us the strongest proof of the exertion of two distinct and counteracting powers.

3°.—When two slips of tin-foil are put into the middle of the quire, including two or more leaves between them, if the electricity be moderately strong, the counteracting powers only strike against the slips, and leave their impressions there. When it is stronger, we generally find one of the slips pierced, but seldom both; and from what is observed in such cases, it would seem as if the power which issued from the outside of the phial, acts more strongly than that which proceeds from within, for the lower slip is most commonly pierced; but that may be owing to the greater space the power from within has to move through, before it strikes the paper.

PAPER IV. PART II.—*Of two Distinct Powers in Electricity.*

The notion of two distinct electrical powers, acting in contrary directions, may appear to some to be the same with that of the effluence and affluence of electrical matter, which M. l'Abbé Nollet gives as the general cause of the phenomena of electricity. It may therefore be not improper to take a nearer view of these two opinions, to see how far they agree, and in what they differ.

This ingenious author had observed that when a body is electrified, a current of electric fluid issues from it, and in the form of diverging rays spreads through the air, and enters into other bodies; and that, at the same time, a current of electric fluid, issuing from other bodies, passes through the air, and in the form of converging rays enters into the body electrified. Hence he concludes, that a continued, and (to use his own terms) simultaneous effluence and affluence of

a fluid matter, extremely subtile, constitutes electricity. On this principle he endeavours to account for all the phenomena that attend the electrification of bodies.

What M. Nollet has observed with regard to two contrary currents in electricity, is by no means inconsistent with the principle of two distinct counteracting powers. On the contrary, the existence of two such currents is, according to Mr. S.'s opinion, a necessary consequence of the exertion of those powers from one body on another. It is a phenomenon of electricity only; not the principle on which all electrical appearances depend. But a more essential difference takes place between this gentleman's opinion and Mr. S.'s: he represents the two currents as consisting but of one and the same fluid; admits but of one kind of electricity; and maintains that two bodies cannot be said to be differently electrified, but as they are electrified in a higher or lower degree. On the other hand, it is Mr. S.'s opinion, that there are two electrical fluids (or emanations of two distinct electrical powers) essentially different from each other; that electricity does not consist in the efflux and afflux of those fluids, but in the accumulation of the one or the other in the body electrified; or, in other words, it consists in the possession of a larger portion of the one or of the other power, than is requisite to maintain an even balance within the body; and lastly, that according as the one or the other power prevails, the body is electrified in one or in another manner.

On the Force of Electrical Cohesion. By Dr. John Mitchell. p. 390.

Dr. M. happening to be at Mr. Symmer's, he desired him to be witness to some electrical experiments he was about to make with silk stockings, of a particular kind, which he had received for that purpose. The weather was then remarkably favourable for electricity, being clear and dry, with a sharp frost, which had continued 5 or 6 days. The wind was easterly, and had been in that quarter for 10 days. It was about noon when we made our experiments: the barometer at 30, and Fahrenheit's thermometer at 32.

The stockings above-mentioned were wove of carded and spun silk, and were more substantial and weighty than those with which he had made the experiments mentioned in his 3d paper. One pair was of a deep black, having been twice dyed, to improve the colour. Another pair was of the natural colour of the silk, of a dusky white, and both new. The pair of black weighed 4 oz. 8 dwt. 4 gr., and the white 3 oz. 18 dwt. 15 gr.

They began with making a few experiments with the thin stockings formerly used: and found the result to be much the same with what is related by Mr. Symmer in his 3d paper; that is, that when the white stocking was put within the black, or vice versâ, and both highly electrified, taking hold of the one,

while a scale with weights was put to the other, they could raise 17 oz. before the stockings separated. They then repeated one or two of those experiments with some little variation of circumstances. They turned one of the stockings inside out, and put that within the other; the inner or rough sides of the stockings being thus together, by which means they took faster hold of each other; and they now found, that it required the weight of 20 oz. to separate them. When the stockings were separated, and applied externally to each other, they then raised the weight of 10 oz.

They next proceeded to try the force of electrical cohesion with the stockings of a more substantial make, viz. those above described; and there they found it to be much more considerable, as appears by the following experiments.

1°.—When the white stocking was put within the black (without either of them being turned inside out) so that the outside of the white was contiguous to the inside of the black, they lifted 9 lb. wanting a few pennyweights. Now, taking the weight of the stocking to be 1 oz. 18 dwt. 15 gr. viz. the half of the weight of the pair as mentioned above, it follows that, by the force of its cohesion with the black, it raised 55 times its own weight.

2°.—When the white was turned inside out, and put within the black, their inner or rough sides being contiguous, they lifted no less than 15 lb. $1\frac{1}{2}$ dwt. before they separated: so that, in this case, the single stocking raised 92 times its own weight.

3°.—When the inner stocking was drawn out, and applied to the outside of the other, they lifted $1\frac{3}{4}$ lb.; that is, between 10 and 11 times the weight of the white stocking.

XXXVII. Some Observations relating to the Lyncurium of the Ancients. By William Watson, M. D., F. R. S. p. 394.

To determine the substance, denominated lyncurium by the ancients, has been the occasion of much controversy among the more modern naturalists; some of whom, as the late Dr. Woodward, believed it to be a species of belemnites; others, as the late M. Geoffroy, considered it as amber. But it is evident from Theophrastus's description of the lyncurium, which is the most complete that is come down to us, that neither the one nor the other of the beforementioned substances could be what he intended. His words are, Καὶ τὸ λυγκουριον. καὶ γαρ εκ τουτου γλὺφεται τα σφραγίδια. καὶ ἔστι στερεωτάτη, κάθαπερ λίθος, ἕλκει γὰρ ὥσπερ τὸ ἤλεκτρον. οἱ δε φάσιν ου μόνον κάρφη καὶ ξυλον, αλλὰ καὶ χαλκὸν καὶ σίδηρον, εαν ᾖ λεπτός. ὥσπερ καὶ Διοκλῆς ἔλεγεν. Ἔστι δέ διαφανής τε σφόδρα καὶ πυῤῥα. . . . . γίνεται δὲ καὶ κατεργασία τίς αυτου πλείων. Hence we learn, that "the lyncurium was a stone used for engraving seals on; that it was very hard; that it was endowed with an attracting

power like amber; and that it was said, and by Diocles among others, to attract not only straws and small pieces of wood, but also copper and iron, if beaten very thin; that it was pellucid, and of a deep red colour; and required no small labour to polish it." The rest of Theophrastus's description is taken up with the fabulous account of the generation of this stone, "that it is formed by the urine of the lynx, which the animal, as soon as it parts with it, hides, and scrapes the earth together over it; and that the stones vary according to the sex and disposition of the animal."

Dioscorides, in his History of the lyncurium, gives us only the fabulous history of its generation, before mentioned by Theophrastus; and subjoins, that it is called by some Ἤλεκτρον πτερυγοφόρον; that is, amber, which attracts feathers to it. Pliny, in his history, disbelieves both the fabulous account of the generation of the lyncurium, as well as its attractive quality, related both by Diocles and Theophrastus, and considers the whole as a falsity; though he is candid enough to confess, that neither himself, nor any one else in that age, had seen a gem of that appellation.

Theophrastus, though more ancient, is, in most particulars, more to be depended on than either Dioscorides or Pliny. He ought to be considered much more of an original author, and one who wrote from his own knowledge than the others, who, valuable as they are, must be regarded, in most respects, as compilers. His account then of the appearance and properties of the lyncurium must be considered, in order to examine if any substance, known in our time, answers his description. But, first, it is plain that Dr. Woodward's hypothesis of the belemnites being the lyncurium, was ill founded; inasmuch as the belemnites is neither pellucid nor fit for engraving seals on, on account of the friability of its texture; neither can it, by any management, be made to attract straws, chips of woods, or other light bodies. Nor is Geoffroy's opinion less liable to exception; as amber, though it has the attractive power mentioned by Theophrastus, yet it has by no means the firm texture requisite to have seals engraved on it; neither is it so very hard, as is expressly said by this author concerning the lyncurium, as to require great labour in polishing it. Add to these, that Theophrastus has given a particular account of the history and properties of amber separately, in the before-mentioned work.

Dr. W. thinks it probable that what we now call the tourmaline was the lyncurium of Theophrastus, as it agrees with that author's description in all its sensible qualities: to wit, that it is a very hard pellucid stone, of a deep red colour; that it is very proper to engrave seals on; that it attracts, like amber, not only straws, and light pieces of wood, but filings of iron and brass, as has been lately evinced by many experiments. And that this stone, though not much attended to by us till very lately, is very common in several parts of the East Indies, and

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more particularly in the island of Ceylon, where it is called by the natives tournamal.

The first account we have had, of late years at least, of this extraordinary stone, was in the History of the Royal Academy of Sciences of Paris, for the year 1717; where we are told, that Mr. Lemery exhibited a stone, which he said was not common, and came from Ceylon. This stone attracted and repelled little light bodies, such as ashes, filings of iron, bits of paper, and such like. The publisher of that history then proceeds to give some reasons for these phenomena. Linneus, in his preface to the Flora Zeylanica, mentions this stone under the name of lapis electricus; and takes notice of Lemery's experiments before-mentioned. Notwithstanding this, no further mention was made of this stone, and its effects, till very lately. The Duke de Noya, in his letter to M. de Buffon, informs us, that when at Naples in the year 1743, the late Count Pichetti, secretary to the king, assured him, that during his stay at Constantinople, he had seen a small stone, called a tourmaline, which attracted and repelled ashes. This account the Duke de Noya had quite forgot; but, being last year in Holland, he saw and purchased two of these stones, which are there called aschentrikker. The making experiments with these called to his remembrance what formerly had been told him by Count Pichetti. With these stones he made, in company with Messrs. Daubenton and Adanson, a great number of experiments, of which the duke has favoured the public with a particular account.

In the year 1757, there were two accounts published on this subject: the one is a memoir of M. Æpinus, read to the Royal Academy at Berlin, intitled, De Quibusdam Experimentis Electricis Notabilioribus. The other is a treatise in quarto, printed at Rostock, intitled, Disputatio de Electricitatibus Contrariis. Auctore Joanne Carolo Wilke. Since which time, Dr. Heberden, having procured some of these stones from Holland, a great number and variety of experiments with them have been made here, particularly by Mr. Wilson; an account of which he has very lately communicated to the Royal Society.

XXXVIII. An Attempt to Account for the Regular Diurnal Variation of the Horizontal Magnetic Needle; and also for its Irregular Variation at the Time of an Aurora Borealis. By John Canton, M. A., F. R. S. p. 398.

The late Mr. George Graham made many observations on the diurnal variation of the magnetic needle, in the years 1722 and 1723: but declared himself ignorant of the cause of that variation, in N° 383 of the Philosophical Transactions, where many of those observations are to be found. About the year 1750, Mr. Wargentin, secretary of the Royal Academy of Sciences in Sweden, took notice both of the regular diurnal variation of the needle, and also of its

being disturbed at the time of an aurora borealis, as recorded in the 47th volume of the Philosophical Transactions; but is silent as to the cause. Mr. C. had no opportunity of making observations of this sort himself, till the latter end of the year 1756; but, after that time, he made near 4000, with an excellent variation-compass, of about 9 inches in diameter. The number of days on which these observations were taken, was 603; and the diurnal variation on 574 of them was regular; that is, the absolute variation of the needle westward, was increasing from about 8 or 9 o'clock in the morning till about 1 or 2 in the afternoon, when the needle became stationary for some time; after that, the absolute variation westward was decreasing, and the needle came back again to its former situation, or near it in the night, or by the next morning. The diurnal variation is irregular when the needle moves slowly eastward in the latter part of the morning, or westward in the latter part of the afternoon; also when it moves much either way after night, or suddenly both ways within a short time. These irregularities seldom happen more than once or twice in a month, and are always accompanied (so far as he had been able to observe) with an aurora borealis. Thus having explained what he means by the regular and irregular diurnal variation, and showed, that this variation is generally regular; he then, in the first place, endeavours to account experimentally for the regular variation; then offers a conjecture concerning the cause of the regular variation: and, lastly, attempts to make it appear probable, that the aurora borealis arises from the same cause.

The attractive power of the magnet, (whether natural or artificial) will decrease while the magnet is heating, and increase while it is cooling; as will appear by the following experiments.

Exper. 1. About E. N. E. from a compass a little more than 3 inches in diameter, he placed a small magnet 2 inches long, half an inch broad, and $\frac{3}{20}$ of an inch thick, parallel to the magnetic meridian; and at such a distance, that the power of the south end of the magnet was but just sufficient to keep the north end of the needle to the N. E. point, or to 45 degrees. The magnet being covered by a brass weight of 16 oz. about 2 oz. of boiling water was poured into it, by which means the magnet was gradually heating for 7 or 8 minutes; and during that time the needle moved about three quarters of a degree westward, and became stationary at $44^\circ\frac{1}{4}$; in 9 minutes more it came back a quarter of a degree, or to $44^\circ\frac{1}{2}$; but was some hours before it gained its former situation, and stood at 45°. N. B. The greater the power of the same magnet, the more it will lose in a given degree of heat.

Exper. 2. On each side of the compass, and parallel to the magnetic meridian, he placed a strong magnet of the size above-mentioned; so that the south ends of both the magnets acted equally on the north end of the needle, and kept it

in the magnetic meridian; but if either of the magnets was removed, the needle was attracted by the other, so as to stand at 45 degrees. The magnets were both covered with brass weights of 16 oz. each. Into the eastern weight he poured about 2 oz. of boiling water; and the needle in one minute moved half a degree, and continued moving westward for about 7 minutes, when it arrived at $2^\circ\frac{3}{4}$. It was then stationary for some time; but, in 24 minutes from the beginning, it came back to $2^\circ\frac{1}{2}$, and in 50 minutes to $2^\circ\frac{1}{4}$. He then filled the western weight with boiling water, and in one minute the needle came back to $1^\circ\frac{1}{4}$; in 6 minutes more it stood half a degree eastward; and after that, in about 40 minutes, it returned to the magnetic north, or its first situation.

It is evident that the magnetic parts of the earth in the north on the east side, and the magnetic parts of the earth in the north on the west side of the magnetic meridian, equally attract the north end of the needle. If then the eastern magnetic parts are heated faster by the sun in the morning than the western, the needle will move westward, and the absolute variation will increase; when the attracting parts of the earth on each side the magnetic meridian have their heat increasing equally the needle will be stationary, and the absolute variation will then be greatest; but when the western magnetic parts are either heating faster, or cooling slower than the eastern, the needle will move eastward, or the absolute variation will decrease; and when the eastern and western magnetic parts are cooling equally fast the needle will again be stationary, and the absolute variation will then be least. This may be still further illustrated by placing the compass and two magnets, as in the last experiment, behind a screen near the middle of the day in summer; then, if the screen be so moved that the sun may shine only on the eastern magnet, the needle will sensibly vary in its direction, and move towards the west; and if the eastern magnet be shaded while the sun shines on the western, the needle will move the contrary way. By this theory, the diurnal variation in the summer ought to exceed that in the winter; and accordingly it is found by observation that the diurnal variation in the months of June and July is almost double that of December and January.

The irregular diurnal variation must arise from some other cause than that of heat communicated by the sun; and here Mr. C. has recourse to subterranean heat, which is generated without any regularity as to time, and which will, when it happens in the north, affect the attractive power of the magnetic parts of the earth on the north end of the needle. Dr. Hales has a good observation on this heat, in the Appendix to the second volume of his Statical Essays, viz. "that the warmth of the earth, at some depth under ground, has an influence in promoting a thaw as well as the change of the weather from a freezing to a thawing state, is manifest from this observation; viz. Nov. 29, 1731,

a little snow having fallen in the night, it was by 11 the next morning, mostly melted away on the surface of the earth, except in several places in Bushy-Park where there were drains dug, and covered with earth, where the snow continued to lie, whether those drains were full of water or dry; as also where elm-pipes lay under ground; a plain proof that these drains intercepted the warmth of the earth from ascending from greater depths below them; for the snow lay where the drain had more than 4 feet depth of earth over it. It continued also to lie on thatch, tiles, and the tops of walls."

That the air nearest the earth will be most warmed by its heat is obvious; and this has frequently been taken notice of in the morning, before day, by means of thermometers at different distances from the ground, by Dr. Miles, at Tooting in Surrey; and is mentioned in p. 526, of the 48th volume of the Philosophical Transactions.

The aurora borealis, which happens at the time the needle is disturbed by the heat of the earth, is supposed to be the electricity of the heated air above it; and this will appear chiefly in the northern regions, as the alteration in the heat of the air in those parts will be greatest. This hypothesis will not seem improbable, if it be considered that electricity is now known to be the cause of thunder and lightning; that it has been extracted from the air at the time of an aurora borealis: that the inhabitants of the northern countries observe the aurora to be remarkably strong, when a sudden thaw happens after severe cold weather; and that the curious in these matters are now acquainted with a substance, that will, without friction, both emit and absorb the electrical fluid, only by the increase or diminution of its heat: for if the tourmalin be placed on a plane piece of heated glass, or metal, so that each side of it, by being perpendicular to the surface of the heating body, may be equally heated; it will, while heating, have the electricity of one of its sides positive, and that of the other negative; this will likewise be the case when it is taken out of boiling water, and suffered to cool; but the side that was positive while it was heating, will be negative while it is cooling, and the side that was negative, will be positive.

For the sake of those who may be desirous of examining the diurnal variations of the needle very minutely, Mr. C. annexed a complete year's observations; and deduced from the regular variations during that time, the mean diurnal variation belonging to each month: whence it appears that the diurnal variation increases from January to June, and decreases from June to December.

The mean diurnal Variation for each Month in the Year 1759.

	′	″
January	7	8
February	8	58
March	11	17
April	12	26
May	13	0
June	13	21
July	13	14
August	12	19
September	11	43
October	10	36
November	8	9
December	6	58

XXXIX. On the Sections of a Solid, hitherto not considered by Geometers. By Wm. Brakenridge, D.D., F.R.S. p. 446.

On an imaginary solid that never can occur, or be of any real use.

XL. A Letter to the Hon. J. Th. Klein, Secretary to the City of Dantzic, from Mr. Peter Collinson, F.R.S., concerning the Migration of Swallows. p. 459.

Mr. C. dissents from Mr. K. in an article he takes great pains to establish; which is, that swallows are not birds of passage; but at the time of their disappearing retire under water, and live therein all the winter. This Mr. C. cannot comprehend, being so contrary to nature and reason; for as they cannot live in that state without some degree of breathing, this requires the circulation of the blood, however weak and languid. Now as respiration is absolutely necessary for circulation, how is it possible to be carried on for so many months under water without the risk of suffocation? Besides, if so remarkable a change was intended, the great wisdom of the Almighty Creator would undoubtedly be seen in some particular contrivance, in the structure of the organs of the heart of this bird, to enable it to undergo so very remarkable a change of elements.

An easy experiment may throw some light on this doubtful affair. At the time of their going away, take a swallow, and confine it in a tub under water: if it remains there for a week or two alive, without any remarkable inconvenience, then there may be some probability of its continuing so many months in that state. The conclusions that are drawn from some of the tribe of insects subsisting under water, are far from being conclusive to found an analogy on; as insects differ from other animals in so many particulars, that very little or nothing can be concluded or inferred of the one from what we observe in the other.

Towards the end of September, the swallows assemble on the reeds in the islands in our river Thames, and have no doubt so done for ages past; and yet Mr. C. never heard or read of any fisherman or other person that has ever found in the winter months a swallow under water in a torpid living state; for if such a marvellous thing had ever happened, it would have been soon communicated to the public. Besides, as these islands of reeds and willows are annually cut down for several uses, and yet not a swallow has been discovered in his aquatic abode; and considering the multitudes seen on these reeds and willows in the autumn; if they took their winter's residence under water, it is most reasonable to think, in a river so frequented, and in so long a course of years, some would have been found in that situation. Another circumstance is, that in great towns remote from water, where rivers and reeds are not near, it is frequently observed that a little before the swallows depart, they every morning early gather together

on the roofs of large houses, exposed to the morning sun: this they daily do for some time, to collect themselves before they take their flight.

Next, to confirm this opinion, that the migration of some species of swallows is certain, Mr. C. thinks he has some undoubted proofs. He has often heard Sir Charles Wager, first lord of the admiralty, relate that in one of his voyages home, in the spring of the year, as he came into soundings in our channel, a great flock of swallows came and settled on all his rigging: every rope was covered, they hung on one another like a swarm of bees; the decks and carvings were filled with them; they seemed almost spent and famished, and were only feathers and bones; but being recruited with a night's rest, they took their flight in the morning.

Capt. Wright, a very honest man, said the like happened to him in a voyage from Philadelphia hither. But a yet stronger confirmation of the swallows being birds of passage, is the observation in Mr. Adanson's history of Senegal, lately published; which is, as near as may be literally translated, from the author's own words; viz. 'The 6th of the same month (October) at half an hour past 6 in the evening, being about 50 leagues from the coast (between the island of Gorea and Senegal), 4 swallows came to take up their night's lodging on the ship, and alighted on the shrouds. He easily caught all 4, and knew them to be the true European swallows. This lucky incident confirmed him in the opinion he had formed, that these birds pass the seas to get into the countries of the torrid zone, at the approach of winter in Europe; and to that purpose he has since remarked, that they do not appear at Senegal but in that season. A circumstance no less worthy of note is, that at Senegal the swallows do not build nests as in Europe; but lie every night by pairs, or single, in the sand upon the sea-shore, where they rather chuse to fix their habitation than up in the country.' Hist. de Senegal, p. 67.

This observation, (as it comes from a professed naturalist, and one who went into those countries on purpose to collect what was curious in that way) seems to put the matter out of doubt; and the hearsay stories of ignorant peasants and credulous people are by no means to be put in competition with it.

Mr. C. was for many years very watchful in taking notice of the times when the swallows leave us, and had twice seen them undoubtedly taking their flight. At two different years, on the 27th and 29th of September, walking in his garden at noon, on very clear sunshiny days, and looking up into the sky, at a very great height, he distinctly saw an innumerable number of swallows, soaring round and round, higher and higher, until his eyes were so pained with looking, that he could no longer discern them.

But as Mr. Klein seems to be so positive, that the hirundo riparia, or sand

martin, at the approach of winter, retires into the holes, in which that species breed up their young, and made their summer's residence, and there pass that cold season in a dormant state, as snakes, lizards, and some other animals do, Mr. C. was the more solicitous to come at the truth. But as these sandy precipices, in which these martins build, are mostly inaccessible, some years passed before he could find a situation where the experiment could be fairly made, without difficulty or danger. Such a sand-hill he found in the parish of Byfleet in Surry. The clergyman being his friend, and well qualified to make the experiment, at his request, was so obliging to undertake it. He gives his letter in his own words.

'Byfleet, October 22, 1757.

'Dear Sir,

'I took a square of about 12 feet, over that part of the cliff where the holes were thickest, which in going down from the surface, I judged would take in about 40 holes. I set to work, and came to the holes; but found no martins, nothing but old nests in the farthest end of the holes, which were from a foot and half to two feet and half deep from the entrance. We carefully searched 40 holes, but found no birds; but at least 30 of them had nests. The passage to them was very near in a straight horizontal line; the nest was sunk about an inch and half below the level of the passage; the materials next the bottom were straws, then coarse and fine grasses; the whole structure of no great elegance. The few eggs that were left behind were of a clear unspotted white, the size of a robin-red-breast's.'

This fair trial, being made by a gentleman of veracity and ability, is very conclusive; for it certainly proves that the sand martins do not take up their winter abode in their summer dwellings. Therefore there is sufficient reason to believe from the before-recited observations on the common swallows, and this so recently made on the sand martins, that they are all birds of passage.

Additional Remark.—There are 4 distinct species of birds that go under the general name swallow; viz. the swift or black martin; 2, the swallow, that builds in chimneys; 3, the martin that builds against houses; 4, the sand martin, that builds in sand-banks. Mr. C. thinks he has clearly proved that some of these species are birds of passage. But some of his friends assert that they pass the winter in cliffs or caverns of the earth, in banks or precipices. What is much to be regretted is, that the gentlemen were not curious enough to distinguish the particular species which they found in a torpid state. Mr. Adanson, in his account of Senegal, has omitted this. So that nothing certain can yet be pronounced, which species stays, or which goes.

XLI. Observations on the Comet seen in January 1760. *By James Short, M.A., F.R.S.* p. 465.

A comet has made its appearance near the constellation of Eridanus, a little to the westward of Orion. Last night (Jan. 9) Mr. S. took its transit over the meridian, and likewise its declination. Its nucleus was small, subtending an angle of not more than 5 or 6 seconds, but very visible through a 2-feet reflector magnifying about 70 times. Its motion is to the westward, with a considerable velocity, seemingly about 2° in a day; for about an hour and a half after taking its transit, he judged it had advanced about 10 or 12 minutes; which was about the rate of the great comet, when it first was seen in the end of the year 1743. This comet is very visible to the naked eye, though no tail could be perceived; and therefore he concluded it was going down to the sun.

	Mean time.
Comet passed the meridian 9th Jan. 1760, at......	$9^h\ 8^m\ 53^s$
Its declination south	$2^\circ\ 15'\ 0''$
Rigel Orionis passed the meridian at	$9^h\ 47^m\ 49^s$

XLII. Observations on the same Comet. By the Rev. J. Michell, M.A. p. 466.

The first observations gave its distance from κ Orion, is 3° 29′; from Rigel, 11° 46′; from Betelgeuse, 17° 10′; and from Sirius, 12° 56′. All these observations were made between a quarter and half an hour past 9, and in the order here set down. At $1^h\ 22^m$, its distance from Rigel was 7° 6′; at $1^h\ 24^m$, from Betelgeuse 15° 53′; and at $1^h\ 36^m$, its distance from Sirius was 17° 36′.

XLIII. An Account of the same Comet. By Nicolas Munckley, of Lincoln's-Inn, Esq. p. 467.

Jan. 9, 1760, Mr. M. observed what appeared to be evidently a comet, west of the constellation of Orion, over the two stars marked μ and ν in the river Eridanus, but nearer the latter than the former; right ascension about 66°, declination about 3° s. It was something dimmer and larger than either of these stars; and through a telescope appeared magnified, and surrounded with a broad, faint, ill defined haziness, like the last comet, such as plainly distinguished it from any thing else in the heavens.

XLIV. Of the same Comet. By Mr. Mark Day. p. 469.

Mr. D. Jan. 9, about 5 o'clock, observed the comet in the southern hemisphere, near the northern extremity of the river Eridanus, tending towards Pegasus, and thought it would cross the ecliptic about 20° in Aries. It moved one degree in less than one hour and half; but seemed too hasty to give the astrono-

mers leave to make many observations on it, unless the weather proved favourable. It passed the meridian about 9.

XLV. On the Vitriolic Waters of Amlwch, in the Isle of Anglesey; with occasional Remarks on the Hartfell Spa, described in the 1st Vol. of the Edinburgh Essays and Observations, and in the 49th Vol. of the Phil. Trans., and their Comparison with other Waters of the same Class. By John Rutty, M. D. Dated Dublin, Feb. 15th, 1750. p. 470.

Amlwch is situated on Trasklwyn mountain, in the parish of Amlwch, in the Isle of Anglesey, the water of which was sent to Dr. R. by Ambrose Lewis, of Beaumorris, having been bottled May 31, 1757, and arrived in Dublin June 3. It appears by the hydrometer to be as light as distilled water, notwithstanding its strong impregnation. It is of a subacid taste, and very nauseously vitriolic; a lasting impression of that sort continuing in the throat giving suspicion of copper, of which however it exhibits no evidence, by any degree of the hue of that metal imparted to polished knives immersed in it, nor of sulphur, by discolouring silver. It retained the above-mentioned taste, on being exposed several days in an open vessel; quite otherwise than happens to our ordinary chalybeates. It curdled with soap: it also curdled equal parts of milk, exhibiting a pretty clear whey. With spirit of hartshorn, spirit of sal ammoniac, and the solution of potashes, it exhibited ochreous and green grumes, as the martial vitriol.

Its appearances with galls, and other austeres, were very singular; for tormentil roots gave it only a dilute ink colour, soon fading; and green tea a dark dun colour, on standing. It had not blackened the corks, except perhaps one out of 6; and when the water was first poured out it struck no more than a slightly bluish tincture with galls; which tincture, on standing all night, became like a dilute ink; but in a glass exposed 32 hours, the galls struck the dilute ink colour sooner: and in some of the water, which he left exposed 4 days, and in some of it, which he left in a phial corked, but only $\frac{3}{4}$ full, 3 weeks, the effect was very different; for to each portion of water so exposed, as before mentioned, the galls imparted a most beautiful bright sky-blue; which blue tincture, a little spirit of vitriol instantly destroyed.

Hence may be seen the fallacy of trusting to a few appearances, and the danger of rash conclusions, without repetitions of experiments and observations; which, if they had been omitted, he had been led to conclude this water to be but slightly impregnated with either a chalybeate or vitriolic principle; with which last it is however assuredly strongly saturated, though the ordinary test with galls does not discover it clearly by the blue tincture, until the dissolving acid is partly exhaled.

The Analysis.—It deposits an ochre, which serves for painting.—It also deposits in the bottles a sediment partly of a red and yellowish colour, and partly white and raggy: which sparkled, and smelt strong, on the red hot iron.—Two pounds 11 oz. yielded 16 grs. (i. e. a gallon 49 grs.) of a light green sediment, of an acid smell, and of an highly acid, vitriolic, and nauseous taste. It ferments strongly, both with solution of potashes, and with spirit of sal ammoniac; and separates a green and ochreous matter with the last. Galls added to its dilutum in distilled water turned it of a deep blue: the characteristic of martial vitriol, to which it also agrees in the experiment of the last paragraph. It produced some degree of coagulation with albumen ovi; and some slight opacity, and small grumes, with saliva. It turned of a brown greenish colour with syrup of violets. It was not attracted by the magnet, until roasted in the crucible; and then it was strongly attracted, and turned as red as minium. It appears therefore that Amlwch water is strongly impregnated with an acid martial vitriol. It kills all the fish in its passage. It has sometimes been drunk; but cannot be borne in a greater dose than ½ a pint, unless diluted with common water, being otherwise vomited up. It cures the mange in horses, and the itch in men, by bathing.

From the above account of the Amlwch water, it appears evidently to agree with that of the Hartfell, above mentioned, and described in the Edinburgh Essays, and in the Phil. Trans. even in certain distinguishing characters common to both, and in which they differ from our ordinary chalybeate waters; viz. 1. In the acid and vitriolic taste, which moreover they retain, when long kept, and at a distance from their fountains; and even on boiling, yielding an acid vitriolic salt, on exhaling to dryness, which the common chalybeates never do, but lose their strength by a small degree of heat. 2. In the blue tincture, which they give with galls; another distinguishing character of English vitriol, of which a weaker solution, like the common chalybeates, gives only the purple colour with galls. 3. In exhibiting green clouds, or grumes, with oil of tartar, like the martial vitriol.

The same, or like appearances, are exhibited by the Shadwell water; by another at Swansea in Wales; and on a late diligent search into the waters of this kingdom, by those of Kilbrew, in the county of Meath; of Ballymurtogh, in the county of Wicklow; and Cross and Coshmnore, in the county of Waterford, and some others: and to conclude, as crystals of martial vitriol have been demonstrated in several of them, Dr. R. does not hesitate to pronounce them acid vitriolic waters; which waters, as they are new in practice, and different in operation and effects from the common chalybeates, he apprehends it will be worth while to endeavour to place them in a more conspicuous point of view.

There are indeed in these waters different degrees of acrimony; for though

most of them are so acid as to curdle milk, yet this is not altogether universal; and though in many of them the acid is so far enveloped in their solid contents as to ferment with alkalies, this appearance is not always conspicuous; whence one would imagine, that the milder sort might be used with more freedom, or less danger; and yet it is certain, that one of the sharpest of them all, viz. that of Kilbrew, in the County of Meath, has been taken inwardly, with success, in some very stubborn cases.

Yet, on the other hand, it has been observed, that even the German Spa sometimes has proved too irritating in some tender constitutions, where our ordinary milder chalybeates have succeeded well; and he was informed by an accurate observer that, in some tabid cases, particularly that called the galloping consumption, the mildest and lightest of our own chalybeates, even though blended with milk, have been found to increase the hectic heats and tension of the pulse.

Now, this observation seems not easily reconcileable to another of Dr. Horseburgh, in the place above-mentioned, on the Hartfell spa (a much stronger and harsher chalybeate than either the German spa, or any of our ordinary chalybeates) viz. that it has actually been given, with notable success, from half a pint to a pint a day, in consumptions of the lungs, far advanced, even attended with hectic heats and night sweats. So memorable a fact, in the cure of a deplorable disease, deserves attention; and the Scotch physicians in that neighbourhood are called on to corroborate it by further observations; as how long those cures stood, and how far they may have been confirmed by the like success in similar cases; whether used with or without milk; and lastly, whether, as an acid austere medicine, they may cool, correct, and give a better consistence, in a colliquative state of the blood; seems well to deserve further inquiry, and that the result should be communicated for the public utility.

There had indeed formerly obtained a general prejudice against the use of the ordinary chalybeates in diseases of the lungs; but, at length, experience has convinced us not only of their safety, but usefulness, and good effects, especially when tempered with milk, in many of those cases. And moreover, it is but doing justice to our acid vitriolic waters, to acknowledge, that the empirical trials made on them by the giddy vulgar, have been frequently such as demonstrate, not only their safety, but even powerful effects in other rebellious disorders; as, particularly, the Kilbrew water (one of the sharpest and most strongly saturated with martial vitriol of all these waters yet discovered) in the notable cure of an ascites, complicated with a jaundice, which Dr. R. had elsewhere related; and he saw no reason why physicians should not, in this as well as other cases, avail themselves of the happy success of such casual experiments.

In order, therefore, to promote a view of this kind, and, as these vitriolic waters are better adapted for use than the ordinary chalybeates, as bearing carriage to remote places, and may be kept fit for use at all seasons of the year, and

are to be preferred in medical intentions, whenever the strongest of the chalybeates are required, and can be borne; he here, from facts and observations made on the several waters of this sort, which had fallen under his notice, gives a short sketch of their general operation and good effects, as a foundation for further improvements.

These waters, then, generally operate as an emetic or cathartic, or both; and have recommended themselves, in external and internal use, as a powerful detergent, repelling, bracing, styptic, cicatrizing, antiscorbutic, and deobstruent medicine, as has appeared by the notable cures they have effected, not only by external use in inveterate ulcers, the itch, mange, scab, tetterous eruptions, scald head, and sore eyes; but also by internal use in hot tetterous eruptions, dysenteries, internal hæmorrhages, in gleets, the fluor albus, and diarrhœa, in the worms, agues, dropsies, and jaundice.

Such has been the success, that has not unfrequently crowned the empirical use of these waters; which, though in some of these cases, it might undoubtedly have been better conducted in the hands of the prudent physician, may however suffice to convince us, that the vitriolic waters are a branch of the materia medica, not to be despised nor overlooked, in the cure of many stubborn chronical diseases.

XLVI. An Account of that Part of America, which is nearest to the Land of Kamtchatka; extracted from the Description of Kamtchatka by Professor Krashennicoff, 2 vols. 4to. Petersburg, 1759. p. 477.

As accounts (and some of them taken from later navigators) of the part of America here mentioned, are to be found in every modern system of geography, it was deemed unnecessary to reprint this paper.

XLVII. Remarks on the Mutations of the Stars. By Tho. Barker, Esq. of Lyndon, in Rutland. p. 498.

It is well known that there have been several alterations among the fixed stars: for instance, Ptolemy's ultima fluvii, a first magnitude star, is in Dr. Halley's catalogue of the southern constellations only a 3d magnitude: and in much less time, the δ of the Great Bear, which Bayer seems to have judged just of the same size with the other 6, is become far duller than any of them. Some stars also have quite disappeared, while again new ones, not seen before, have been discovered; and there are others periodically larger and smaller. Two very remarkably bright, yet short-lived stars, have been also seen, one in Cassiopea, the other in Serpentarius; which breaking out at once, with greater lustre than any other fixed star, gradually faded, and changing to different colours, in about a year and half were no longer visible. But no one has yet remarked that any lasting star was of a different colour in different ages; Greaves, on the contrary,

takes notice, that the colours of the stars and planets are the same now as the ancients observed, which is very true in general; for Ptolemy, in his catalogue of stars, says, Arcturus, Aldebaran, Pollux, Cor Scorpii, and Orion's Shoulder, with another to be mentioned presently, are ὑποκιρρος, reddish: and the 5 here mentioned are still of that colour, and probably the only considerable stars which are so.

But to this rule there seems to be one exception, and that in a remarkable star: for old authors mention the Dog star, which is now white, and not at all inclined to redness, as being then very much so, as in many passages of their works.

Hyginus, in distinguishing Canis from Sirius as two different stars, seems to contradict all other writers, who speak of them as one, except perhaps two or three latter ones, who directly quote Hyginus's words. Sirius, or Canis, the brightest star in the heavens, is that which Ptolemy calls in the mouth; Eratosthenes and Hyginus, in the tongue; but whether Bayer γ, which Flamsteed calls a 3d magnitude star, Ptolemy only a 4th, was in more ancient times larger, Mr. B. will not pretend to say; since Eratosthenes and Hyginus both speak of two stars in the Dog's head, as thought worthy of particular names. If in Hyginus, flammæ candorem means the whiteness of its light, as candor often does, he expressly contradicts what is mentioned by others; yet still thinks Ptolemy's authority seems greater than that of Hyginus. But candor is also used for innocence, beauty, brightness, &c.

However in most places candor is used in the same sense as in Hyginus, for brightness, without regard to colour; for so he must be understood, not only to avoid contradiction between him and Ptolemy, but from the name Sirius, which it could not be called from its whiteness, Σειριος bearing no relation to that, but to brightness, heat, or dryness; all which the ancients speak of as properties of the Dog star. Again, it is brightness wherein it excels all other stars, and not in whiteness; for Orion's foot and others are as white, but there is none so bright as the Dog star. All this is said on supposition there was but one remarkable star in the Dog's head, that in the mouth; for if there were two, as Hyginus says, we are not here concerned with either the brightness or colour of his Sirion, which was in the head, as it certainly faded before Ptolemy's time, who mentions only one, that in the mouth, and which, he says, was then red, but is now white.

XLVIII. The Method of making Sal Ammoniac in Egypt; as communicated by Dr. Linneus, from his Pupil Dr. Hasselquist, who had been lately in those Parts. By John Ellis, Esq., F. R. S. p. 504.

Sal ammoniac is made from the soot arising from the burnt dung of four-

footed animals that feed only on vegetables. This dung is collected in the first 4 months of the year, when all their cattle, such as oxen, cows, buffaloes, camels, sheep, goats, horses, and asses, feed on fresh spring grass, which, in Egypt, is a kind of trefoil, or clover; for when they are obliged to feed their cattle on hay, and their camels on bruised date kernels, their excrements are not fit for this purpose; but when they feed on grass, the poor people of Egypt are very careful to collect their dung quite fresh, and for that purpose follow the cattle all day long, in order to collect it as it falls from them; and if it is too moist, they mix it with chaff, stubble, short straw, or dust, and make it up in the form of cakes, about the same size and shape as it lies on the ground. Then they fix it to a wall to dry, till it is fit to be burnt.

For want of wood, which none but the rich in Egypt can afford to buy, they burn this dung through the whole country, and sell a vast quantity of it to the salt-makers. The excrements of the camel are not found at all preferable to any other; and its urine is never used for this purpose, though generally reported so by authors. The salt-workers pretend that the human excrements, and those of goats and sheep, are preferable to any other. The months of March and April is the only time they make the salt.

Sal ammoniac is made in the following manner: They build an oblong oven, about as long again as broad, of brick and moist dung, of such a size that the outside, or flat part of the top of the arch, may hold 50 glass vessels, 10 in length, and 5 in breadth, each vessel having a cavity left for it in the brick-work of the arch. These glass vessels are globular, with a neck an inch long, and 2 inches wide. They are of different sizes, in different salt-works, containing from a gallon to 2 gallons: but in general are about 18 inches diameter. They coat each vessel over with a fine clay, which they find in the Nile, and afterwards with straw; they then fill them $\frac{2}{3}$ full of soot, and put them into their holes on the top of the oven.

They make the fire gentle at first, and use the afore-mentioned dried dung for the fuel; they increase the heat gradually, till they bring it to the highest degree, which the workmen call hell-fire, and continue it for 3 days and 3 nights together. When the heat is come to its due degree, the smoke shows itself with a sourish smell, that is not unpleasant; and in a little time the salt sticks to the glasses, and covers the whole opening. The salt continues subliming, till the above-mentioned time is expired; then they break the glasses and take out the salt, just in the same form, and of the same substance, that it is sent all over Europe. At each salt-work they have a glass furnace to melt the old glasses, and make new ones.

XLIX. Montium quorundam præaltorum, magna ligni fossilis copia quasi infarctorum, brevis descriptio Sam. Christ. Hollmanni, Phil. Professoris Goettingensis, et S. R. Sodalis. p. 506.

In this paper it is stated that immense quantities of fossil wood are found in the lofty mountains situated in the confines of Hesse and the principality of Goettingen.

L. Experiments in Electricity: in a Letter from Father Beccaria, Professor of Experimental Philosophy at Turin, to Benj. Franklin, LL.D., F.R.S. p. 514.

To be read improved in the published works of Fa. Beccaria.

Remarks on the preceding Paper. By Benj. Franklin, LL.D., F.R.S. p. 525.

For the better understanding this paper, it is necessary to know that Father Beccaria uses a large chain, suspended by silk lines, for the purpose of a prime conductor; and that his machine for turning the glass globe is so contrived, as that he can, on occasion, readily isolate it, i. e. place it on glass or wax, together with the person that works it. When the communication is thus cut off between the earth and the chain, and also between the earth and the machine, he observes that the globe being turned, both the chain and the machine show signs of electricity; and as these signs, when examined, appear to be different in the chain and in the machine, and the globe having, as he supposes, drawn from the machine part of its natural or common quantity of electricity, and given it to the chain, he calls the electricity appearing in the chain, electricity by excess; and the electricity appearing in the machine, electricity by defect; which answer to our terms of positive and negative electricity, or electricity plus and minus. And thus his expressions, electrifying by the chain, and electrifying by the machine, are to be understood, electrifying positively, and electrifying negatively.

LI. An Uncommon Case of an Hæmoptysis. By Eras. Darwin, M. D. p. 526.

A gentleman residing near Litchfield, between 40 and 50 years of age, of a pale and meagre habit, had been daily afflicted with violent head-aches for several years; and, about 4 years before, after having taken a considerable quantity of Peruvian bark, became suddenly paralytic. The use however of his right limbs was so much restored as only to remain weaker than the other; when, on suddenly awaking from his sleep about 2 o'clock in the morning, May 7, 1759, he spit up 4 or 5 oz. of florid blood. He immediately lost 12 or 14 oz. from the arm, had elixir of vitriol given him, and in the evening had a clyster, and lost blood again to about 10 oz.

On the 8th, about the same hour, he again suddenly awaked, and spit about

the same quantity of blood as before. He was now advised to increase the quantity of elixir of vitriol, had a bolus of extractum campechense every 6 hours, and had a leech applied to a blind pile that had long appeared after going to stool. On the 9th, at the same hour, he had again the same discharge as before. That these hæmorrhages were from the pulmonary artery, rather than the bronchial, appears from the sudden exspuition, the quantity, the floridity, and from the discharge being without pain, and unmixed with phlegm.

As he had no feverish symptoms, either when he first awaked, or during the day, no more blood was taken from him; and as he constantly slept profoundly from 10 o'clock till 2, when the complaint seized him, he was now advised to be awakened, and rise out of his bed, at one in the morning, and remain awake till 3, omitting all medicines. He continued to rise from bed for a week, and has ever since used himself to awake at the same time; and has not only been entirely free from this complaint, and that without any further discharge from the hæmorrhoidal vessels; but has got more flesh, and his head-aches are become even inconsiderable. Dr. D. says, he ought not here to omit, that he had a vomit given him on the 12th, and twice repeated at the intervals of 3 or 4 days.

As the patient, from a former hemiplegia, had probably many parts of his body rendered less irritable than is natural, and as he constantly slept profoundly, and the hæmoptoe always awaked him after 4 hours sleep, Dr. D. was led to conclude that, during this sleep, the lungs were not sufficiently sensible to push forwards the whole circulation; and that hence the blood, gradually accumulated, ruptured some minute branches of the pulmonary artery, before the uneasiness became great enough to awake the patient. And as much as the evidence of a single case in medicine may be estimated, the successful cure would seem to evince the truth of this doctrine.

He adds, that the anxiety with which patients reduced to great weakness awake from their sleep, and the hurried pulse, have by others been observed to be owing to an accumulation of blood in the lungs, during their state of decreased sensibility; and how detrimental, in these cases, might be the administration of opium, or nitre; while the want of sleep, or the recurring hæmorrhage, might seem, to the unwary practitioner, to need their assistance.

After a few days, observing some cough remain, it seemed advisable to give 2 or 3 vomits; as, from late experience, they did not endanger a renewal of the discharge, and must promote the expectoration of the eschar, or any extravasated blood; which otherwise, by its delay acquiring a putrid acrimony, perhaps most frequently erodes the contiguous vessels, and, forming new ulcerations, becomes the general cause of consumptions, subsequent to accidental spittings of blood.

LII. Of the late Earthquakes in Syria. In a Letter from Dr. P. Russel, to his Brother, A. Russell, M. D., F. R. S. Dated Aleppo, Dec. 2, 1759. p. 529.

June 10, in the morning, a slight shock of an earthquake was felt here. October 30, about 4 in the morning, was a pretty severe shock, indeed the most violent he had ever felt, which lasted somewhat more than a minute, but did no damage in Aleppo. In about 10 minutes after this first, there was a 2d shock; but the tremulous motion was less violent, and did not last above 15 seconds. This earthquake occasioned little alarm among the natives, and even with the Europeans was the topic only for a day. But the subject was soon revived, by letters from Damascus, where the same shock felt as at Aleppo, and several other successive ones had done considerable damage. From this time we had daily accounts of earthquakes from Damascus, Tripoly, Seidon, Acri, and all along the coast of Syria.

Nov. 25 was a more severe shock. About half an hour after 7 at night the earthquake came on: the motion at first was gently tremulous, increasing by degrees till the vibrations became more distinct, and so strong as to shake the walls of the houses with considerable violence; they again became more gentle, and thus changed alternately several times during the shock, which lasted in all about 2 minutes. In about 8 minutes after this was over, a slight shock of a few seconds duration succeeded. At a quarter after 4 next morning was another shock, which lasted somewhat less than a minute, and was hardly so strong as that of the preceding night. The night of the 26th was rainy and cloudy. At 9 o'clock was a slight shock of a few seconds. The motion here appeared to be very deep, and was rather undulatory than tremulous. From midnight of the 25th, besides these now mentioned, 4 or 5 slighter shocks were felt; but Mr. R. was sensible of none till the morning of the 28th, when they had a short pulsatory shock. The same day, at 2 o'clock, they had a pretty smart shock, lasting about 40 seconds. From this time he was sensible of no more, though others either felt or imagined several slight vibrations every day.

At Antioch many houses have been thrown down, and some few people killed.

The earthquake of the evening of the 25th proved fatal to Damascus; one-third of the city was thrown down, and of the people great numbers perished in the ruins. The greater part of the surviving inhabitants fled to the fields, where they continued, being hourly alarmed by slighter shocks, which deterred them from re-entering the city, or attempting the relief of such as might yet be saved, by clearing away the rubbish. Other accounts make the loss of the inhabitants amount to 20,000.

Tripoli suffered rather more than Aleppo; 3 minorets, and 2 or 3 houses,

were thrown down, while the walls of numbers of the houses were rent. Many other towns suffered more or less.

LIII. Remarks on the Bovey Coal. By Jeremiah Mills, D.D., F.R.S.* p. 534.

The Devonshire fossil is commonly known by the name of the Bovey coal. It is found on a common surrounded with hills, called Bovey Heathfield, in the parish of South-bovey, 13 miles south-west of Exeter, and 3 miles west of Chudleigh. The uppermost of these strata rises within a foot of the surface, under a sharp white sand, intermixed with an ash-coloured clay, and underlies to the south about 20 inches in a fathom. The perpendicular thickness of these strata, including the beds of clay with which they are intermixed, is about 70 feet. There are about 6 of each, and they are found to continue eastward, in an uninterrupted course, to the village of Little Bovey, a mile distant, and probably extend much farther. The strata of coal near the surface are from 18 inches to 4 feet thick, and are separated by beds of a brownish clay, nearly of the same dimensions, but diminishing in thickness downwards in proportion as the strata of coal grew larger; and both are observed to be of a more compact and solid substance in the lower beds. The lowermost stratum of coal is 16 feet thick; it lies on a bed of clay, under which is a sharp green sand, not unlike sea sand, 17 feet thick, and under that a bed of hard close clay, into which they bored, but found no coal. From the sand arises a spring of clear blue water, which the miners call mundic water, and a moisture of the same kind trickling through the crevices of the coal tinges the outside of it with a blue cast.

Some small and narrow veins of coal are found intermixed with, and shooting through the beds of clay, forming impressions like reeds and grass, and very similar to those generally found on the top of coal-mines. The clay also (at least that part of it which lies nearest to the coal) seems to partake of its nature, having somewhat of a laminous texture, and being in a small degree inflammable; and among this clay, but adhering to the veins of coal, are found lumps of a bright yellow loam, extremely light, and so saturated with petroleum, that they burn like sealing wax, emitting a very agreeable and aromatic scent.

Though the substance and quality of this coal, in its several strata, are much

* Dr. Mills was born at High Cleer, in Hampshire, of which place his father was minister, in 1713, and he died in 1784; consequently at 71 years of age. Dr. M. was esteemed a very learned divine and antiquary. He succeeded Dr. Lyttleton as dean of Exeter, and also as president of the Society of Antiquaries, to whose Archæologia he was a great contributor. Dr. Mills was a zealous champion for the genuineness of the Rowley poems, of which he printed an edition in 4to, with glossarial annotations; which laid him open to the attacks of the critics, who were sceptical on those supposed relics of antiquity.

alike, and it is all indiscriminately used for the same purposes; yet there is some difference in the colour, form, and texture of the several veins. The exterior parts, which lie nearest to the clay, have a greater mixture of earth, and are generally of a dark brown, or chocolate colour; some of them appear like a mass of coal and earth mixed; others have a laminous texture, but the laminæ run in such oblique, waving, and undulating forms, that they bear a strong resemblance to the roots of trees, like some specimens from Lough Neagh in Ireland, which seem to be the same sort of fossil.

There are other veins of this coal, which lie more in the centre of the strata, and abound most in the lowest and thickest bed, the substance of which is more compact and solid: these are as black and almost as heavy as pit coal; they do not so easily divide into laminæ, and seem to be more strongly impregnated with bitumen: these are distinguished by the name of stone coals, and the fire of them is more strong and lasting than that of other veins. But the most remarkable and curious vein in these strata, is that which they call the wood coal, or board coal, from the resemblance which the pieces have to the grain of deal boards. It is sometimes of a chocolate colour, and sometimes of a shining black. The former sort seems to be less impregnated with bitumen, is not so solid and heavy as the latter, and has more the appearance of wood. It lies in straight and even veins, and is frequently dug in pieces of 3 or 4 feet long, and with proper care might be taken out of a much greater length. Other pieces of the same kind are found lying on them in all directions, but without the least intermixture of earth, or any other interstices, except some small crevices, by which the pieces are divided from each other in all directions. When it is first dug, and moist, the thin pieces will bend like horn, but when dry it loses its elasticity, and becomes short and crisp. At all times, it is easily to be separated into very thin laminæ or splinters, especially if it lie any time exposed to the heat of the sun, which like the fire makes it crackle, separate, and fall to pieces. The texture of this fossil consists of a number of laminæ, or very thin plates, lying over each other horizontally, in which small protuberances sometimes appear, like the knots of trees; but on examination they are only mineral nuclei, which occasion this interruption in the course of the laminæ; and pieces of spar have been sometimes found in the middle of this wood coal.

Though the texture of this coal is laminated, yet it does not appear to have any of those fibrous intersections observed in the grain of all wood. This coal easily breaks transversely, and the separated parts, instead of being rugged and uneven, are generally smooth and shining, in which even the course of the laminæ is hardly discernible. They dig its coal in an open pit, together with the clay that is mixed with it; and though it lies very close and compact in its original bed, yet it is so easily separated, that they can afford to sell it for half

a crown a ton at the pit. The smaller coal is separated from the clay by a screen, or grated shovel; the larger, which rises sometimes in pieces of above an hundred weight, is piled up by hand. There is hardly any other use made of it at present but to bake the earthen ware of a manufacture erected at South Bovey, and for burning lime-stone.

The fire made by this coal is more or less strong and lasting according to its different veins: those which lie nearest to the clay, having a greater mixture of earth, burn heavily, leaving a large quantity of brownish ashes; that which they call the wood coal is said to make as strong a fire as oaken billets, especially if it be set on edge, so that the fire, as it ascends, may insinuate itself between, and separate the laminæ. But that of the stone coal is accounted most strong and durable, being apparently more solid and heavy, and probably also more strongly impregnated with bitumen.

When this coal is put into the fire, it crackles and separates into laminæ, as the cannel coal does into irregular pieces, burns for some time with a heavy flame, becomes red hot, and gradually consumes to light white ashes. Though the transverse crevices made in it by the fire give it the external appearance of a wooden brand, yet if quenched when red hot, the unconsumed part does not look like charcoal, but seems to be almost as smooth and solid as when first put into the fire.

Notwithstanding the resemblance which this fossil bears to wood, especially when viewed in detached pieces, yet the following observations on its situation, its form and properties will prove it to be not of a vegetable but of a mineral origin. In the first place, there does not seem to be any imaginable cause in nature which could bring together such a mass of fossil wood as is found in this, and other strata of the like kind in different parts of Europe. It extends here to the depth of 70 feet: in that near Munden they have sunk 50 feet, without coming to the bottom. Fossil trees, though frequently found single, or in small numbers, are generally discovered in morasses and soft ground, where they have either buried themselves by their own weight, or been overwhelmed by some accidental cause: but the Bovey strata are found in a dry soil, intermixed with clay and sand, and by their regular course and continuance, carry the most undoubted marks of never having been disturbed since their original formation. Fossil trees likewise preserve their form and size, their length and roundness, their branches and roots, their fibrous texture and strength, and are either found entire, or in such large pieces, that there is no room to doubt of their nature, since the very species of wood is frequently distinguishable in them; whereas the Bovey coal comes out only in flat pieces, of a few feet long, like the splinters of large masts; and on them are discovered no signs of roots, branches, or bark, no round pieces, or concentric circles, which distinguish the annual growth of

trees; the laminæ, which have the appearance of wood, being always horizontal, according to the situation of the pieces in the strata: or could we suppose a number of fossil trees to be brought together, and ranged in this regular manner in the several strata, yet by the form and roundness of their trunks, they must be in a great measure encompassed by the soil, in which they are buried; whereas there is not the least mixture of earth, or any other aperture in the Bovey strata of coal, except a few crevices, common to this sort of fossil, which divide the pieces from each other in all directions, and seem to be inconsistent with the nature and fibrous texture of wood. If the basis or matrix of this fossil were wood, it would acquire, by being impregnated with bitumen, a greater degree of inflammability; whereas it neither kindles nor consumes so fast as wood.

Dr. M. mentions a number of other places where such fossil substances are found; and then concludes with observing the several particulars in which all the species of the bituminous fossils resemble each other. They seem to be generally found between beds of clay or stone; are of a dark brown, or black colour, of a laminated texture; pliable when moist, and fresh dug, but crisp and brittle when dry; full of cracks, and easily breaking transversely; they all sink in water, and emit the same nauseous and bituminous smell; they differ in being more or less solid, heavy, and inflammable, according to the proportions and principles of which they consist; and if any doubt could remain of their being a mineral substance, it must be removed by the following analysis. One pound of Bovey coal, of the woody kind, powdered, put into a glass retort, and distilled in sand, yielded $4\frac{1}{4}$oz. of phlegm, which had the appearance of common water, but somewhat of a bituminous smell and taste; near 4 oz. of a turbid whitish bituminous liquor, of an intolerable fetid smell, and extremely pungent to the tongue; about 2 drs. of a heavy bituminous matter, which would not mix with the liquor above mentioned, but sunk entirely to the bottom, and (which is very remarkable) there was not the least appearance of any light oil floating on the bituminous liquor. There remained in the retort about 7 oz. of a very black powder, which had the same bituminous smell, not very heavy; some of which being put on a red-hot iron, emitted a little smoke, but no flame. The ashes of this fossil, when burnt, being boiled in water, and the water evaporated, there remained no salt behind.

LIV. A New Method of Computing the Sums of Certain Series. By Mr. John Landen. Communicated by Mr. Thomas Simpson, F. R. S. p. 553.

1. Supposing x to be the sine of the circular arc z, whose radius is 1, $\frac{\dot{x}}{\sqrt{1-x^2}}$ will be $= \dot{z}$; and consequently, $\frac{\dot{x}}{\sqrt{x^2-1}} = \frac{\dot{z}}{\sqrt{-1}}$. Hence, by taking the correct fluents, we have hyp. log. $\frac{x+\sqrt{x^2-1}}{\sqrt{-1}} = \frac{z}{\sqrt{-1}}$.

Hence, writing a for one 4th of the periphery of the circle whose radius is 1, and taking x equal to the said radius, we find hyp. log. $\frac{1}{\sqrt{-1}} = \frac{a}{\sqrt{-1}}$; and consequently hyp. log. $\sqrt{-1} = \frac{-a}{\sqrt{-1}}$, and hyp. log. $-1 = \pm \frac{2a}{\sqrt{-1}}$.

2. The hyp. log. of $\frac{1}{1-x}$ being $= x + \frac{x^2}{2} + \frac{x^3}{3} + \frac{x^4}{4}$, &c.

$$\text{F}' = \text{fluent of } \frac{\dot{x}}{x} \text{ hyp. log. } \frac{1}{1-x}, \text{ is } = x + \frac{x^2}{2^2} + \frac{x^3}{3^2} + \frac{x^4}{4^2}, \text{ \&c.}$$

$$\text{F}'' = \text{fluent of } \frac{\dot{x}}{x} \text{ F}' = x + \frac{x^2}{2^3} + \frac{x^3}{3^3} + \frac{x^4}{4^3}, \text{ \&c.}$$

$$\text{F}''' = \text{fluent of } \frac{\dot{x}}{x} \text{ F}'' = x + \frac{x^2}{2^4} + \frac{x^3}{3^4} + \frac{x^4}{4^4}, \text{ \&c.}$$

$$\text{F}^{\text{iv}} = \text{fluent of } \frac{\dot{x}}{x} \text{ F}''' = x + \frac{x^2}{2^5} + \frac{x^3}{3^5} + \frac{x^4}{4^5}, \text{ \&c.}$$

&c. &c. &c.

3. By writing, in the first equation in the preceding article, $\frac{1}{x}$ instead of x, we have

$$\text{Hyp. log. } \frac{1}{1-\frac{1}{x}} = x^{-1} + \frac{x^{-2}}{2} + \frac{x^{-3}}{3}, \text{ \&c.}$$

But the hyp. log. of $\frac{1}{1-\frac{1}{x}}$ is $=$ hyp. log. $\frac{x}{x-1} =$ hyp. log. $\frac{1}{1-x} +$ hyp. log. x $+$ hyp. log. $-1 = \pm 2b + \text{X} +$ hyp. log. $\frac{1}{1-x}$, b being put for $\frac{a}{\sqrt{-1}}$, and X for the hyp. log. of x. It is evident, therefore, that

Hyp. log. $\frac{1}{1-x}$ is $= \mp 2b - \text{X} + x^{-1} + \frac{x^{-2}}{2} + \frac{x^{-3}}{3}$, &c. where, of the two signs prefixed to $2b$, the upper one takes place, when the hyp. log. of -1 is taken equal to $\frac{-2a}{\sqrt{-1}}$, likewise when x is taken equal to $\sqrt{-1}$; and the lower one takes place, when the hyp. log. of -1 is taken equal to $\frac{2a}{\sqrt{-1}}$, also when x is taken equal to $\frac{1}{\sqrt{-1}}$: therefore, if we observe to take the value of hyp. log. of -1, as last mentioned, and x equal to $\frac{1}{\sqrt{-1}}$, instead of $\sqrt{-1}$, we need retain only the lower of the said signs.

4. For brevity sake, we shall, in what follows, put the series

$$1 + \frac{1}{2^2} + \frac{1}{3^2} + \frac{1}{4^2}, \text{ \&c.} = \text{P}'',$$

$$1 + \frac{1}{2^4} + \frac{1}{3^4} + \frac{1}{4^4}, \text{ \&c.} = \text{P}^{\text{iv}},$$

$$1 + \frac{1}{2^6} + \frac{1}{3^6} + \frac{1}{4^6}, \text{ \&c.} = \text{P}_{\text{vi}},$$

&c. &c.

$$1 + \frac{1}{3^2} + \frac{1}{5^2} + \frac{1}{7^2}, \text{ \&c.} = Q'',$$
$$1 - \frac{1}{3^3} + \frac{1}{5^3} - \frac{1}{7^3} +, \text{ \&c.} = Q''',$$
$$1 + \frac{1}{3^4} + \frac{1}{5^4} + \frac{1}{7^4}, \text{ \&c.} = Q^{iv},$$
$$1 - \frac{1}{3^5} + \frac{1}{5^5} - \frac{1}{7^5} +, \text{ \&c.} = Q^{v},$$
&c. &c.

5. Multiplying the last equation in art. 3, by $\frac{\dot{x}}{x}$, and taking the correct fluents we have

$$F' = 2P'' + 2bX - \frac{X^2}{2} - x^{-1} - \frac{x^{-2}}{2^2} - \frac{x^{-3}}{3^2}, \text{ \&c.}$$

Whence, by multiplying by $\frac{\dot{x}}{x}$, and taking the fluents, we get

$$F'' = 2P''X + bX^2 - \frac{X^3}{2\,3} + x^{-1} + \frac{x^{-2}}{2^3} + \frac{x^{-3}}{3^3}, \text{ \&c.}$$

Again, multiplying the last equation by $\frac{\dot{x}}{x}$, and taking the correct fluents, we find

$$F''' = 2P^{iv} + P''X^2 + \frac{bX^3}{3} - \frac{X^4}{2.3.4} - x^{-1} - \frac{x^{-2}}{2^4} - \frac{x^{-3}}{3^4}, \text{ \&c.}$$

And by proceeding in the same manner, we find

$$F^{iv} = 2P^{iv}X + \frac{P^{v}X^3}{3} + \frac{bX^4}{3.4} - \frac{X^5}{2.3.4.5} + x^{-1} + \frac{x^{-2}}{2^5} + \frac{x^{-3}}{3^5}, \text{ \&c.}$$
&c. &c.

6. Now, it is obvious, that $x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$, &c. the value of F' in art. 2, must be equal to $2P'' + 2bX - \frac{X^2}{2} - x^{-1} - \frac{x^{-2}}{2^2} - \frac{x^{-3}}{3^2}$, &c. the value of F' in art. 5, when both series converge.

Therefore, $\frac{x + x^{-1}}{1^2} + \frac{x^2 + x^{-2}}{2^2} + \frac{x^3 + x^{-3}}{3^2}$, &c. is then $= 2P'' + 2bX - \frac{X^2}{2}$.

From which equation, by taking x equal to -1, we have $-\frac{1}{1^2} + \frac{1}{2^2} - \frac{1}{3^2} + \frac{1}{4^2} -$, &c. $= P'' + b^2 = P'' - a^2$; and, by taking x equal to $\frac{1}{\sqrt{-1}}$, we have $-\frac{1}{1^2} + \frac{1}{2^2} - \frac{1}{3^2} + \frac{1}{4^2} -$, &c. $= 4P'' + 3b^2 = 4P'' - 3a^2$.

Therefore $4P'' - 3a^2$ is $= P'' - a^2$: hence P'' is found $= \frac{2a^2}{3}$.

Moreover $\frac{1}{1^2} + \frac{1}{2^2} + \frac{1}{3^2} + \frac{1}{4^2}$, &c. being $= P''$, by supposition, and $-\frac{1}{1^2} + \frac{1}{2^2} - \frac{1}{3^2} + \frac{1}{4^2} -$, &c. $= P'' - a^2$, as found above; we, by subtraction, get $\frac{2}{1^2} + \frac{2}{3^2} + \frac{2}{5^2}$, &c. $(= 2Q'') = a^2$, and, consequently $Q'' = \frac{a^2}{2}$.

SCHOLIUM. The hyp. log. of $\frac{1}{1-x}$ being $= x + \frac{x^2}{2} + \frac{x^3}{3}$, &c. we, by writing $1 - x$ instead of x, have

Hyp. log. of $\frac{1}{x} = 1 - x + \frac{(1-x)^2}{2} + \frac{(1-x)^3}{3}$, &c. and consequently $\mathrm{x} = -1 - x - \frac{(1-x)^2}{2} - \frac{(1-x)^3}{3}$, &c.

Moreover the fluent of $\frac{\dot{x}}{x} \times$ hyp. log. of $\frac{1}{1-x}$ is $= x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$, &c. which vanishes when x vanishes; and the fluent of $\frac{\dot{x}}{1-x} \times \mathrm{x}$ is $= (1-x) + \frac{(1-x)^2}{2^2} + \frac{(1-x)^3}{3^2}$, &c. $- \mathrm{P}''$, being corrected so as to vanish when x vanishes.

But the fluent of $\frac{\dot{x}}{x} \times$ hyp. log. of $\frac{1}{1-x}$ + fluent of $\frac{\dot{x}}{1-x} \times \mathrm{x}$ is $= \mathrm{x} \times$ hyp. log. of $\frac{1}{1-x}$, which also vanishes when x vanishes.

Therefore $\mathrm{x} \times$ hyp. log. of $\frac{1}{1-x}$ is $= x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$, &c. $+ 1 - x + \frac{(1-x)^2}{2^2} + \frac{(1-x)^3}{3^2}$, &c. $- \mathrm{P}''$.

Whence, by taking x equal to $\frac{1}{2}$, we find $-$ square of hyp. log. of $2 = 2 \times (\frac{1}{1^2.2^1} + \frac{1}{2^2.2^2} + \frac{1}{3^2.2^3}$, &c.) $- \mathrm{P}''$: hence, P'' being before found $= \frac{2a^2}{3}$, it appears that when x is $= \frac{1}{2}$, the series $x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$, &c. is $= \frac{a^2}{3} - \frac{1}{2} \times$ (hyp. log. of $2)^2$.

7. Further, $x + \frac{x^2}{2^3} + \frac{x^3}{3^3}$, &c. the value of F' in art. 2, must be equal to $2\mathrm{P}''\mathrm{x} + b\mathrm{x}^2 - \frac{\mathrm{x}^3}{2.3} + x^{-1} + \frac{x^{-2}}{2^3} + \frac{x^{-3}}{3^3}$, &c. the value of F'' in art. 5, when both series converge.

Therefore $\frac{x - x^{-1}}{1^3} + \frac{x^2 - x^{-2}}{2^3} + \frac{x^3 - x^{-3}}{3^3}$, &c. is then $= 2\mathrm{P}''\mathrm{x} + b\mathrm{x}^2 - \frac{\mathrm{x}^3}{2.3}$.

Whence, by taking x equal to -1, we have $4b\mathrm{P}'' + 4b^3 - \frac{8b^3}{2.3} = 0$; and, consequently, $\mathrm{P}'' = \frac{2a^2}{3}$, as before found.

And, by taking x equal to $\frac{1}{\sqrt{-1}}$ we find

$$\frac{2}{\sqrt{-1}} \times \mathrm{Q}''' = 2b\mathrm{P}'' + b^3 - \frac{b^3}{2.3} = \frac{4a^3}{3\sqrt{-1}} - \frac{a^3}{\sqrt{-1}} + \frac{a^3}{2.3\sqrt{-1}} = \frac{a^3}{2\sqrt{-1}}.$$

Therefore Q''' is $= \frac{a^3}{4}$.

8. From what is done above, it evidently follows, that

$$-\mathrm{P}^{\mathrm{iv}} \text{ is} = \frac{2b^2\mathrm{P}''}{3} + \frac{2.8b^4}{3.4.5},$$

$$-\mathrm{P}^{\mathrm{vi}} = \frac{2b^2\mathrm{P}^{\mathrm{iv}}}{3} + \frac{8b^4\mathrm{P}''}{3.4.5} + \frac{3.32b^6}{3.4.5.6.7},$$

&c. &c.

$$-\mathrm{Q}^{\mathrm{iv}} = b^2\mathrm{P}'' + \frac{3.2b^4}{3.4},$$

$$-\mathrm{Q}^{\mathrm{vi}} = b^2\mathrm{P}^{\mathrm{iv}} + \frac{4b^4\mathrm{P}''}{3.4} + \frac{5.8b^6}{3.4.5.6},$$

&c. &c.

$$\frac{Q^{v}}{\sqrt{-1}} \text{ is } = bP^{iv} + \frac{b^3P''}{2.3} + \frac{9b^5}{2.2.3.4.5},$$

$$\frac{Q^{vii}}{\sqrt{-1}} = bP^{vi} + \frac{b^3P^{iv}}{2.3} + \frac{b^5P''}{2.3.4.5} + \frac{13b^7}{2.2.3.4.5.6.7},$$

&c. &c.

Whence the values of P^{iv}, P^{vi}, &c. Q^{iv}, Q^{vi}, &c. Q^{v}, Q^{vii}, &c. may be easily obtained, in terms of a.

9. Hyp. log. $(\frac{1+x}{1-x})^{\frac{1}{2}}$ being $= x + \frac{x^3}{3} + \frac{x^5}{5}$, &c.

$G' =$ fluent of $\frac{\dot{x}}{x}$ hyp. log. $(\frac{1 \times x}{1-x})^{\frac{1}{2}}$ is $= x + \frac{x^3}{3^2} + \frac{x^5}{5^2}$, &c.

$G'' =$ fluent of $\frac{\dot{x}}{x}$ $G' = x + \frac{x^3}{3^3} + \frac{x^5}{5^3}$, &c.

$G''' =$ fluent of $\frac{\dot{x}}{x}$ $G'' = x + \frac{x^3}{3^4} + \frac{x^5}{5^4}$, &c.

&c. &c. &c.

10. By writing, in the first equation in the preceding article, $\frac{1}{x}$ instead of x, we have

$$\text{Hyp. log. } \left(\frac{1+\frac{1}{x}}{1-\frac{1}{x}}\right)^{\frac{1}{2}} = x^{-1} + \frac{x^{-3}}{3} + \frac{x^{-5}}{5}, \text{ \&c.}$$

But the hyp. log. of $\left(\frac{1+\frac{1}{x}}{1-\frac{1}{x}}\right)^{\frac{1}{2}}$ is $=$ hyp. log. $(\frac{x+1}{x-1})^{\frac{1}{2}} =$ hyp. log. $(\frac{1+x}{1-x})^{\frac{1}{2}} \mp$ hyp. log. $\sqrt{-1} = \pm\, b +$ hyp. log. $(\frac{1+x}{1-x})^{\frac{1}{2}}$.

It is manifest, therefore, that

$$\text{Hyp. log. } (\frac{1+x}{1-x})^{\frac{1}{2}} \text{ is } = \mp\, b + x^{-1} + \frac{x^{-3}}{3} + \frac{x^{-5}}{5}, \text{ \&c.}$$

where, with respect to the two signs prefixed to b, the same observation may be made as in art. 3.

11. Multiplying the last equation by $\frac{\dot{x}}{x}$ and taking the correct fluents, we have

$$G' = 2Q'' + bx - x^{-1} - \frac{x^{-3}}{3^2} - \frac{x^{-5}}{5^2}, \text{ \&c.}$$

Whence, by multiplying by $\frac{\dot{x}}{x}$, and taking the fluents, we get

$$G'' = 2Q''x + \frac{bx^2}{2} + x^{-1} + \frac{x^{-3}}{3^3} + \frac{x^{-5}}{5^3}, \text{ \&c.}$$

Again, multiplying the last equation by $\frac{\dot{x}}{x}$, and taking the correct fluents, we find $G''' = 2Q^{iv} + Q^{v}x^2 + \frac{bx^3}{2.3} - x^{-1} - \frac{x^{-3}}{3^4} - \frac{x^{-5}}{5^4}$, &c.

And, by proceeding in the same manner, we find

$$G^{iv} = 2Q^{iv}x + \frac{Q''x^3}{3} + \frac{bx^4}{2.3.4} + x^{-1} + \frac{x^{-3}}{3^5} + \frac{x^{-5}}{5^5}, \text{ \&c.}$$

&c. &c.

12. Now, it is obvious, that $x + \frac{x^3}{3^2} + \frac{x^5}{5^2}$, &c. the value of G′ in art. 9, must be equal to $2Q'' + bX - x^{-1} - \frac{x^{-3}}{3^2} - \frac{x^{-5}}{5^2}$, &c. the value of G′ in art. 11, when both series converge.

Therefore $\frac{x + x^{-1}}{1^2} + \frac{x^3 + x^{-3}}{3^2} + \frac{x^5 + x^{-5}}{5^2}$, &c. is then $= 2Q'' + bX$.

Whence, by taking x equal to $\frac{1}{\sqrt{-1}}$, we have $2Q'' + b^2 = 0$; and, consequently, $Q'' = \frac{a^2}{2}$ as in art. 6.

13. Likewise $x + \frac{x^3}{3^3} + \frac{x^5}{5^3}$, &c. the value of G″ in art. 9, must be equal to $2Q''X + \frac{bX^2}{2} + x^{-1} + \frac{x^{-2}}{3^3} + \frac{x^{-5}}{5^3}$, &c. the value of G″ in art. 11, when both series converge.

Therefore $\frac{x - x^{-1}}{1^3} + \frac{x^3 - x^{-3}}{3^3} + \frac{x^5 - x^{-5}}{5^3}$, &c. is then $= 2Q''X + \frac{bX^2}{2}$.

Hence, by taking $x = \frac{1}{\sqrt{-1}}$, we find $\frac{2}{\sqrt{-1}} \times Q''' = 2bQ'' + \frac{b^3}{2} = \frac{a^3}{2\sqrt{-1}}$; and, consequently, $Q''' = \frac{a^3}{4}$, as in art. 7.

14. From what is done in the last 5 articles, it evidently follows, that

$$-Q^{iv} \text{ is } = \frac{b^2Q''}{2} + \frac{b^4}{2.2.3},$$

$$\frac{Q^{v}}{\sqrt{-1}} = bQ^{iv} + \frac{b^3Q''}{2.3} + \frac{b^5}{2.2.3.4},$$

$$-Q^{vi} = \frac{b^2Q^{iv}}{2} + \frac{b^4Q''}{2.3.4} + \frac{b^6}{2.2.3.4.5},$$

$$\frac{Q^{vii}}{\sqrt{-1}} = bQ^{vi} + \frac{b^3Q^{iv}}{2.3} + \frac{b^5Q''}{2.3.4.5} + \frac{b^7}{2.2.3.4.5.6},$$

&c. &c.

Whence (as well as from the theorems in art. 8) may the values of Q^{iv}, Q^{v}, Q^{vi}, Q^{vii}, &c. be readily found, in terms of a.

15. G′ being $= x + \frac{x^3}{3^2} + \frac{x^5}{5^2}$, &c. by art. 9.

$$H' = \text{fluent of } x\dot{x}G' \text{ is} = \frac{x^3}{1^2.3} + \frac{x^5}{3^2.5} + \frac{x^7}{5^2.7}, \text{ \&c.}$$

$$H'' = \text{fluent of } \frac{\dot{x}}{x}H' = \frac{x^3}{1^2.3^2} + \frac{x^5}{3^2.5^2} + \frac{x^7}{5^2.7^2}, \text{ \&c.}$$

$$H''' = \text{fluent of } x\dot{x}H'' = \frac{x^5}{1^2.3^2.5} + \frac{x^7}{3^2.5^2.7} + \frac{x^9}{5^2.7^2.9}, \text{ \&c.}$$

$$H^{iv} = \text{fluent of } \frac{\dot{x}}{x}H''' = \frac{x^5}{1^2.3^2.5^2} + \frac{x^7}{3^2.5^2.7^2} + \frac{x^9}{5^2.7^2.9^2}, \text{ \&c.}$$

&c. &c. &c.

16. Moreover, G′ being $= 2Q'' + bX - x^{-1} - \frac{x^{-3}}{3^2} - \frac{x^{-5}}{5^2}$, &c. by art. 11, by multiplying by $x\dot{x}$, and taking the correct fluents, we get $H' = x^2Q'' - Q'' + \frac{bx^2X}{2} - \frac{bx^2}{4} + \frac{b}{4} - x + 1 + 2S'' + \frac{x^{-1}}{1.3^2} + \frac{x^{-3}}{3.5^2} + \frac{x^{-5}}{5.7^2}$, &c. S″ being put for the series $\frac{1}{1^2.3^2} + \frac{1}{3^2.5^2} + \frac{1}{5^2.7^2}$, &c.

Now, it is obvious, that this value of H′ must be equal to the value of H′ in the preceding article, when both series converge.

Therefore $\frac{3x^3 - x^{-1}}{1^2.3^2} + \frac{5x^5 - 3x^{-3}}{3^2.5^2} + \frac{7x^7 - 5x^{-5}}{5^2.7^2}$, &c. is then $= x^2Q'' - Q'' + \frac{bx^2X}{2} - \frac{bx^2}{4} + \frac{b}{4} - x + 1 + 2S''$.

Hence, by taking x equal to -1, we find $-2S'' = b^2 + 2 + 2S''$; and, consequently, $S'' = \frac{a}{4} - \frac{1}{3}$.

And, by taking x equal to $\frac{1}{\sqrt{-1}}$, we find

$+\frac{2}{\sqrt{-}} \times (\frac{1}{1^2.3^2} - \frac{1}{3^2.5^2} + \frac{1}{5^2.7^2} -$, &c.$) = -2Q'' - \frac{b^2}{2} + \frac{b}{2} - \frac{1}{\sqrt{-1}} + 1 + 2S'' = \frac{a}{2\sqrt{-1}} - \frac{1}{\sqrt{-1}}$; and, consequently, $\frac{1}{1^2.3^2} - \frac{1}{3^2.5^2} + \frac{1}{5^2.7^2}$, &c. $= \frac{1}{3} - \frac{a}{4}$.

17. Seeing that Q'' is $= \frac{a^2}{2}$, and $S'' = \frac{a^2}{4} - \frac{1}{3}$, it follows, from the last article, that H′ is $= x^2Q'' + \frac{bx^2X}{2} - \frac{bX^2}{4} + \frac{b}{4} - x + \frac{x^{-1}}{1.3^2} + \frac{x^{-3}}{3.5^2} + \frac{x^{-5}}{5.7^2}$, &c.

Whence, by multiplying by $\frac{\dot{x}}{x}$, and taking the correct fluent, we get

$H'' = \frac{x^2Q''}{2} + \frac{bx^2X}{4} - \frac{bx^2}{4} + \frac{b}{4} + \frac{bX}{4} - x + \frac{a^2}{4} - \frac{x^{-1}}{1^2.3^2} - \frac{x^{-3}}{3^2.5^2} - \frac{x^{-5}}{5^2.7^2}$, &c.

And hence, by multiplying by $x\dot{x}$, and taking the correct fluents, we have $H''' = \frac{x^4Q''}{8} + \frac{bx^4X}{16} - \frac{5bx^4}{64} + \frac{bx^2}{16} + \frac{bx^2X}{8} + \frac{b}{64} - \frac{x^3}{3} - \frac{x}{9} + \frac{4}{9} + \frac{a^2x^2}{8} - \frac{3a^2}{16} + 4S''' + \frac{x^{-1}}{1.3^2.5^2} + \frac{x^{-3}}{3.5^2.7^2} + \frac{x^{-5}}{5.7^2.9^2}$, &c. S''' being put for the series $\frac{1}{1^2.3^2.5^2} + \frac{1}{3^2.5^2.7^2} + \frac{1}{5^2.7^2.9^2}$, &c.

Now, this value of H‴ being equal to the value of H‴ in art. 15, when both series converge, it follows, that $\frac{5x^5 - x^{-1}}{1^2.3^2.5^2} + \frac{7x^7 - 3x^{-3}}{3^2.5^2.7^2} + \frac{9x^9 - 5x^{-5}}{5^2.7^2.9^2}$, &c. is then $= \frac{x^4Q''}{8} + \frac{bx^4X}{16} - \frac{5bx^4}{64} + \frac{bx^2}{16} + \frac{bx^2X}{8} + \frac{b}{64} - \frac{x^3}{3} - \frac{x}{9} + \frac{4}{9} + \frac{a^2x^2}{8} + \frac{3a^2}{16} = 4S'''$.

Hence, by taking x equal to -1, we find $-4S''' = \frac{3b^2}{8} + \frac{8}{9} + 4S'''$; and consequently $S''' = \frac{3a^2}{64} - \frac{1}{9}$.

Many other instances of the use of this method might be given; but these may suffice to enable the intelligent reader to pursue the speculation farther, at his pleasure.

The above subject is much further pursued by the ingenious author, in his Mathematical Memoirs, p. 67, published 1780.

LV. Conjectures concerning the Cause, and Observations on the Phenomena of Earthquakes; particularly of that Great Earthquake of Nov. 1, 1755, which proved so fatal to the City of Lisbon, and whose Effects were felt as fa

as Africa, and more or less throughout almost all Europe. By the Rev. John Michell, M. A. p. 566.

INTRODUCTION.—It has been the general opinion of philosophers, that earthquakes owe their origin to some sudden explosion in the interior of the earth. This opinion is agreeable to the phenomena which seem to point out something of that kind. The conjectures, however, of the cause of such an explosion have not been yet sufficiently supported by facts; nor have the more particular effects which will arise from it been traced out; and the connection of them with the phenomena explained. To do this is the intent of the following pages; and the dreadful earthquake of the 1st of Nov. 1755 supplies us with more * facts for this purpose than any other earthquake of which we have an account.

That these concussions should owe their origin to something in the air seems very ill to correspond with the phenomena. This will sufficiently appear, as those phenomena are hereafter recounted; nor does there appear to be any such certain and regular connection between earthquakes and the state of the air, when they happen, as is supposed by those who hold this opinion. It is said, for instance, that earthquakes always happen in calm still weather: but that this is not always so may be seen in an account of the earthquakes in Sicily of 1693, (Phil. Trans. N° 207,) where we are told, "the south winds have blown very much, which still have been impetuous in the most sensible earthquakes, and the like has happened at other times." Other examples to the same purpose we have in an account of the earthquakes that happened in New England in 1727 and 1728; the author of which says, that he could neither observe any connection between the weather and the earthquakes, nor any prognostic of them; for that they happened alike in all kinds of weather, at all times of the tides, and at all times of the moon. Phil. Trans. N° 409. If, however, it should still be supposed, notwithstanding these instances to the contrary, that there is some general connection between earthquakes and the weather, at the time when they happen, yet surely it is far more probable that the air should be affected by the causes of earthquakes, than that the earth should be affected in so extraordinary a manner, and to so great a depth; and that this, and all the other circumstancs attending these motions, should be owing to some cause residing in the air.

Let us then, rejecting this hypothesis, suppose that earthquakes have their origin under ground, and we need not go far in search of a cause, whose real existence in nature we have evidence of, and which is capable of producing all the appearances of these extraordinary motions. The cause I mean, says Mr. M.

* See the 10th vol. of these Abridgments.

is subterraneous fires. These fires, if a large quantity of water should be let out upon them suddenly, may produce a vapour, whose quantity and elastic force may be fully sufficient for that purpose. The principal facts, from which I would prove, that these fires are the real cause of earthquakes, are as follows.

SECTION I.—1st. The same places are subject to returns of earthquakes, not only at small intervals for some time after any considerable one has happened, but also at greater intervals of some ages.

Both these facts sufficiently appear from the accounts we have of earthquakes. The tremblings and shocks of the earth at Jamaica in 1692, at Sicily in 1693, and at Lisbon in 1755, were repeated sometimes at larger and sometimes at smaller intervals, for several months. The same thing has been observed in all other very violent earthquakes. At Lima, from the 28th of October 1746, to Feb. 24, 1747, there had been numbered no less than 451 shocks, many of them little inferior to the first great one which destroyed that city.

The returns of earthquakes also, in the same places, at larger distances of time, are confirmed by all history. Constantinople, and many parts of Asia Minor, have suffered by them, in many different ages: Sicily has been subjected to them, as far back as the remains even of fabulous history can inform us of: Lisbon did not feel the effects of them for the first time in 1755: Jamaica has frequently been troubled with them, since the English first settled there; and the Spaniards, who were there before, used to build their houses of wood, and only one story high, for fear of them: Lima, Callao, and the parts adjacent, were almost totally destroyed by them twice, within the compass of about 60 years; nor were these the only instances of the like kind which happened there; for, from the year 1582 to 1746, they have had no less than 16 very violent earthquakes, besides an infinity of less considerable ones; and the Spaniards, at their first settling there, were told by the old inhabitants, when they saw them building high houses, that they were building their own sepulchres.

2dly, Those places that are in the neighbourhood of burning mountains are always subject to frequent earthquakes; and the eruptions of those mountains, when violent, are generally attended with them.

Asia Minor and Constantinople may be considered as in the neighbourhood of Santerini. The countries also about Etna, Vesuvius, mount Hæcla, &c. afford us sufficient proofs to the same purpose. But of all the places in the known world, probably no countries are so subject to earthquakes as Peru, Chili, and all the western parts of South America; nor is there any country in the known world so full of volcanos: for, throughout all that long range of mountains, known by the name of the Andes, from 45 degrees south latitude to several degrees north of the line, as also throughout all Mexico, being about 5000 miles in extent, there is a continued chain of them.

3dly, The motion of the earth in earthquakes is partly tremulous, and partly propagated by waves, which succeed one another sometimes at larger and sometimes at smaller distances; and this latter motion is generally propagated much farther than the former.

The former part of this proposition wants no confirmation: for the proof of the latter, viz. the wave-like motion of the earth, we may appeal to many accounts of earthquakes: it was very remarkable in the two which happened at Jamaica in 1687-8 and 1692. In an account of the former, it is said that a gentleman there saw the ground rise like the sea in a wave, as the earthquake passed along, and that he could distinguish the effects of it to some miles distance, by the motion of the tops of the trees on the hills. Again, in an account of the latter, it is said "the ground heaved and swelled like a rolling swelling sea," insomuch that people could hardly stand on their legs by reason of it. The same has been observed in the earthquakes of New England, where it has been very remarkable. A gentleman giving an account of one that happened there Nov. 18, 1755, says, the earth rose in a wave, which made the tops of the trees vibrate 10 feet, and that he was forced to support himself, to avoid falling, while it was passing. The same also was observed at Lisbon, in the earthquake of the 1st Nov. 1755, as may be plainly collected from many of the accounts that have been published concerning it, some of which affirm it expressly: and this wave-like motion was propagated to far greater distances than the other tremulous one, being perceived by the motion of waters, and the hanging branches in churches, through all Germany, among the Alps, in Denmark, Sweden, Norway, and all over the British isles.

4thly, It is observed in places which are subject to frequent earthquakes, that they generally come to one and the same place from the same point of the compass. It may be added also, that the velocity with which they proceed, (as far as one can collect it from the accounts of them) is the same; but the velocity of the earthquakes of different countries is very different.

Thus all the shocks that succeeded the first great one at Lisbon in 1755, as well as the first itself, came from the north-west. This is asserted by the person who says he was about writing a history of the earthquakes there: all the other accounts also confirm the same thing; for what some say, that they came from the north, and others, that they came from the west, cannot be considered as any reasonable objection to this, but rather the contrary. The velocity also with which they were all propagated was the same, being at least equal to that of sound; for they all followed immediately after the noise that preceded them, or rather the noise and the earthquake came together: and this velocity agrees well with the intervals between the time when the first shock was felt at Lisbon, and the time when it was felt at other distant places, from the comparison of

which it seems to have travelled at the rate of more than 20 miles per minute.

An historical account of the earthquakes which have happened in New England says, that of 5 considerable ones 3 are known to have come from the same point of the compass, viz. the north-west: it is uncertain from what point the other 2 came, but it is supposed that they came from the same with the former. The velocity of these has been much less than that of the Lisbon earthquakes: this appears from the interval between the preceding noise and the shock, as well as from the wave-like motion before-mentioned. All the greater earthquakes that have been felt at Jamaica seem, by the accounts given of them, to have come from the sea, and, passing by Port-Royal, to have gone northwards. The velocity of these also was far short of the velocity of the Lisbon earthquakes. The earthquake of London, on the 8th of March 1750, was supposed to move from east to west. The same thing happened in a slight shock felt there in the last century, as the person who told Mr. M. this, had an opportunity of observing; for being by accident in a scalemaker's shop at the time when it happened, he found that all the scales vibrated from east to west. All the shocks that have been lately felt at Brigue, in Valais, have likewise come from the same point of the compass, viz. the south.

5thly, The Lisbon great earthquake has been succeeded by several local ones since, the extent of which has been much less.

Such were the earthquakes in Switzerland; those on the borders of France and Germany; those in Barbary, &c.

Sect. ii.—However well these facts may agree with the supposition before laid down, that subterraneous fires are the cause of earthquakes, one doubt may perhaps remain: viz. how it is possible that fires should subsist which have no communication with the outward air? In answer to this might be alleged the example of green plants, which take fire by fermentation, when laid together in heaps; where the admission of the outward air is so far from being necessary, that it will effectually prevent their doing so. But to pass by this, we have many instances more immediately to the purpose. It can hardly be supposed that the fires of the generality of volcanos receive any supply of fresh air (for this must be effectually prevented by that vapour which is continually rushing out at all their vents,) and yet they subsist, and frequently even increase, for many ages. Now these are fires of the very same kind with those he supposes to be the cause of earthquakes. Other facts, still more expressly to the purpose, are as follow:

In the earthquake of Nov. 1, 1755, we are told that both smoke and light flames were seen on the coast of Portugal, near Colares; and that on occasion of some of the succeeding shocks, a slight smell of sulphur was perceived

to accompany a " fog, which came from the sea, from the same quarter whence the smoke appeared." In an account of an earthquake in New England, it is said that at Newbury, 40 miles from Boston, the earth opened, and threw up several cart-loads of sand and ashes; and that the sand was also slightly impregnated with sulphur, emitting a blue flame when laid on burning coals. One of the relaters of the earthquake in Jamaica in 1692, has these words: " In Port-Royal, and in many places all over the island, much suphureous combustible matter has been found (supposed to have been thrown out on the opening of the earth,) which on the first touch of fire would flame and burn like a candle. " St. Christopher's was heretofore much troubled with earthquakes, which, on the eruption there of a great mountain of combustible matter, which still continues, wholly ceased, and have never been felt there since."

Again, we are told that on the 20th Nov. 1720, a burning island was raised out of the sea, near Tercera, one of the Azores, at which place several houses were shaken down by an earthquake, which attended the eruption of it. This island was about 3 leagues in diameter, and nearly round; whence it is manifest that the quantity of pumice stones and melted matter, which must have been requisite to form it, was amazingly great: in all probability it must have far exceeded all that has been thrown out of Etna and Vesuvius together within the last 2000 years. This may satisfy us that the fire which occasioned all this must have subsisted for many years, and this without any communication with the external air. It is worth observing that several instances of this kind have happened among the Azores. There are besides many marks of subterraneous fires about these islands, several places sending up smoke or flames. These islands are also subject to violent and frequent earthquakes. We have more instances to the same purpose, near the island of Santerini in the Archipelago, where there have been several little islands raised out of the sea by a submarine volcano. Of the eruption of one of these in the year 1708, with all the circumstances that attended it, there is a very good account in the Phil. Trans. N° 314, 317, 332. It was raised in a place where the sea had been formerly 100 fathoms deep, and was attended with earthquakes before it showed itself above water, as well as after. It is reported, that the island of Santerini itself was originally raised out of the sea in the same manner; but, be that as it will, we have certain accounts of new islands raised there, or additions made to the old ones, from time to time, for above 1900 years backwards, and there have always been earthquakes at the time of these eruptions.

Another example of the same kind happened at Manila, one of the Philippine islands, in the year 1750. This also was attended with violent earthquakes, to which that island, as well as the rest of the Philippines, is very much subject. Add to these the many instances of vast quantities of pumice stones which have

been sometimes found floating on the sea, at so great a distance from the shore, as well as from any known volcano, that there can be little doubt of their being thrown up by fires subsisting under the bottom of the ocean.

From these instances, we may with great probability conclude, that the fires of volcanos produce earthquakes. He does not however suppose that the earthquakes, which are frequently felt in the neighbourhood of volcanos, are owing to the fires of those volcanos themselves; for volcanos, giving passage to the vapours that are there formed, should rather prevent them, as in the instance at St. Christopher's, before mentioned. We also meet with frequent instances confirming the same thing among the Andes. Antonio d'Ulloa (speaking of what happens among these mountains) says, 'Experience shows us, that on the fresh breaking out of any volcano it occasions so violent a shock to the earth, that all the villages which are near it are overthrown and destroyed, as it happened in the case of the mountain Carguayraso. This shock, which we may without the least impropriety call an earthquake, is seldom found to accompany the eruptions, after an opening is once made; or if some small trembling is perceived, it is very inconsiderable; so that after the volcano has once found a vent the shocks cease, notwithstanding the matter of it continues to be on fire.' The greater earthquakes therefore seem rather to be occasioned by other fires, that lie deeper in the same tract of country; and the eruptions of volcanos, which happen at the same time with earthquakes, may with more probability be ascribed to those earthquakes than the earthquakes to the eruptions, whenever at least the earthquakes are of any considerable extent.

Sect. 3. It may be asked perhaps why we should suppose that several subterraneous fires exist in the neighbourhood of volcanos? In evidence of this, we have frequent instances of new volcanos breaking out in the neighbourhood of old ones: Carguayraso, just mentioned, may supply us with one example to this purpose; and in the night of the 28th of October 1746, in which Lima and Callao were destroyed, no less than four new ones burst forth in the adjacent mountains. To the same purpose we may allege the instances of many volcanos lying together in the same tract of country: as for example, the many places, not so few as 40, among the Azores, which either do now, or have formerly sent forth smoke and flames; the many volcanos also among the Andes, already mentioned: thus Etna, Strombolo, and Vesuvius, and Solfatara too, are all in the same neighbourhood: and Mons. Condamine says he has traced lavas, exactly like those of Vesuvius, all the way from Florence to Naples. In Iceland also we have, besides Hecla, not only several other volcanos, but also a great number of places that send up sulphureous vapours. But the examples of this kind are so frequent that there are few instances to be produced of single volcanos, without evident marks either that there have been others formerly in

their neighbourhood, or that there are at present subterraneous fires near them.

This frequency of subterraneous fires, in the neighbourhood of volcanos, will appear still more probable, if we consider the internal structure of the earth; and as it will be necessary also, in order to understand what follows, to know a little more of this matter than what falls under common observation, he gives the reader some account of it.

The earth (as far as one can judge from the appearances) is not composed of heaps of matter casually thrown together, but of regular and uniform strata. These strata, though they frequently do not exceed a few feet, or perhaps a few inches in thickness, yet often extend in length and breadth for many miles, and this without varying their thickness considerably. The same stratum also preserves a uniform character throughout, though the strata immediately next to each other are very often totally different. Thus, for instance, we shall have perhaps a stratum of potters' clay; above that a stratum of coal; then another stratum of some other kind of clay; next a sharp grit sand stone; then clay again; next perhaps sand stone again; and coal again above that; and it frequently happens that none of these exceed a few yards in thickness. There are however many instances in which the same kind of matter is extended to the depth of some hundreds of yards; but in all these, a very few only excepted, the whole of each is not one continued mass, but is again subdivided into a great number of thin laminæ, that seldom are more than 1, 2, or 3 feet thick, and frequently not so much. Beside the horizontal division of the earth into strata, these strata are again divided and shattered by many perpendicular fissures, which are in some places few and narrow, but oftentimes many, and of considerable width. There are also many instances where a particular stratum shall have almost no fissures at all, though the strata both above and below it are considerably broken: this happens frequently in clay, probably on account of the softness of it, which may have made it yield to the pressure of the superincumbent matter, and fill up those fissures which it originally had; for we sometimes meet with instances in mines, where the correspondent fissures in an upper and lower stratum are interrupted in an intermediate stratum composed of clay, or some such soft matter. Though these fissures do sometimes correspond to one another in the upper and lower strata, yet this is not generally the case, at least not to any great distance: those clefts however in which the larger veins of the ores of metals are found are an exception to this observation; for they sometimes pass through many strata, and those of different kinds, to unknown depths.

From this constitution of the earth, viz. the want of correspondence in the fissures of the upper and lower strata, as well as on account of those strata which are little or not at all shattered, it will come to pass that the earth cannot easily

be separated in a direction perpendicular to the horizon, if we take any considerable portion of it together; but in the horizontal direction, as there is little or no adhesion between one stratum and another, it may be separated without difficulty. Those fissures which are at some depth below the surface of the earth are generally found full of water; but all those that are below the level of the sea must always be so, either from the oozing of the sea, or rather of the land waters between the strata.

The strata of the earth are frequently very much bent, being raised in some places, and depressed in others, and this sometimes with a very quick ascent or descent; but as these ascents and descents in a great measure compensate one another, if we take a large extent of country together, we may consider the whole set of strata as lying nearly horizontally. What is very remarkable however in their situation is, that from most, if not all large tracts of high and mountainous countries, the strata lie in a situation more inclined to the horizon than the country itself, the mountainous countries being generally, if not always, formed out of the lower strata of earth. This situation of the strata may be not unaptly represented in the following manner. Let a number of leaves of paper, of several different sorts or colours, be pasted upon one another; then bending them up together into a ridge in the middle, conceive them to be reduced again to a level surface, by a plane so passing through them as to cut off all the part that had been raised; let the middle now be again raised a little, and this will be a good general representation of most, if not all large tracts of mountainous countries, together with the parts adjacent, throughout the whole world.

From this formation of the earth, it will follow that we ought to meet with the same kinds of earths, stones, and minerals, appearing at the surface, in long narrow slips, and lying parallel to the greatest rise of any long ridges of mountains; and so in fact we find them. The Andes in South America, as it has been said before, have a chain of volcanos that extend in length above 5000 miles: these volcanos are probably all derived from the same stratum. Parallel to the Andes is the Sierra, another long ridge of mountains, that run between the Andes and the sea; and ‘ these two ridges of mountains run within sight of each other, and almost equally, for above 1000 leagues together,’ being each, at a medium, about 20 leagues wide. The gold and silver mines wrought by the Spaniards are found in a tract of country parallel to the direction of these, and extending through a great part of their length.

The same thing is found to obtain in North America also. The great lakes, which give rise to the river St. Laurence, are kept up by a long ridge of mountains that run nearly parallel to the eastern coast. In descending from these towards the sea, the same sets of strata, and in the same order, are generally met with throughout the greatest part of their length. In Great Britain too we have

an instance to the same purpose, where the direction of the ridge varies about a point from due north and south, lying nearly from N. by E. to S. by W. There are many more instances of this to be met with in the world, if we may judge from circumstances, which make it highly probable that it obtains in a great number of places, and in several they seem to put it almost out of doubt. The reader is not to suppose however that in any instances the highest rise of the ridge, and the inclination of the strata from thence to the countries on each side, is perfectly uniform; for they have frequently very considerable inequalities, and these inequalities are sometimes so great that the strata are bent for some small distance, even the contrary way from their general inclination.

At considerable distances from large ridges of mountains, the strata for the most part assume a situation nearly level; and as the mountainous countries are generally formed out of the lower strata, so the more level countries are generally formed out of the upper strata of the earth. Hence in countries of this kind the same strata are found to extend themselves a great way, as well in breadth as in length: we have an instance of this in the chalky and flinty countries of England and France, which (excepting the interruption of the channel, and the clays, sands, &c. of a few countries) compose a tract of about 300 miles each way.

Besides the raising of the strata in a ridge, there is another very remarkable appearance in the structure of the earth, though a very common one; and this is what is usually called by miners the trapping down of the strata; that is, the whole set of strata on one side a cleft are sunk down below the level of the corresponding strata on the other side. If in some cases this difference in the level of the strata, on the different sides of the cleft, should be very considerable, it may have a great effect in producing some of the singularities of particular earthquakes.

PART II. In the former part of this essay, having recounted some of the principal appearances of earthquakes, as well as those particulars in the structure of the earth, on which he supposed these appearances to depend, Mr. M. thinks it is sufficiently manifest that in some instances, at least, earthquakes are actually produced by subterraneous fires; it therefore remains to be shown how all the appearances above recited, as well as many other minuter circumstances attending earthquakes, may be accounted for from the same cause.

Sect. 1. The returns of earthquakes in the same places, either at small or large intervals of time, are consistent with the cause assigned: subterraneous fires, from their analogy to volcanos, might reasonably be supposed to subsist for many ages, though we had not those instances already mentioned which put the matter out of doubt. And as it frequently happens that volcanos rage for a time, and then are quiet again for a number of years; so we see earthquakes also fre-

quently repeated for some small time, and then ceasing again for a long term excepting perhaps now and then some slight shock. And this analogy between earthquakes, and the effects of volcanos is so great, that he thinks it cannot but appear striking to any one who will read the accounts of both, and compare them together. The raging of volcanos is not one continued and uniform effect; but an effect repeated at unequal intervals, and with unequal degrees of force: thus for instance, we have perhaps 2 or 3 blasts discharged from a volcano, succeeding each other at the interval of a few seconds only; sometimes the intervals are of a quarter of an hour, an hour, a day, or perhaps several days. And as these intervals are very unequal, so is the violence of the blasts also: sometimes stones, &c. are thrown by these blasts to the distance of some miles; at other times perhaps not to the distance of 100 yards. The same difference is observed in the intervals and violence of the shocks of earthquakes, which are repeated at small intervals for some time.

Sect. 2. The frequency of earthquakes in the neighbourhood of burning mountains is a strong argument of their proceeding from a cause of the same kind: and the analogy of several volcanos lying together in the same tract of country, as well as new ones breaking out in the neighbourhood of old ones, tends greatly to confirm this opinion; but what makes it still the more probable, is that peculiarity in the structure of the earth before mentioned. It has been already observed that the same strata are generally very extensive, and that they commonly lie more inclining from the mountainous countries than the countries themselves: these circumstances make it probable that those strata of combustible materials, which break out in volcanos on the tops of the hills, are to be found at a considerable depth under ground in the level and low countries near them. If this should be the case, and if the same strata should be on fire in any places under such countries, as well as on the tops of the hills, all vapours, of whatever kind, raised from these fires, must be pent up, unless so far as they can open themselves a passage between the strata; whereas the vapours raised from volcanos find a vent, and are discharged in blasts from their mouths. Now if, when they find such a vent, they are yet capable of shaking the country to the distance of 10 or 20 miles round, what may we not expect from them when they are confined? We may form some idea of the force and quantity of these vapours from their effects: it is no uncommon thing to see them throw up at once such clouds of sand, ashes, and pumice stones, as are capable of darkening the whole air, and covering the neighbouring country with a shower of dust, &c. to some miles distance: great stones also of some tons weight are often thrown to the distance of 2 or 3 miles by these explosions; and Mons. Bouguer tells us, that he met with stones in South America, of 8 or 9 feet diameter, that had been

thrown from the volcano Cotopaxi, by one of these blasts, to the distance of more than 3 leagues.

If we suppose that these vapours, when pent up, are the cause of earthquakes, we must naturally expect that the most extensive earthquakes should take their rise from the level and low countries; but more especially from the sea, which is nothing else than waters covering such countries. Accordingly we find that the great earthquake of Nov. 1, 1755, which was felt at places near 3000 miles distant from each other, took its rise from under the sea; as is manifest from that wave which accompanied it. The same thing is to be understood of the earthquake that destroyed Lima in the year 1746, which, it has been said, was felt as far as Jamaica; and as it was more violent than the Lisbon earthquake, so if this be true, it must probably have been more extensive also. There have been many other very extensive earthquakes in South America: Acosta says that they have been often known to extend themselves 1, 2, or 300, and some even 500 leagues along the coast. These have been generally, if not always, attended with waves from the sea.

Sect. 3. Mr. M. having said before, that he imagined earthquakes were caused by vapours raised from waters suddenly let out upon subterraneous fires, does not easily find any other cause capable of producing such sudden and violent effects, or of raising such an amazing quantity of vapour in so small a time. That the blasts discharged from volcanos are always produced from this cause is highly probable; that they are often so cannot admit of the least doubt. There can be no doubt, that considerable quantities of water must be often let out upon the fires of these volcanos, and whenever this happens it will be immediately raised by their heat into a vapour, whose elastic force is capable of producing the most violent effects.*

* There are many effects produced by the vapour of water, when intensely heated, which make it probable, that the force of gunpowder is not near equal to it. The effects of an exceedingly small quantity of water, on which melted metals are accidentally poured, are such as could in no wise be expected from the like quantity of gunpowder. Founders, if they are not careful, often experience these effects to their cost. An accident of this kind happened about 40 years since, at the casting of 2 brass cannon at Windmill-hill, Moorfields. 'The heat of the metal of the first gun drove so much damp into the mould of the second, which was near it, that as soon as the metal was let into it, it blew up with the greatest violence, tearing up the ground some feet deep, breaking down the furnace, untiling the house, killing many spectators on the spot with the streams of melted metal, and scalding many others in a most miserable manner.' [See the note at the end of process 44th of the English translation of Cramer's Art of Assaying Metals.

Other instances of the violence of vapours raised from water are frequently to be met with: one of Papin's digesters being placed between the bars of a grate, where there was a fire, was after some time burst by the violence of the steam, the fire was all blown out of the grate, and a piece of the digester was driven against the leaf of a strong oak table, which it broke to pieces. [See Phil.

Both the tremulous and wave-like motion observed in earthquakes, may be accounted for from such a vapour. In order to trace more particularly the manner in which these two motions will be brought about, let us suppose the roof over some subterraneous fire to fall in. If this should be the case, the earth, stones, &c. of which it was composed, would immediately sink in the melted matter of the fire below: hence all the water contained in the fissures and cavities of the part falling in would come in contact with the fire, and be almost instantly raised into vapour. From the first effort of this vapour, a cavity would be formed (between the melted matter and superincumbent earth) filled with vapour only, before any motion would be perceived at the surface of the earth: this must necessarily happen, on account of the compressibility of all kinds of earth, stones, &c. but as the compression of the materials immediately over the cavity, would be more than sufficient to make them bear the weight of the superincumbent matter, this compression must be propagated on account of the elasticity of the earth, in the same manner as a pulse is propagated through the air; and again the materials immediately over the cavity, restoring themselves beyond their natural bounds, a dilatation will succeed to the compression; and these two following each other alternately for some time, a vibratory motion will be produced at the surface of the earth. If these alternate dilatations and compressions should succeed each other at very small intervals, they would excite a like mo-

Trans. No. 454.] The Marquis of Worcester also, in his Century of Inventions, tells us that he burst a cannon by the same means.

It has been sometimes imagined that the vapours which occasion earthquakes were of the same kind with those fulminating damps, of which we often meet with instances in coal mines. Now, there are several things which make it very probable that this is not the case: it is true, the force of such vapours is very great; we have had instances where large beams of timber have been thrown to the distance of 100 yards by them: (See Phil. Trans. No. 136) but what is this to the force of that vapour which could throw stones of 20 or 30 tons weight to the distance of 3 leagues? Nor indeed is it at all probable that any vapour, already in the form of a vapour, can, by suddenly taking fire, increase its dimensions so much as to produce that immense quantity of motion which we observe in some earthquakes: but this is rather to be expected from some solid body, such as water, which is capable of being converted, and that almost instantly, into one of the lightest, and perhaps one of the most elastic, vapours in the world. Air, when heated to the greatest degree that it is capable of receiving from the hottest fires we can make, acquires a degree of elasticity about 5 times as great as that of common air; the vapour of gunpowder, while it is inflamed, has also about 5 times the elastic force which it has when cold. (See Robins's excellent tract on Gunnery.) Now if we suppose a fulminating damp, of any kind, to increase its elasticity, when inflamed in the same proportion, this will be abundantly sufficient to make it produce any effects, which we have ever seen produced by any of the damps of mines, &c. And indeed whoever carefully examines the effects, either of the damps of mines, or of those fulminating damps that are raised from some metals, when in fusion, or when they are dissolving in acids, will rather be inclined to think that the force of inflamed vapours is so far from exceeding the proportion of 5 to one, that it falls considerably short of it.—Orig.

tion in the air, and thereby occasion a considerable noise. The noise that is usually observed to precede or accompany earthquakes, is probably owing partly to this cause, and partly to the grating of the parts of the earth together, occasioned by that wave-like motion before mentioned. After the water, that first came in contact with the fire, has formed a cavity, all the rest of the water contained in the fissures, immediately communicating with the hollow left by the part that fell in, must run out upon the fire, the steam taking its place. Hence may be generated a vast quantity of vapour, the effects of which shall be considered presently. This steam will continue to be generated, supposing the fire to be sufficiently great, till the fissures before mentioned are evacuated, or till the water begins to flow very slowly; when the steam already formed will be removed by the elasticity of the earth, which will again subside, and pressing on the surface of the melted matter, will force it up a little way into all the clefts, by which the water might continue to flow out. By this means all communication between the fire and the water will be prevented, excepting at these clefts, where the water, dripping slowly on the melted matter, will gradually form a crust on it, that will soon stop all further communication in these places likewise; and the fissures, that had been before evacuated, will be again gradually replenished by the oozing of the water between the strata.

As a small quantity of vapour almost instantly generated at some considerable depth below the surface of the earth, will produce a vibratory motion, so a large quantity will produce a wave-like motion. The manner in which this wave-like motion will be propagated, may in some measure be represented by the following experiment. Suppose a large cloth or carpet spread on a floor, to be raised at one edge, and then suddenly brought down again to the floor, the air under it being by this means propelled, will pass along till it escapes at the opposite side, raising the cloth in a wave all the way as it goes. In like manner, a large quantity of vapour may be conceived to raise the earth in a wave, as it passes along between the strata, which it may easily separate in a horizontal direction, there being little or no cohesion between one stratum and another. The part of the earth that is first raised, being bent from its natural form, will endeavour to restore itself by its elasticity, and the parts next to it beginning to have their weight supported by the vapour, which will insinuate itself under them, will be raised in their turn, till it either finds some vent, or is again condensed by the cold into water, and by that means prevented from proceeding any farther.

If a large quantity of vapour should continue to be generated for some time, several waves might be produced by it; and this would be in some measure the case if the quantity at first generated was exceedingly great, though the whole of it was generated in less time than while the motion was propagated through the distance between two waves. These waves must rise the higher, the nearer they

are to the place whence they have their source; but at great distances from it, they may rise so little, and so slowly, as not to be perceived, but by the motions of waters, hanging branches in churches, &c. The vibratory motion occasioned by the first impulse of the vapour, will be propagated through the solid parts of the earth, and therefore it will much sooner become too weak to be perceived than the wave-like motion; for this latter, being occasioned by the vapour insinuating itself between the strata, may be propagated to very great distances, and even after it has ceased to be perceived by the senses, it may still discover itself by the appearances before-mentioned.

Sect. 4.—All earthquakes derived from the same subterraneous fire must come to the same place in the same direction; and those only which are derived from different fires will come from different points of the compass; but probably as it seldom happens that earthquakes, caused by different fires, affect the same place, we therefore find in general that they come from the same quarter; it is not however to be supposed that this should always be the case, for it will probably sometimes happen to be otherwise; and this is to be expected in such places as are situated in the neighbourhood of several subterraneous fires; or where, being subject to the shocks of some local earthquake of small extent, they now and then are affected by an earthquake, produced by some more distant, but much more considerable cause. Of this last case we seem to have had some instances in the earthquake of Nov. 1, 1755, and those local ones which succeeded it.

As we may reasonably infer from many earthquakes coming to the same place, from the same point of the compass, that they are all derived from the same cause, and that a permanent one; so we may reasonably infer the same thing also from their being propagated with the same velocity; but this argument will still come with the greater force, if it be considered that the velocity of any vapour, which insinuates itself between the strata of the earth, depends on its depth below the surface: for the deeper it lies the greater will be its velocity. We may therefore conclude from the sameness of the velocity of the earthquakes of the same place that their cause lies at the same depth; and from the inequality of the velocity of the earthquakes of different places, that their causes lie at different depths. Both these are consistent with the supposition, that earthquakes owe their origin to subterraneous fires, since the strata in which these subsist may be easily conceived to lie at different depths in different parts of the world.

Sect. 5.—From the same cause we may easily account for those local earthquakes, which succeed the greater and more extensive ones. If there are many subterraneous fires subsisting in different parts of the world, the vapour coming from one fire may well be supposed, as it passes, to disturb the roof over some

other fire, and by that means occasion earthquakes, by the falling in of some part of it; and this may be the case, in some measure, even where the vapour passes at some small distance over the fire; but it will be most likely to take place where the vapour either passes at some distance under it, or between the stratum, in which the fire lies, and that next above or below it.

PART III. *Sect* 1.—In the former part of this tract, a part of the roof over some subterraneous fire was supposed to fall in; this is an event that cannot happen merely accidentally; for so long as the roof rests on the matter on fire, no part of it can fall in, unless the matter below could rise and take its place; now, it is very difficult to conceive how this should happen, unless it was to rise by some larger passages than the ordinary fissures of the earth, which seem much too narrow for that purpose; for besides that the melted matter cannot be supposed to have any very great degree of fluidity, it must necessarily have a hard crust formed on it, at all the fissures, by the long continued contact of the water contained in them: these impediments seem too great to be overcome by the difference of the specific gravities of the part that is to fall in, and the melted matter, which is the only cause that can tend to make it descend; the manner therefore, in which he supposes this event may be brought about, is as follows:

The matter of which any subterraneous fire is composed, must be greatly extended beyond its original dimensions by the heat. As this will be brought about gradually, while the matter spreads itself, or becomes hotter, the parts over the fire will be gradually raised and bent; and this bending will for some time go on without any other consequence; but as the fire continues to increase, the earth will at last begin to be raised somewhat beyond the limits of it. By this means an annular space will be formed at the edges next to the fire, and surrounding it, a vertical section of which space, through a diameter of the fire, will be 2 long triangles, the shortest side or base of each lying next the fire, and the 2 longer sides being formed by the upper and lower strata, which will be separated for a considerable extent, proportionably to the distance through which they are raised from each other. This space will be gradually filled with water, as it is formed, the melted matter being prevented from filling it, by its want of fluidity, as well as on account of the other circumstances, under which it is to spread itself; for the lentor and sluggishness of this kind of matter is such, that, when somewhat cooled on the surface by the contact of the air only, it will not flow perhaps 10 feet in a month, though in a very large body, instances of which we have in the lavas of Etna, Vesuvius, &c. It is not to be expected then that it should spread far when it comes in contact with water at its edges as soon as it is formed, and when it is perhaps several months in acquiring a thickness of a few inches; but it must, by degrees, form a kind of wall between the fire and the opening into the annular space before described. This wall will gradually

increase in height, till it becomes too tall in proportion to its thickness to bear any longer the pressure of the melted matter; which must necessarily happen at last, because its thickness will not exceed a certain limit. Besides the giving way of this wall, the fire may undermine the space containing the water, and by that means open a communication between them. Let us suppose one of these to come to pass, and the time arrived when the partition begins to yield. If then the water had any way to escape readily, the breach would be made, and the melted matter would burst forth immediately, and flow out in large quantities at once among it; but as this is not the case, and it can only escape by oozing slowly between the strata, and through the fissures, the way that it came, the breach will be made gradually; whence we may account for some appearances that have preceded great earthquakes.

We are told that 2 or 3 days before an earthquake in New England, the waters of some wells were rendered muddy, and stunk intolerably; why might not this be occasioned by the waters contained in the spaces before described, which, being impregnated with sulphureous steams, were driven up, and mixed with the waters of the springs? At least, there can be no doubt, by whatever means it was brought about, that this phenomenon was owing to the same cause, already beginning to exert itself, which afterwards gave rise to the succeeding earthquake.

Something like this happened before the Lisbon great earthquake of 1755. We are told, that at Colares, about 20 miles from it, "in the afternoon preceding the 1st of November, the water of a fountain was greatly decreased; on the morning of the 1st of November it ran very muddy, and after the earthquake it returned to its usual state, both in quantity and clearness." The same author says, a little lower, "in the afternoon of the 24th, I was much apprehensive that the following days we should have another great earthquake; for I observed the same prognostics as in the afternoon of Oct. 31, that is," &c. "And I further observed that the water of a fountain began to be disturbed to such a degree, that in the night it ran of a yellow clay colour; and from midnight to the morning of the 25th I felt 5 shocks, one of which seemed as violent as that of the 11th of December." But the most extraordinary appearance of any that preceded this earthquake was that of the agitation of the waters of Lochness, and some others of the lochs in Scotland, about half an hour before any motion was felt at Lisbon, though the cause of all these great effects could not lie far from thence, and certainly lay to the south of Oporto. Nor is it probable that there should be any mistake in the time, not only because the difference is too great, as well as the concurrent testimonies too many, to admit of such a solution; but because they mention another greater agitation that happened about an hour and half after the former; which latter agrees with the

times when the agitations of the waters were observed in England, if we allow only a proper interval for the motion to be propagated so far northward, proportionably to the time it took up in travelling from its original source near Lisbon.

These appearances seem to be connected with that mentioned in the preceding article, and they probably may both be accounted for, by supposing a considerable quantity of vapour to be raised, while the partition before-mentioned was beginning to give way; during which time a partial communication between the water and fire would be brought on, and that by degrees only. Hence the vapour, not being produced at once but gradually, might creep silently between the strata, towards that quarter where the superincumbent mass of earth was lightest; and by this means some places very near the source of the vapour might be little, or not at all, affected by it, while others might be greatly affected, though they lay at a great distance; and even those places, which lay immediately over the part where the vapour was passing, might not perceive any effect, on account of the gentleness of the motion occasioned by the small quantity of it. This might continue to be the case, till it came to some country where, the set of strata above being much thinner, the vapour would not only be hurried forward, but collected also into a much narrower compass; and therefore, raising the earth more, would produce more sensible effects; and this we ought chiefly to expect in the most mountainous countries, according to the idea before given of them.

Sect. 2.—We are told that in the Lisbon earthquake of 1755, "the bar (at the mouth of the Tagus) was seen dry from shore to shore; then suddenly the sea, like a mountain, came rolling in: and about Bellem castle the water rose 50 feet almost in an instant: and had it not been for the great bay opposite to the city, which received and spread the great flux, the low part of it must have been under water." The same phenomena were observed to accompany the same earthquake at the island of Madeira: where we are told that at the city of Funchal, "the sea, which was quite calm, was observed to retire suddenly some paces: then rising with a great swell, without the least noise, and as suddenly advancing, it overflowed the shore, and entered the city. It rose full 15 feet perpendicular above high-water mark, though the tide, which ebbs and flows there 7 feet, was then at half ebb. In the northern part of the island the inundation was more violent, the sea retiring there above 100 paces at first, and suddenly returning, overflowed the shore, forcing open doors, breaking down the walls of several magazines and storehouses, and carrying away, in its recess, a considerable quantity of grain, and some hundred pipes of wine."

Both these appearances seem to admit of an easy solution, supposing their cause to lie under the bed of the ocean: for in the farther progress of the communication between the fire and water, the vapour, that is gradually raised at first, will at last begin to raise the roof over the fire, which being supported by

so light a vapour, there will now be no want of fluidity in the matter it rests on, and the difference of specific gravity between the two, instead of being small, will be very great; hence, if any part of the roof gives way, it must immediately fall in, the vapour readily rising and taking its place; and a beginning being once made, a communication will be opened with numberless clefts and fissures, that must occasion the falling in of vast quantities of matter, which, as soon as the vapour can pass round them, will want their support: then will follow the great effects already described. Now while the roof is raising, the waters of the ocean over it must retreat, and flow from thence every way; this however being brought about slowly, they will have time to retreat so gently, as to occasion no great disturbance; but as soon as some part of the roof falls in, the cold water contained in its fissures, mixing with the steam, will immediately produce a vacuum, in the same manner as the water injected into the cylinder of a steam engine, and the earth subsiding, and leaving a hollow place above, the waters will flow every way towards it, and cause a retreat of the sea on all the shores round about; then presently the waters being again converted by the contact of the fire into vapour, together with all the additional quantity which has now an open communication with it, the earth will be raised, and the waters over it will be made to flow every way, and produce a great wave immediately succeeding the previous retreat.

Sect. 3.—That great quantity of water, which we have supposed to be let out on subterraneous fires, and by that means to produce earthquakes, will supply us with a reason why they observe a sort of periodical return. This water must extinguish a great portion of the burning matter, in consequence of which it will be contracted within much narrower bounds; and though the effects before described could not take place at first, but by the great extension of the heated matter, yet after they have once taken place, they may well continue to do so for some time; for the great disturbance in the first instance, by the falling in of a great part of the roof, must render the frequent communication between the fire and water not only very easy, but almost unavoidable; and this will continue to be so till the roof is well settled, and the surface of the melted matter sufficiently cooled, after which it may require a long time for the fire to heat it again so much as will be necessary to make it produce the former effects. Now, as the matter has been more or less cooled, or as the combustible materials are with more or less difficulty set on fire again, as well as on account of other circumstances, the returns of these effects will be later or earlier; but though they will not, for this reason, observe any exact period, yet they will generally fall within some sort of limits, till either the matter that occasions them is consumed, or till the fires open themselves a passage, and become volcanos.

Sect. 4.—It has been already intimated, that the most extensive earthquakes

frequently take their rise from the sea. According to the description of the structure of the earth before given, any combustible stratum must lie at greater depths in places under the ocean than elsewhere; hence far more extensive fires may subsist there than where the quantity of matter over them is less; for any vapour raised from such fires, having both a stronger roof over it, and being pressed by a greater weight, beside the additional weight of the water, will not only be less at liberty to expand itself, and consequently of less bulk, but it will also be easily driven away towards the parts round about, where the superincumbent matter is less, and therefore lighter. On the other hand, any vapour raised from fires, where the superincumbent matter is lighter, finding a weaker roof over it, and being not so easily driven away under strata that are thicker and heavier, will be very apt to break through, and open a mouth to a volcano; and it must necessarily do this long before the fires can have spread themselves sufficiently to be nearly equal to those which may subsist in places that lie deeper. All this seems to be greatly confirmed by the situation of volcanos, which are almost always found on the tops of mountains, and those often some of the highest in the world.

If then the largest fires are to be supposed to subsist under the ocean, it is no wonder that the most extensive earthquakes should take their rise from thence: the great earthquake of Lisbon has been shown to have done so; and that the cause of it was also at a greater depth than that of many others, appears from the greater velocity with which it was propagated.

The great earthquake that destroyed Lima and Callao, in 1746, seems also to have come from the sea; for several of the ports on the coast were overwhelmed by a great wave, which did not arrive till 4 or 5 minutes after the earthquake began, and which was preceded by a retreat of the waters as well as that at Lisbon. Against this it may perhaps be alleged, that there were 4 volcanos broke out suddenly in the neighbouring mountains, when this earthquake happened, and that the fires of these might be the occasion of it. This however is not very probable; for, to omit the argument of the wave and previous retreat of the waters, already mentioned, it is not very likely that more than one fire was concerned: besides, the vapour, opening itself a passage at these places, could not well be supposed, if it took its rise from thence, to spread itself far; especially towards the sea, where it is manifest that the strata over it were of great thickness, as appears from the great velocity with which the earthquake was propagated there; the shocks also continued with equal, or nearly equal, violence, for some months after the openings were made; whereas, if these fires had been the cause of them, they must immediately have ceased on the fires finding a vent, as it has happened in other cases. It is therefore much more probable that a very large quantity of vapour, taking its rise from some far

more extensive fire under the sea, spread itself from thence, and as it passed in places where the roof over it was naturally much thinner, as well as greatly weakened by the undermining of these fires, it opened itself a passage and burst forth.

As the most extensive earthquakes generally proceed from the lowest countries, but especially from the sea, so those of a smaller extent are generally found among the mountains: hence it almost always happens that earthquakes which are felt near the sea, if at all violent, are felt also in the higher lands; whereas there are many among the hills, and those very violent ones, which never extend themselves to the lower countries. Thus we are told that at Jamaica "shakes often happen in the country, not felt at Port Royal: and sometimes are felt by those that live in and at the foot of the mountains, and by nobody else." On the other hand, the earthquake that destroyed Port Royal extended itself all over the island: and the same was observed of a smaller earthquake that happened there in 1687-8; which latter undoubtedly came from the sea, as appears by Sir Hans Sloane's account of it.

Earthquakes of small extent are also very common among the mountains of Peru and Chili. Antonio d'Ulloa says, "Whilst we were preparing for our departure from the mountain Chichi-Choco, there was an earthquake which was felt 4 leagues round about: our field tent was tossed to and fro by it, and the earth had a motion like that of waves; this earthquake however was one of the smallest that commonly happen in that country." The same author tells us, in another place, that "during his stay at the city of Quito, or in the neighbourhood of it, there were two earthquakes, violent enough to overturn some houses in the country, which buried several persons under their ruins."

Sect. 5.—It is generally found that earthquakes in hilly countries are much more violent than those which happen elsewhere; and this is observed to be the case, as well when they take their rise from the lower countries as among the hills themselves. This appearance being so easily to be accounted for, from the structure of the earth already described, Mr. M. contents himself with establishing the certainty of a fact which tends so greatly to confirm it.

The earthquakes that have infested some of the towns in the neighbourhood of Quito, have not only been incomparably more violent than that which destroyed Lisbon, but they seem to have exceeded that also which destroyed Lima and Callao. In Lisbon, many of the houses were left standing, though few of them were less than 4 or 5 stories high. At Lima also, it is only said that "all the buildings, great and small, or at least the greatest part of them, were destroyed." Callao likewise, as it appears from the accounts we have of it, had many houses left unhurt by the earthquake, till the wave came which overwhelmed the whole town, and threw down every thing that lay in its way. All

these effects seem to be greatly short of those produced by an earthquake that happened at Latacunga, in the year 1698, when the whole town, consisting of more than 600 houses, was entirely destroyed in less than 3 minutes time, a part of one only escaping; notwithstanding that the houses there are never built more than one story high in order, if possible, to avoid these dangers. Ambato, a village about the same size as Latacunga, together with a great part of Riobamba, another town in the same neighbourhood, were also entirely destroyed, or received considerable damage from it. At the same time a volcano burst out suddenly in the neighbouring mountain of Carguayraso: and "near Ambato the earth opened in several places, and there yet remains to the south of that town a cleft of 4 or 5 feet broad, and about a league in length, lying north and south; there are also several other like clefts on the other side of the river." The city of Quito was affected at the same time, but received no damage, though it is no more than 42 geographical miles from Latacunga, not far from which the greatest violence of the shock seems to have exerted itself. These towns are supposed to stand by far the highest of any in the world, being as high above the level of the sea as the tops of some of the highest mountains in Europe; and the ground on which Riobamba stands, wants but 90 yards of being 3 times as high as Snowdon, the highest mountain in Wales.

The country on which these towns stand serves as a base, from which arise another set of high lands and mountains, which are much the highest in the known world. Among these mountains there are no less than 6 volcanos, if not more, within an extent of 120 miles long, and less than 30 broad, the lowest of which exceeds the height of Riobamba by above $\frac{2}{3}$ of a mile, and the highest by more than twice that quantity. Now, as the earthquakes have been more violent at the foot of these mountains than in the lower lands, so they have been still more violent towards the tops of them: this is sufficiently manifest from the many rents made in them, and the rocks that have been broken off from them on such occasions: but it appears still more manifestly, and beyond all dispute, in the bursting forth of volcanos, which are almost always at the very summit of the mountains, where they are found. In these instances, the earth, stones, &c. which lay over the fire, are generally scattered by the violence of the vapour, that breaks its way through, to the distance of some miles round about.

The great earthquake of the 1st of November 1755, was also more violent among the mountains than at the city of Lisbon. We are told that "the mountains of Arrabida, Estrella, Julio, Marvan, and Cintra, being some of the largest in Portugal, were impetuously shaken, as it were, from their very foundations; and most of them opened at their summits, split and rent in a wonderful manner, and huge masses of them were thrown down into the subjacent vallies."

The same was observed at Jamaica likewise. In the earthquake that destroyed Port-Royal in 1692, we are told, that " more houses were left standing at that town than in all the island besides. It was so violent in other places that people were violently thrown down on the ground, where they lay with their legs and arms spread out, to prevent being tumbled about by the incredible motion of the earth. It scarcely left a planter's house or sugar-work standing all over the island: I think it left not a house standing at Passage-fort, and but one in all Liganee, and none in St. Iago, except a few low houses, built by the wary Spaniards. In Clarendon precinct the earth gaped, and spouted up with a prodigious force great quantities of water into the air, 12 miles from the sea; and all over the island there were abundance of openings of the earth, many thousands. But in the mountains, are said to be the most violent shakes of all; and it is a generally received opinion, that the nearer to the mountains the greater the shake; and that the cause thereof, whatever it is, lies there. Indeed they are strangely torn and rent, especially the blue, and other highest mountains, which seem to be the greatest sufferers, and which, during the time that the great shakes continued, bellowed out prodigious loud noises and echoings.

" Not far from Yallowes, a mountain, after having made several moves, overwhelmed a whole family, and a great part of a plantation lying a mile off; and a large high mountain near Portmorant, near a day's journey over, is said to be quite swallowed up.

" In the blue mountains, whence came those dreadful roarings, may reasonably be supposed to be many strange alterations of the like nature; but those wild desert places being very rarely, or never, visited by any body, we are yet ignorant of what happened there; but whereas they used to afford a find green prospect, now one half part of them, at least, seem to be wholly deprived of their natural verdure."

SECT. VI.—Mr. M. has supposed that fires lying at the greatest depths generally produce the most extensive earthquakes; we must however except from this rule those cases where the depths are very great: for, as the weight of 3 miles perpendicular of common earth is capable of absolutely repressing the vapour of inflamed gunpowder, so we may well suppose that there may be a quantity of earth sufficient to repress the vapour of water, and keep it within its original limits, though ever so much heated. Now, whenever this is the case, it is manifest that it can produce no effect: or it may happen, that though the quantity of earth may not be sufficient absolutely to repress the vapour, yet it may be so great, as to suffer it to expand but very little: in this case an earthquake arising from it would be but of small extent; the wave-like motion would be little or none; the vibratory motion would be felt every where; and the propagation of the motion would be very quick. This last circumstance being almost the only

one, by which these earthquakes can be known from those which owe their origin to shallower fires, it must be very difficult to distinguish them with certainty, as it is almost impossible to distinguish the difference of the time of their happening in different places, when the whole perhaps is comprehended within the space of 2 or 3 minutes; possibly however some of the earthquakes which we have had in England may have been of this class.

Sect. 7.—If we would inquire into the place of the origin of any particular earthquake we have the following grounds to go upon: 1st, The different directions in which it arrives at several distant places: if lines be drawn in these directions, the place of their common intersection must be nearly the place sought: but this is liable to great difficulties; for there must necessarily be great uncertainty in observations which cannot, at best, be made with any great precision, and which are generally made by minds too little at ease to be nice observers of what passes; also the directions themselves may be somewhat varied, by the inequalities in the weight of the superincumbent matter, under which the vapour passes, as well as by other causes. 2ndly, We may form some judgment concerning the place of the origin of a particular earthquake from the time of its arrival at different places; but this also is liable to great difficulties. In both these methods however we may come to a much greater degree of exactness by taking a medium among a variety of accounts, as they are related by different observers. But, 3dly, we may come to the greatest degree of exactness in those cases where earthquakes have their source from under the ocean; for in these instances the proportional distance of different places from that source may be very nearly ascertained by the interval between the earthquake and the succeeding wave: and this is the more to be depended on, as people are much less likely to be mistaken in determining the time between two events which follow one another at a small interval than in observing the precise time of the happening of some single event.

Let us now, by way of example, endeavour to inquire into the situation of the cause that gave rise to the earthquake of Nov. 1st, 1755, the place of which seems to have been under the ocean, somewhere between the latitudes of Lisbon and Oporto, (though probably somewhat nearer to the former,) and at the distance perhaps of 10 or 15 leagues from the coast. For, 1st, the direction in which the earthquake arrived at Lisbon was from the north-west; at Madeira it came from the north-east; and in England it came from the south-west; all of which perfectly agree with the place assumed. 2dly, The times in which the earthquake arrived at different places agree also with the same point. And, 3dly, the interval between these, and the time of the arrival of the subsequent wave, concur in confirming it. That all this might the better appear, Mr. M. subjoined the following table, assuming the point,

from which he computed, at the distance of about a degree of a great circle from Lisbon, and a degree and a half from Oporto. In consequence of this supposition, he added 3 minutes to the interval between the time when the shock was felt at Lisbon, and at the several other places. The first column in the table contains the names of places; the 2d, the distances from the assumed point, reckoned in half degrees; the 3d, the time that the earthquake took up in travelling to each, expressed in minutes; and the 4th contains the time in which the wave was propagated from its source to the respective places, expressed in minutes likewise.

	Half deg.	Min.	Min.
Lisbon	2	3	12
Oporto	3	5	
Ayamonte	6		53
Cadiz	9	12	82
Madrid	9	11	
Gibraltar	11	18	
Madeira	19	25	152
Mountsbay	20		267
Plymouth	21		360
Portsmouth	23	29	
Kingsale	23		290
Swansea	24		530
The Hague	30	32	
Lochness	33	66	
Antigua	98		565
Barbadoes	101		485

In computing the times in this table, allowance was made for the difference of longitude, as it is laid down in the common maps, which are not always greatly to be depended on. The times themselves also are often so carelessly observed as well as vaguely related, that many of them are subject to considerable errors; the concurrent testimonies however are so many that there can be no doubt about the main point; and that the errors might be as small as possible, Mr. M. not only endeavoured to select those accounts that had the greatest appearance of accuracy, but, in all cases where it was to be had, always took a mean among them. In many of the accounts the relaters say only between such hours, or about such an hour.

It is observable, in the preceding table, that the times which the wave took up in travelling are not in the same proportion with the distances of the respective places from the supposed source of the motion; this however is no objection against the point assumed, since it is manifest, wherever it was, that it could not be far from Lisbon, as well because the wave arrived there so very soon after the earthquake as because it was so great, rising, as we are told, at the distance of 3 miles from Lisbon to the height of 50 or 60 feet. The true reason of this disproportion seems to be the difference in the depth of the water; for, in every instance in the above table, the time will be found to be proportionably shorter or longer as the water through which the wave passed was deeper or shallower.

Sect. 8.—If we would inquire into the depth at which the cause lies that occasions any particular earthquake, Mr. M. knows of no method of determining it, which does not require observations not yet to be had; but if such could be procured, and they were made with sufficient accuracy, he thinks some kind of guess might be formed concerning it: for, 1st, in those instances where the

vapour discharges itself at the mouths of volcanos, (as in the case of the earthquake at Lima,) it might perhaps be possible for a careful observer to trace the thickness of the several strata from thence to the place where the earthquake took its rise, or at least as far as the shore, if it took its rise from under the sea. If this could be once done in any one instance, and the velocity of such an earthquake nicely determined, we might then guess at the depth of the cause in other earthquakes, where we knew their velocity, by taking the depths proportional to those velocities, which probably would answer very nearly. 2ndly, If, in any instance, it should be possible to know how much the motion of any earthquake was retarded by passing under the ocean, the depth of the ocean being known, the depth at which the vapour passed would be also; for the velocity under the water would be to the velocity if there had been no water in the subduplicate ratio of the weight in the latter case to the weight in the former; allowing earth to be about $2\frac{1}{2}$ times the weight of water, the depth will be readily found. 3dly, Let us conceive the earth to be formed according to the idea before given of it, and that the same strata are at a medium of the same thickness for a very great extent, as well in those places where several of the upper ones are wanting as where they are not. On this supposition we may discover the depth at which the vapour passes, by comparing the several velocities of the same earthquake in places where the thicknesses of the superincumbent mass are different.

As the observations relating to the earthquake of Nov. 1st, 1755, are too gross, it would be in vain to attempt, by any of the foregoing methods, to determine with any certainty the depth at which the cause of it lay; but if Mr. M. might be allowed to form a random guess about it, he would suppose, (on a comparison of all circumstances,) that it could not be much less than a mile, or a mile and half, and he thinks it is probable it did not exceed 3 miles.

LVI. Observations on the Comet of Feb. 1760. *By the Abbé De la Caille,* F. R. S. Dated Paris, Feb.* 18, 1760. p. 635.

I will venture to send you some of my observations on the present comet, because bad weather may have prevented it from being seen in England.

* M. De la Caille, a celebrated French astronomer, was born at Rumigny, in 1713, and died at Paris in 1762, at only 49 years of age. He was educated at the College of Lisieux at Paris. Here he became the friend of Cassini de Thury, with whom he was associated in projecting the meridian line to pass through France. In 1739 he was appointed professor of mathematics in the Mazarine college; and in 1741 elected a member of the Academy of Sciences. In 1750 he went to the Cape of Good Hope to observe the stars in the southern hemisphere. His writings are very numerous, excellent, and greatly esteemed, especially his Elements of Astronomy.

	Equal time.	Longitude.	North lat.
Feb. 8	$9^h\ 29^m\ 28^s$	♌ $20^\circ\ 17'\ 22''$	$3^\circ\ 26'\ 42''$
9	8 48 50	18 49 18	4 46 28
11	7 22 35	16 5 3	7 14 50
13	7 47 4	14 43 21	8 24 18
14	6 41 0	12 20 18	10 30 9

These observations, with another made at Marseilles, on the first day, at $9^h\ 55^m\ 38^s$ equal time, when the longitude of the comet was found in ♌ $23^\circ\ 29'\ 46''$, and its north latitude $31'\ 20''$, enabled him to compute the elements of its orbit. Its motion is direct. The ascending node is in ♌ $19^\circ\ 42'\ 0''$, and the place of the perihelion in ♉ $26^\circ\ 41'\ 22''$. The inclination of the orbit is $80^\circ\ 51'\ 30''$, and the distance of the perihelion $\frac{85248}{100000}$ of the radius of the orbit of the earth. The comet passed the perihelion Nov. 25, 1759, at $20^h\ 55^m$, mean time at Paris.

LVII. Extracts of some Letters from Signor Abbate de Venuti, F. R. S. to J. Nixon, A. M., F. R. S. relating to several Antiquities in Italy. p. 636.

Sig. V. had bought a relievo, which in his opinion was very singular. It was of marble, 2 palms wide, and 1 high, and represented in a neat taste, a faun, with a tail and wings; which latter circumstance had never occurred to his observation before. He seemed to be dancing, and his dog at his feet in the same attitude. Near him was a tree, to which was tied a very elegant open chariot (thensa,) and beneath it there appeared a table, such as were used in entertainments, with a goblet on it, charged with relievo in embossed work.

He also met with a cornelian, on which was engraved a man cloathed with a pallium, and sitting on a chair: before him there appeared a lighted furnace, and on it a vessel of glass, or earthen ware. The artist himself held in his hand a pair of iron pincers, with handles, to take off the vessel from the fire, without burning himself.

Remarks on the preceding Extracts. By John Nixon, A. M., F.R.S. p. 639.

These throw no light on the above antiquities.

LVIII. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1759. By John Wilmer, M.D. p. 644.

This is the 38th presentation of this kind, completing to the number of 1900 different plants.

LIX. Of the Animal sent from the East Indies, by General Clive, to His Royal Highness the Duke of Cumberland, which is now in the Tower of London. By James Parsons, M.D., F.R.S.* p. 648.

This animal is something taller than the largest sized cat, being about 15 inches high at the shoulders; slender and light, though strong. The head is small in proportion to the rest, and the neck slender. It has nothing fierce in its aspect, but is mild, and very tame. It is exactly of a fawn-colour, having its ears black on their outsides, and lined with white hairs, and some white round the root of each ear; it is also white under the throat and belly, and a little so on the backs of its limbs. Its eyes are small, and its head like that of a cat, but somewhat slenderer; its legs are genteel and straight, with the paws of a cat, having the power of dilating and contracting its toes, which are armed with strong crooked nails, in the same manner as a cat or tyger does; and its actions are like those of a cat. Dr. P. sat and watched its motions, and saw it lick its foot, and rub it over its face several times exactly like a cat; and was told by the man who showed it, that when offended, it hisses. Dr. P. examined its teeth, and found them in the same number and manner with those of a cat. As to its food, they give it raw mutton every day; and when sick, which it often is, they give it a live fowl, or rabbit, which it seizes eagerly, and lies upon it without motion, for a considerable time, to suck the blood, and this proves a certain cure. The tail is like that of a cat also.

None of the natural historians have yet any account of this animal, except the learned Dr. Walter Charleton, who has a bad figure of it, where the head is, contrary to truth, very large and strong in appearance, the tail like that of a fox, and the whole as strong as a mastiff dog: the name given it in the plate is the same with this, but differently spelled, thus, Siyah-ghush.

LX. Of the Frog-fish of Surinam. By Mr. George Edwards, F.R.S. p. 653.

In the appendix to Merian's Nat. History of the Insects of Surinam, where she treats of the transformation of fishes into frogs, and of frogs into fishes, after explaining how the European frog is changed from a minute fish, or tad-

* This animal is the *Felis Caracal* of the Gmelinian edition of the Systema Naturæ of Linneus, and belongs to that division in the genus which contains such species as have the ears terminated by pencil of hairs. It is a native of the eastern regions, and particularly, (as it is said) of Persia. Its general size is that of a fox, and its colour a light reddish bay. The figure given by Dr. Parsons is by no means remarkable for its elegance. In the Count de Buffon's History of Quadrupeds is a much superior representation. The caracal is an animal of great strength and fierceness, and is said to be occasionally tamed by the Persians, and used for the chace of various game.

pole, into a perfect frog, she proceeds to describe the gradual transformation of a species of frogs found in great numbers in the river of Surinam, into perfect fishes, and gives 5 figures to illustrate her description; the subjects she says were then in the collection of Albert Seba at Amsterdam, from whom she also had her figures and information, as appears since by the account published by Mr. Seba of his curious cabinet of natural history, in two folio volumes, a copy of which, finely illuminated, is now in the British Museum.*

LXI. Of a Remarkable Operation on a Broken Arm. By Mr. Charles White, Surgeon at Manchester. Dated Manchester, March 27th, 1760. p. 675.

This communication contains an account of a fracture of the humerus in a boy 9 years old, which not being united at the expiration of 6 months from the accident, Mr. White recommended (instead of amputation which had been proposed) to make a longitudinal incision down to the bone, to bring out one of the ends of it, (which might be done with great ease as the arm was very flexible) and to cut off the oblique end either by the saw or cutting pincers; then to bring out the other end of the bone, and to cut off that likewise; afterwards to replace them end to end, and then treat it entirely as a compound fracture. This proposal was at length acceded to, and the operation was performed. After the dressings were finished, the limb was placed in a fracture box contrived on purpose; the lad was confined to his bed, and the rest of the treatment was the same as that of a compound fracture.

The wound was nearly healed in a fortnight's time, when an erysipelas came on, and spread itself all over the arm, attended with some degree of swelling: this by fomentations and the antiphlogistic method soon went off, and the cure proceeded happily without any interruption. In about 6 weeks after the operation, the callus began to form, and was grown quite firm at the above date; that arm was as long as the other, but somewhat smaller by such long continued bandage; he daily acquired strength in it, and it was thought he would soon be fit to be discharged from the hospital.

* The history of the *Rana paradoxa* of Linneus, and of its tadpole, commonly called the Frog-fish of Surinam, is now much better understood than in the days of Madam Merian and Seba, when the absurdities mentioned in the account here extracted by Edwards, seem to have been generally believed. The larva or tadpole in this species of frog is of larger size than usual, and even exceeds that of the full-formed animal when first arrived at its perfect state. This, however, is not entirely peculiar to the present species, but takes place in some others. In the British Museum is a specimen both of the Rana paradoxa and of its tadpole, which appear to have been wanting in that collection at the time the present paper was written.

LXII. *Of a Bone found in the Pelvis of a Man at Brussels. By Terence Brady, M.D.* p. 660.

Herewith was sent a draught of a bone found in the pelvis or basin of a man that died in the military hospital of Brussels, the 12th of March 1760, of a 7 days inflammatory distemper. This extraordinary concretion weighed about 20 oz., had all the external appearances of a bone, with the hardness, solidity, and specific gravity of common stone. It was chequered or marbled, so that the primitive particles of the bone were seen to be whiter and harder than the darker part: it was formed on the lower extremity of the mesocolon, and probably as it grew large was carried down by its own weight into the basin, where it had no adherence to, nor connection with, any of the adjacent parts, but lay in its own very thin membrane or periosteum, between the os pubis and the bladder, somewhat to the right side. It was joined to the mesenterium by a tough, compact, membranaceous, glandulous substance, in the form of an inverted cone, whose point was firmly inserted in the cavity observed on the top: here the membranaceous fibres were turned into bones, or, vice versa, the fibres of bones degenerated into fleshy membranes: there was no intermediate cartilaginous substance to be observed. By drawing up this conic body with very little effort, the bone followed, to the great surprize of all the spectators; after which there was no further inquiry made in regard to the other viscera of the abdomen. It was only taken notice of, that the omentum was quite consumed, and the mesenterium very much swelled and schirrous.

The man that bred this monstrous bone lived to the age of 55, of which he was 28 years a musketeer in Bareith's imperial regiment. He was always strong and healthy till about 5 years before, when he began to complain of the hardness of his belly, and now and then of a suppression of urine, of which last inconveniency he could help himself by turning on his right side, and lying a little on his face: in that position, the bone did no more press on his bladder. He never missed doing soldier's duty till his last sickness, about 7 days before his death.

We have examples of membranes, and of several soft parts of the body, being ossified; but Dr. B. believes there is not such a monstrous production as this to be seen any where. About 20 years before, he saw at Mantua, 2 inches of the aorta near the heart turned to bone, in a man that was a long time tormented with a violent palpitation of the heart.

LXIII. *An Extraordinary Case of a Lady, who swallowed Euphorbium. By Dr. Willis, of Lincoln.* p. 662.

In Dec. 1758, Mrs. Willis of Lincoln fell into a slow fever, occasioned by

too small a discharge of the lochia after lying-in, and a redundancy of milk, the consequence of her not suckling her child. On the 18th day after her delivery, by the mistake of her nurse, she took, instead of a draught that was ordered for her, 2 oz. of the tincture of euphorbium.* The shocking symptoms which immediately ensued, violent suffocation, and an intolerable burning pain in the mouth, throat, and stomach, soon discovered the horrible mistake. Dr. W. was in the room in about 4 or 5 minutes after the accident happened, unapprised of the nature of it, and therefore the more shocked, when he found every body in tears of despair, offering no means of relief, as they had no hopes of success.

When made acquainted with what had happened, it occurred to him, that warm water and oil were the likeliest things to correct and expell the poison. He imagined a large quantity of warm water might probably make the patient vomit, and in some measure help to discharge the caustic tincture. He was sure the water would at the same time mitigate its violence, by diluting it, and by precipitating the acrid gum from the spirit, by which it would necessarily be hindered from touching the membranes of the stomach and bowels in so many points, and from penetrating into their substance.

There was happily a large tea kettle of water on the fire, of which, being first qualified with a proper quantity of cold water, he immediately gave the patient a basin lukewarm, and repeated it as fast as possible, conjuring her to use her utmost resolution to swallow; which she certainly did in a most surprising manner. After the 3d basin, she vomited very freely: what was brought up smelt very strong of the camphor, and seemed to contain a good deal of the tincture, with the gum separated from the spirit. She still drank on, but complained of excessive burning and torture in her stomach, crying out continually she was burnt to death.

He had then recourse to oil between whiles, in the quantity of 2 or 3 oz. at a time; and drenched her plentifully sometimes with oil and sometimes with water. She vomited very copiously, and he repeated the oil and water interchangeably, till she had taken about 2 gallons of water and a flask of oil in a very short time.†

* The tincture was thus made; R. Gum. Euphorb. ʒij. Spt. Vin. rectif. ℥ij. Sol. add. Camph. ʒij. The camphor was ordered to weaken the caustic quality of the tincture, which being applied to a horse's leg without the camphor had made a blemish.

† Dr. Sydenham, being called to a man, who had taken mercur. sublim. corros. about an hour before the doctor saw him, the poison having affected his lips, &c. only ordered water to be taken in a large quantity, and thrown up copiously in glisters. But as the corrosive sublimate of mercury is to be considered as a poison, whose caustic acrimony consists in a saline principle, and water is the proper solvent, diluent, and vehicle of all saline substances, the propriety of Sydenham's ordering water alone is sufficiently apparent. Poisons of a saline nature being dissolved in the fluids of

Imagining the deleterious draught had not had sufficient time to bring on any violent inflammation or excoriation, or to make its way into the blood, vomiting and purging, with plenty of diluents and sheathing substances, seemed the likeliest means to save the patient, if any thing could be hoped to succeed in so perilous a situation. Dr. W. therefore ordered a mild but operative emetic of pulv. rad. ipecacuan. and a mixture with spermaceti and oil to be taken occasionally; still following up the patient with oil and water. He had reason to expect the emetic would also purge as well as vomit, and not only clear the stomach of the remains of the poisonous draught, but likewise carry downwards what portion of it might have passed through the pylorus by the contraction and agitation of the ventricle on the preceding vomiting. The apothecary demurred at the emetic, and objected the danger of its aggravating the effects of the poison by its stimulus and irritation. Though Dr. W. was not in the least convinced by the objection, yet, from an apprehension of the reflections which might probably be made after the tragical scene which seemed to be inevitable, he was staggered in his proceeding, and wished the objection had not been started. His brother, observing his uneasiness, asked him if she should send for Dr. Dymock. He gladly accepted this offer, as it rid him of his perplexity, and would give satisfaction to all concerned to have the best advice. In the mean time, he plied the patient with oil and water alternately, with which she vomited; but still grievously complained of a burning heat in the stomach and bowels. Her breath and all she vomited smelt very strong of the camphor. Her pulse was moderate, and not much quickened. He had now given her about a gallon more of water, and half a flask more of oil, when Dr. Dymock arrived. On informing him of the particulars of the case, he without hesitation ordered an emetic

the stomach and intestines, do not confine their ravages to these parts only, but are apt to enter the absorbent vessels, and insinuate themselves into the road of the circulation. Water is here a good antidote, as it dilutes such substances, washes them off the sensible membranes, destroys their acrimony, and readily passing through all sorts of canals, soon carries them out of the body. But the case is otherwise with gummy resinous poisons, such as euphorbium. These being indissoluble in water, are not so apt to enter the absorbent vessels, and pass into the blood, but by their acrimony shut up the orifices of those canals, and preclude a passage. Therefore oil here should be called in to the assistance of water. For the caustic resinous substance of euphorbium being precipitated or separated from the spirit, and formed into clots by the water, would still be apt to stick to the tender nervous membranes of the stomach and bowels, and by its intolerable acrimony cause violent vellications, inflammations, and gangrene. But the oil contributes greatly to prevent these fatal effects by sheathing the corrosive acrimony of the poison, preventing its adhesion to the delicate lining membranes of these first passages, and defending them from the violence of its attacks; while at the same time it promotes the discharge of their contents. These qualities of oil also render it very serviceable in other species of poison.—Orig.*

* Where metallic salts have been swallowed in poisonous quantities, the vegetable or mineral alkali should be added to the water and oil; or a solution of soap in water should be given.

of vin. ipecacuan. which was immediately given: but the patient complained more and more of an inward burning heat, which made it necessary to supply her with more water before the emetic operated. It had however in a short time the desired effect, and operated plentifully both by vomit and stool, especially the latter way. The stools, which continued to be discharged for near an hour, without any griping pain, very manifestly discovered both camphor and oil being mixed with them. The purging now began gradually to abate, and soon after the burning heat in the region of the stomach became more tolerable, and insensibly got better, the camphor being no longer perceptible in the breath or evacuations. Her drink now was water with the addition of a little milk.

The patient's spirits, which, by means of the great irritation and feverish tumult the caustic tincture had excited, kept up surprisingly, now began to fail her, and she was with difficulty got into bed: where, after complaining for a short time only of a soreness in the first passages, she lay sweating profusely for 4 hours in a very low desponding condition. A gentle opiate was exhibited, which took effect; and after a sound sleep of 5 hours, the patient waked very easy, took some of the spermaceti mixture, and had another sleep of 3 hours. She now found herself free from all her complaints; the previous slow fever, as well as the effects of the poisonous tincture, being entirely carried off. It is remarkable, that the patient found herself, for 4 days successively, in so happy a state of ease and tranquillity, as she had never before experienced, and to this day enjoys a perfect state of health.

The following observations, which this case naturally suggests, seem (says Dr. W.) to deserve attention.

1st. That in any similar accidents of swallowing corrosive poisonous substances, a quick and resolute administration of these simple bodies, water and oil, in a large quantity, seems to be the most effectual method of preventing any bad consequences, and far preferable to the numerous boasted antidotes which have been handed down to us. 2dly. That an emetic may be more safely and effectually administered, and its operation waited for, after the acrimony of the poison has been sheathed and blunted, and the coats of the stomach defended from its attacks by a liberal use of water and oil, than immediately after it is swallowed. 3dly. That as the slow fever and redundancy of milk, as well as the poison, were carried off by the copious discharge excited in the easy manner above-mentioned; might we not often hope for success in fevers occasioned by similar causes, plenitude and obstruction, from plentiful evacuations, brought on after the same manner, by simple, diluent, and sheathing medicines? 4thly. The camphor was undoubtedly of great service in curbing the destructive effects of the euphorbium, by blunting its acrimony, and soothing the nerves into an insensibility of irritation, and consequently an incapacity of spasmodic affections.

Dr. W. had tried the tinct. euphorb. cum camphorâ on a horse's leg several times, and found it not near so caustic as without the camphor. And it is well known how much camphor involves the spiculæ, corrects the acrimony, and mitigates the effects of cantharides, saccharum Saturni, and rough, mercurial, and antimonial preparations. 5thly, To water and oil therefore we may justly add camphor as a powerful corrector and expeller of poisons in general. This it probably effectuates, 1st, by blunting the acrimony; 2dly, by calming the nervous system, and securing it from spasmodic tumult and convulsion, which may be a consequence of its sheathing quality; 3dly, by its extreme subtilty and volatility, by which it freely penetrates the smallest recesses of the body, and powerfully promotes a diaphoresis. Some late instances of the effects of camphor in poisonous cases greatly confirm this account.

These 3 simple bodies then, water, oil, and camphor, challenge the first place among the antidotes hitherto discovered, both for internal and external use, and are much more to be depended on than any of the elaborate compositions calculated for this purpose by the ancients, as the Theriac. Androm. Mithridat. Conf. Paulin. &c.

LXIV. Of Artificial Cold produced at Petersburg. By Dr. Himsel. Translated from the French by James Parsons, M. D., F. R. S. p. 670.

On Decem. 14, 1759, they had at Petersburg the most excessive cold weather that ever was known, even to 205° of De Lisle's thermometer. At that time Professor Braun repeated Fahrenheit's experiments in order to produce excessive cold by means of spirit of nitre combined with snow. He saw with surprize the quicksilver fall considerably in the thermometer, and descend even to 470° at last: there the quicksilver remained fixed in the open air for the space of a quarter of an hour, and did not begin to rise till it was carried into a warm room. The immobility of the quicksilver made him conjecture that it might be frozen, or become a solid body. But as Mr. Braun had not broken the glasses, he could only at that time form a conjecture. Dec. 25 in the morning, between 9 and 10, De Lisle's thermometer was at the 199th degree of cold; and Mr. Braun, as well as Professor Æpinus, then repeated this experiment. As soon as the former had observed the quicksilver immoveable in the thermometer he broke the glass; and he found the quicksilver frozen, but not entirely; for in the middle of the glass ball there was a small portion yet remaining fluid. Mr. Æpinus's thermometer fell with extreme rapidity almost to the 500th degree, and in breaking the glass from below, he found the quicksilver contained in it absolutely frozen. Both these gentlemen found that the quicksilver, thus rendered solid, bore hammering and extension, like other metals; but being afterwards exposed to the open air, it soon recovered its former fluidity. Mr. Æpinus

went further in order to examine the quicksilver when it was made solid. He poured quicksilver into a glass tube as thick as one's finger, closed at bottom, but open at top. The quicksilver in this cylinder, which was about one inch and half long, froze in three quarters of a minute; and he observed that it became solid, perfectly resembling other metals, except iron: it continually contracted, and its surface, which was at first pretty high, soon sunk very low. This cylinder of frozen quicksilver sunk to the bottom of fluid quicksilver, in the same manner as is observed of other metals except iron. We know the contrary happens with regard to water frozen and other fluids, which extend as they become solid, and their ice swims in the fluid matter of which they were produced.

Dec. 26 in the morning, between 9 and 10, the cold became extremely sharp at 211°, and such as exceeded the greatest degree of artificial cold fixed by Fahrenheit; for 40° below zero, in Fahrenheit's thermometer, is equal to 210° of that of De Lisle.

Mr. Braun repeated this experiment again exactly with the same success with that of the day before. The counsellor and professor Lomonossow made the same experiment on the same day; and by means of aquafortis the cold came to 495 degrees. He then poured in spirit of common or sea salt, and the quicksilver fell down in the thermometer to 554 degrees; and in taking the thermometer from the mixture the quicksilver continued to fall in the open air to the 552d degree. He threw yet into the glass a little more snow, pouring on it some oil of vitriol, and suddenly the quicksilver fell to 1260 degrees. He then broke the ball, and found the mercury changed to a solid body. The quicksilver, which yet remained in the tube, was also become solid, and appeared like a loose silver wire attached to the ball, which was flexible every way. He gave the ball of quicksilver several blows with a turned ax, and it became flat like a half ruble, or English half crown; but receiving thereby some cracks, it dissolved in about 20 minutes. These experiments were made when the air was at about 208 degrees of cold.

It must be remarked that distilled quicksilver only was made use of in every experiment; nay in some the quicksilver was revivified from sublimation. There can therefore be no suspicion that what they used was impure or mixed with any heterogeneous matter. This appears to have happened to Mr. De Lisle de la Croyere, when he says, that in Siberia he found the quicksilver congealed in the barometer: and even his papers, which are in the academy, show that he made a mistake in his remarks; for according to them the mercury became solid as soon as it fell to about 195° or 200°: but the mercury which is pure does not congeal at that degree; for otherwise it would not be very extraordinary with us to see it take a solid form, because it is not rare to find the cold at this degree

here. We may believe that the quicksilver used by Mr. de la Croyere was impure, and therefore might sooner become an amalgama than mercury. Though in the experiment made by Mr. Lomonossow the quicksilver fell to 1260°, yet this philosopher says, that he could not sufficiently observe in his hurry whether the ball might not have received some crack, and the quicksilver thereby perhaps might have had liberty to fall the lower, which otherwise would not have happened; for the same thing happened to Mess. Braun, Zeicher, and Æpinus, that the balls of their thermometers were cracked and broken.

In making these experiments, it is to be observed, that it is necessary to use fuming spirit of nitre, or of such as is evaporated till the fumes become red; for the common aquafortis which is used had not the desired effect. Mr. Æpinus has found that this experiment is very easily and speedily made in the following manner. Take spirit of nitre, cooled as much as possible, and with it half fill a wine-glass, throwing in as much snow at the same time, and stirring it till it becomes of the consistence of pap: then you have almost in an instant the necessary degree for the congelation of the quicksilver. Now in reflecting on the procedure of other philosophers, especially of Mess. Muschenbroek and Reaumur, for producing artificial cold by the commixtion of snow with aquafortis, as the former has mentioned in his edition of the Experiments of the Academy of Florence, tom. i. p. 174, and Mr. Reaumur, in the Memoirs of the Academy of Sciences of Paris for the year 1734, it is astonishing how it happens that these learned men have not obtained, by a great deal, the degree of cold produced by the gentlemen of the academy of Petersburgh; for their manner of making the experiments does not seem to differ much from that of Mr. Braun, as to what relates to any essential circumstances, nor from the manner mentioned before, so as to hinder them from producing effects nearly equal. Indeed a certain degree of external cold appears absolutely necessary to the experiment. Mr. Æpinus, who made it the 28th of December, in a room where De Lisle's thermometer showed 122 degrees of cold, cooled the spirit of nitre in liquifying snow to 150 degrees, and the snow which they used came to the same degree; in making the mixture, the result was an augmentation of cold to 300 degrees. It must then happen, that they had obtained the surprizing degree necessary to congeal the mercury; which Mr. Zeicher also at length obtained; the degree of cold of the air being the 175th degree of De Lisle's thermometer, or the 30th of that of Fahrenheit.

LXV. Of a Complete Luxation of the Thigh Bone, in an Adult Person, by External Violence. By Mr. Charles White, Surgeon at Manchester. p. 676.

As Robert Hogg, (a farmer in Clyfton, about 4 miles from Manchester) a strong, robust, middle-aged man, was taking a load of wheat from off a horse

on the 20th of March 1759, his foot slipping, he fell backwards; his breech on the pavement, and the load of wheat on his belly and thighs. The servants carried him into the house, and laid him on a bed, where he remained in the most racking torture, when Mr. W. came to him, which was about 2 hours after the accident happened. He found his right buttock as large again as the other; the knee and foot of the same side turned inwards; and the thigh much shortened. On endeavouring to make the thigh perform its rotatory motion, there was not the least crackling to be heard. This convinced him that the head of the bone was thrown out of the acetabulum; and on examination he could distinctly feel it under the glutæi muscles: to which situation of it, and not to any bruise, he was now satisfied that the size of the buttock was owing.

He soon reduced it by the following easy and very simple method: some napkins being first wrapped round one of the posts at the foot of the bed, to prevent its galling him, he ordered the patient to be laid on his back, with one leg on each side the post, and then directed 3 or 4 assistants to pull at the dislocated limb, the post now placed to his groin being a fixed point to pull against. While they were making this extension, Mr. W. clapped his left hand on the head of the bone, to help it into its place; and at the same instant, with his right hand, turning the knee outwards, threw the bone into the socket with the greatest facility imaginable, but with such an uncommonly loud noise as greatly astonished all who were present.

He was perfectly easy from that moment; the enlargement of the buttock entirely subsided. In a fortnight he was able to move about without assistance; and in 2 months afterwards walked as far as Manchester, being then quite sound; and the limb that had been dislocated of the same length with the other.

Remarks. Both ancients and moderns have fallen into great errors in regard to the treatment of accidents that have happened to the hip joint. The ancients, who for want of frequent opportunities of dissecting bodies, were ignorant that the neck of the femur was often broken, always imagined it to be luxated. Their patients were therefore sometimes tormented (in hopes of a reduction) without any advantage; and this want of success made the surgeons at other times abandon their patients when they might have been relieved. The moderns have fallen into a contrary extreme, but attended with as bad consequences. Boerhaave in particular was of opinion that there never was a dislocation of the thigh bone by any external violence, but that the head of it was commonly broken off at its neck near the great trochanter. The opinion of so learned a man has had such weight with the generality of the profession, that it has been taken for granted that in these cases the neck of the bone was always broken; consequently the reduction was seldom attempted, and the unfortunate patients remained

cripples during the rest of their lives. But the point is now, Mr. W. thinks, cleared up beyond the possibility of a doubt.

In the 2d vol. of the Memoirs of the Royal Academy of Surgery at Paris, there are 2 cases related to show the resources of nature, where luxations of the thigh-bone have not been reduced. Here it appears (from examination after death), that in the first case the bone was thrown out upwards and outwards, the cotyloid cavity greatly diminished in size, and its figure changed from round to oval. The head of the femur was received into another cavity formed on the os ileum, under the glutæus minimus, which served it as a capsula to secure it within this preternatural cavity. This accident was occasioned by a fall when the patient was a child. She was afterwards able to walk about, though she continued a cripple to the time of her death, which happened at the age of 68. In the other case the bone was luxated downwards and inwards, and the head fixed on the foramen ovale.

There is a case too related in the Edinburgh Essays, vol. 2d, of a man at Worcester, who had the head of the bone thrown out of the acetabulum, and lodged in the groin. It was with some difficulty reduced, and the man suffered no other inconvenience than that of the leg's being about a quarter of an inch longer than the other.

To these Mr. W. adds, that about 30 years ago his father was sent for to a man who had luxated his thigh bone 3 or 4 days before. The head of it lay in the groin, which the surgeon who was first employed did not discover. However, it was immediately replaced, and the patient recovered the use of his limb in a very short time.

From what he had said, he would by no means have it concluded that the neck of the bone is not sometimes broken, or that it is not even oftener broken than luxated: but from the case which had fallen directly under his notice, joined to those which he had above recited, he thinks it must appear very clear, that it has been frequently luxated, and that in 2 different ways.

LXVI. Conjectures on an Inedited Parthian Coin. By the Rev. John Swinton, B.D., of Christ-church, Oxon. F.R.S. p. 680.

This is a small brass coin, nearly of the size of the middle brass ones, but in bad preservation.

LXVII. Of a Stony Concretion taken from the Colon of a Horse. By Mr. H. Baker, F.R.S. p. 694.

Mr. B. laid before the R. S. $\frac{1}{2}$ of a stony concretion, formed in the colon of a horse, which was sent to him from Norwich by Mr. W. Arderon, F.R.S.

This horse was 25 years old, had been often ill, and under the farrier's care; which made Mr. A. desire to have it opened when dead.

The whole stone, when first taken out, weighed $10\frac{1}{2}$ lb.: and Mr. A. caused it to be sawed in two, that a better judgment might be made of the manner of its formation. Several smaller concretions were also found in the colon; but they were much less solid, and more irregular than the large one. This ball was full 7 inches in diameter, and consisted of many laminæ or coats, which formed a number of concentric circles, around a nucleus in the centre, which seemed to be a small shiver of black flint. Fifteen or 16 of these coats were easily distinguishable, and some had been broken off: they varied something in colour, and were in general so stony that they would probably take a pretty good polish. The coats differed in thickness, according perhaps to the time they were in forming: for it should seem, if conjecture might be allowed, that each coat was formed in a longer or shorter time, according to its thickness; and that between the finishing of one coat, and the beginning of the next, there was some interval of time, and some suspension of that attractive power whereby, or of that component matter whereof, the several coats were respectively formed.

LXVIII. An Explanation of the Modes or Tones in the Ancient Grecian Music. By Sir Francis Haskins Eyles Stiles, Bart., F. R. S. p. 695.

This is another ineffectual, though voluminous attempt, after Meibomius and Wallis, to explain and restore the music of the ancients.

LXIX. An Inquiry into the Measure of the Roman Foot. By Matthew Raper, Esq., F. R. S. p. 774.

The ancient foot-rules now remaining; the representations of the foot in sculpture; and the measure of it, derived from the congius, differ so much among themselves, and from each other, as to be insufficient evidences separately: and the great disagreement of the foot from the congius, with the rest, has not hitherto been satisfactorily accounted for. The foot-rules found in old ruins at Rome are of various lengths; and the age of none of them being certainly known, no precise measure can be determined from them, otherwise than by taking a mean from such as appear to be most perfect. But though this may have been the foot in use at some time or other, yet as these rules are probably of different ages, both the greatest and least of them may have answered to the standards of their times. For though we have no account of any alteration ever made in the standard of the Roman foot, yet the wear of a standard measure by use, and the making new to replace the old ones, must probably create a difference; especially as the Romans had not those inducements to so precise an accuracy in these matters as the later discoveries in natural philosophy (particu-

larly the invention of the pendulum) have introduced among the moderns. Add to this, that the different state of Europe, which has for some ages been divided into many considerable kingdoms and sovereign states, independent of each other, equally civilized, and carrying on a more constant and regular mercantile commerce with each other, than was known to the Romans, must necessarily introduce more frequent inquiries into the weights and measures of the different states, and a more careful examination of the respective standards of each, than the Romans could have any occasion for: and use in these matters is the parent of accuracy. We can arrive at no greater certainty from the marbles than from the foot rules. These indeed do not differ so widely from each other as the rules; which seems to be the reason why most of the writers on this subject have given them the preference: but of the 4 that are extant, no 2 agree in the same measure; nor is the age of any one of them known: and as they were intended for representations only, and not for use, their accuracy may reasonably be doubted.

Festus, Frontinus, and Rhemnius Fannius say, the side of the quadrantal, which contained 8 congii, was a Roman foot. A standard congius of Vespasian is still in being, and has been measured by several learned men; but the foot derived from it exceeds those on the marbles, and the greatest number of the foot-rules so much, that Mr. Greaves could find no better way of accounting for so great a difference, than by supposing what Festus and Fannius say (for he does not quote Frontinus) to be a vulgar error: whereas the name of this standard shows its figure to have been a cube; which adds probability to their testimony, that its side was intended for the measure of the foot.

The measures of public roads in the itineraries can be of little use in this inquiry; for they omit fractions, and we do not know whether the distances of the towns are reckoned from the market-places or from the gates; but a difference of half a mile in 60 is equivalent to the tenth part of an inch in the foot: therefore no exact measure is to be expected from thence, even though the modern mensurations of Cassini, Riccioli, and others, were more unexceptionable than they really are. The distances between the ancient mile-stones are not liable to these objections; and if a sufficient number of such as stand nearest to Rome were carefully measured, their authority would be considerable. But it is not found that any are now standing within 30 miles of that city, nor that any of these have been measured, or even any in Italy; and provincial measures are not of equal authority.

There is still another method by which may be discovered the measure of the Roman foot; which is from the remains of the ancient buildings now standing at Rome: and though many have made use of some single parts of them for this purpose, yet no one has hitherto compared the measures of the principal parts

of any one of them with each other, which is the only way to discover the measure by which a building was constructed.

With this view therefore Mr. R. carefully examined the measures of the buildings contained in that treasury of ancient Roman architecture, entitled Les Edifices antiques de Rome, and published at Paris by Mons. Desgodetz in the year 1682. In order to this, it was first necessary to ascertain the proportion of the Paris foot (the measure used by this author) to some known English standard. The Paris foot is one 6th part of the toise in the Chatelet; which was renewed in the year 1668, and the new standard has continued in use ever since.

In the year 1742 the R. A. S. at Paris, at the request of the R. S. of London, sent over a measure of half the toise of the Chatelet; from which Mr. Graham determined the proportion of the Paris foot to that of London, to be as 1065.41$\frac{2}{3}$ to 1000. Mons. le Monnier, of the R. A. S., from the same originals, found their proportion as 864 to 811, or as 1065.351 to 1000. The difference is inconsiderable, and we may, without injustice to Mr. Graham's known skill and accuracy in these matters, suppose their true proportion to be as 1065.4 to 1000. Mr. Graham's measure of the London yard, together with that of half the toise of the Chatelet, are deposited in the archives of the R. S. at London; and of the R. A. S. at Paris; and whenever Mr. R. mentions the London foot, without specifying any particular standard of it, he would be understood to mean this measure.

In this inquiry we are to seek a common measure to the several parts of each building, that shall not differ very widely from some assumed magnitude of the Roman foot: and though we might take this assumption from any of the ancient foot-rules now remaining, yet the nearer it is taken to the truth, the better guide it will be to us, and the more it will facilitate our inquiry. Now as a mean measure derived from these rules will probably be nearer the truth than either the greatest or the least of them, so one that shall include such other remains of antiquity, as have hitherto been made use of to discover the measure of the Roman foot, will be still more unexceptionable, as the writers on this subject are not agreed, which of the different authorities is to be preferred. The representations of this foot in sculpture are 4 in number; one on the sepulchral monument of Cossutius, formerly in the Colotian gardens at Rome; another on that of Statilius, in the Belvedere; a 3d on that of M. Ebutius in the Villa Mattei; and the 4th on a marble, without incription, dug up of late years in the Via Aurelia, which being in the possession of the Marquis Capponi, is called by P. Revillas the Capponian foot. Most of the early writers on this subject have expressed their measure of the Roman foot by a diagram; and Snellius observing that the paper contracted in drying, after the impression was taken off, endeavoured to make a proper allowance for it. But Greaves, finding the measures of these figures to differ in different copies of the same impression, took another method;

and seems to have been the first that compared the original figures on the monuments of Cossutius and Statilius with a modern standard. This he did with such care and diligence, that his measures deserve a particular examination.

The London foot, which he used on this occasion, was taken from the iron standard of 3 feet in the Guild-hall, London; which having been long since lost or destroyed, we have nothing left to discover its true magnitude but the measures others have taken of it, and those which have since been taken of such magnitudes as Greaves had compared with his copy of it.

Snellius, from a measure sent him of this iron standard, determined the proportion of the Rhynland to the London foot, as 1000 to 968. The Rhynland foot, according to Picard, contains 696 such parts as the Paris foot contains 720: whence the proportion of the latter to this measure from the iron standard, is as 1065.4 to 997 nearly. Eisenschmid found the Rhynland foot to contain 1391.3 such parts as the Paris foot contains 1440; which gives 1065.4 to less than $996\frac{1}{7}$, for the proportion of the Paris foot to that of the iron standard. Huyghens makes the Paris to the Rhynland foot as 144 to 139; whence the proportion of the former to Snellius's London foot, will be nearly as 1065.4 to $995\frac{1}{2}$. But there is reason to believe that Huyghen's measure of the Rhynland foot was too small. By these comparisons it appears that Snellius's measure of the London foot, from this iron standard, was at least 3 parts in 1000 shorter than Graham's London foot.

Our countryman Norwood, in 1635, measured the distance between London and York, in order to determine the length of a degree on the meridian; which he found to contain 367196 London feet of this iron standard. The French found the measure of a degree in the latitude 66° 20′, to be 57438 toises, and at the equator 56783. Hence the measure of a degree in 52° 44′ (the middle latitude between London and York) will be found to be 57276 toises, or 343656 Paris feet. These numbers give the proportion of the Paris foot to that of the iron standard, as 1065.4 to 997.1 —, wanting somewhat less than 3 parts in 1000 of Graham's London foot.

Picard's paper De Mensuris, and another on the same subject by Auzout, printed with it, contain some measures which Greaves had before compared with his London foot. Both these papers were written after the renewal of the standard of the Chatelet in 1668. The former is so full of inacuracies and mistakes, that little use can be made of it; but Auzout's measures appear to be accurate; and as he seems to have taken his Paris foot from the toise in the Chatelet for this purpose, it was probably a correct measure of that standard.

Such of his measures as answer to Greaves's, are here reduced to thousandth parts of the London foot, reckoning his Paris foot to contain 1065.4 such parts.

	Greaves.	Auzout.	Diff.	Diff. per foot.
The Statilian foot	972	969.96	— 2.04	2.1
The braccio of Florence ...	1913	1908.84	— 4.16	2.18
The braccio of Siena	1974	1973.21	— 0.79	0.4
Pætus's palm	732	731.35	— 0.65	0.89

All these differences fall the same way, and show that Greaves's London foot bore a less proportion to Auzout's Paris foot than that of 1000 to 1065.4.

After thus examining many other measures of different things, as taken by the modern philosophers, Mr. R. thence concludes: All that can be determined from such uncertain and discordant data, as here collected, is a measure that shall probably be neither the greatest nor the least magnitude of the Roman foot. And for this he takes a mean from all the measures above recited, which is nearly 968 thousandth parts of the London foot.

Before entering on the examination of the ancient buildings it may be proper to say something concerning the nature of the evidence to be expected from them. All buildings are planned and executed by some measure of the country where they are built. At Rome this measure was the foot, which was divided by the workmen into 4 palms, and each palm into 4 digits.*

If the Roman buildings were correctly executed, and we had the true dimensions of their several parts in any known measure, some divisors consisting of Roman feet, and parts of those feet, applied to these measures, must, in the same building, give the same quotient to all; and this quotient will be the measure of the foot, by which that building was constructed, in parts of the known measure. Therefore, where a range of simple divisors, applied to the principal parts of any building, give as nearly the same quotient as can be expected from the common inaccuracies of workmanship, we may reasonably conclude that these divisors were the architects' numbers; and the foot derived from them, that by which the building was constructed.

As an architect cannot be supposed to be limited to a few digits in the extent of the front, or of the depth of large buildings, it is probable such measures consisted of whole feet. These and the diameters of circular buildings Mr. R. calls prime measures. In all large prime measures, the preference is to be given to a round number for the divisor; as it is more probable a building should be designed for 100 feet in front than for 99 to 101: and because the passus was 5 feet, Mr. R. reckons any multiple of 5 a round number.

* Vitruvius, lib. 3, c. 1. Frontinus de Agrorum Qualit. Both these authors are technical writers, and give this as the division used by workmen; and the ancient foot-rules are so divided. They both mention the duodecimal division, which seems to have been used by the vulgar; for the Romans divided every integer into 12 unicæ.—Orig.

The diameters of columns are of less authority than any other horizonta measures; not only on account of the difficulty of measuring them correctly, but because errors of workmanship, to which they are more liable than square measures, more sensibly affect the magnitude of the foot in small measures than in large ones.

Uprights, of any considerable height, are of less authority than horizontal measures, from the difficulty of taking them correctly; and being designed by modules, few of them answer well to the foot measure. But here we must except such shafts of columns as are of one block of marble; which seem to be as good authority as any part of a building: for the necessity of making them all exactly of the same length, must produce accuracy; and the doing this was no difficult piece of workmanship. Being likewise commonly (if not always) wrought at the quarry, to save expence in the carriage, they were probably bespoke to some simple measure; and we shall find all such shafts answer to some number of whole palms.

In this ingenious way then Mr. R. took the measures of the principal parts of a great number of the ancient Roman buildings, and divided them by the most probable divisors, for the near length of the foot. These quotients fall mostly between the numbers 963 and 972, that is, of such parts as the London foot contains 1000. And at length Mr. R. concludes thus:

It appears from the measures of these buildings, that the Roman foot before the reign of Titus exceeded 970 parts in 1000 of the London foot, and in the reigns of Severus and Dioclesian fell short of 965. Whether this difference proceeded from any alteration in the standard, or from a false measure of it being got into common use, either before the reign of Titus or after, is uncertain. We have no account of any alteration made by law in the Roman standards after the Plebiscitum Silianum, quoted by Festus; but as great a difference as this might arise from their having been lost or destroyed.

They were kept in the capitol; and Rigaltius, from a passage in Hyginus, observes that the standard of the foot was deposited in the temple of Juno Moneta. Now the capitol was burnt no less than 3 times; first in the civil war of Sylla, then again when Sabinus was besieged in it by the troops of Vitellius; and the 3d time in that dreadful conflagration which happened in the reign of Titus. Whether the standards were destroyed in the first of these fires is uncertain; but they could hardly escape the fury and confusion of the 2d, when, according to Pliny, the temple of Juno Moneta seems to have been burnt to the ground. And if we may credit Xiphilin (whose account of the 3d is in some measure confirmed by Spartian), not only the temple of Jupiter Capitolinus, but those adjoining to it, were burnt down in the last.

Vespasian rebuilt the capitol after the 2d conflagration, and restored the

ancient records from copies of them that were got abroad, and probably the standards at the same time. The congius was restored by weight, according to the Plebiscitum Silianum, as the inscription on it testifies. The quadrantal was too cumbersome a vessel for common use, to which the congius (like our gallon) was well adapted; so that Mr. R. does not see what other purpose it could serve, but to adjust the congius to its capacity, and the foot to its side: but here we see the congius adjusted by weight, and it is not very likely that a new quadrantal should be made for no other end but to adjust the foot by, when so many copies of the old standard were extant. Therefore it is not improbable that the standard of the foot was at this time restored, without any regard to its relation to the quadrantal.

But as to the difference between the foot derived from the congius, and that found from other authorities, Mr. R. further observes, that the correct adjustment of weights to measures is a very difficult matter, even in this age and in this kingdom, where workmanship is arrived at a high pitch of accuracy. And what errors rude workmanship is liable to, sufficiently appears from the weights Pætus and Villalpandus have given of the congius. Therefore he sees no reason to reject the testimonies of those authors, who say that the cubic foot contained a quadrantal of wine; and as little to believe that these two standards were ever truly adjusted to each other.

But had the original standard of the Roman foot been truly adjusted to the quadrantal, and continued invariable from the time of its first establishment, yet a false measure of it might at one time or other have got into common use at Rome, as well as a false measure of the French foot did at Paris; where in the year 1668 the mason's foot was found to exceed the foot of the Chatelet by $\frac{5}{72}$ of a Paris inch, which is above $\frac{1}{14}$ of a London inch: and the unaccountable negligence which appears in the Roman coinage, gives sufficient ground to suspect that they were not more accurate in their measures.

LXX. Description of a Metalline Thermometer. By Keane Fitzgerald, Esq. F. R. S. p. 823.

Mr. Boyle, the great promoter of experimental philosophy, made a thermometer on the principle of air, which to a certain degree of heat or cold, answered very minutely. Alcohol, or spirit of wine, has been more generally used; but has been found to lose in time much of its expanding quality; and also to be frozen by an intense degree of cold. Mercury, as not deemed subject to these inconveniencies, has therefore been allowed the most proper for the purpose.

Mr. Fahrenheit has since improved the mercurial thermometer to a great degree, and brought it to as much perfection as perhaps it will bear. He has remarked, that when the barometer shows a greater degree of pressure of the

atmosphere; the same liquor will receive 8 or 9° of heat more than when the barometer is at the lowest. But whether this proceeds entirely from the liquor's receiving a greater degree of heat by the pressure of the atmosphere may be a matter of some doubt; as it seems, by comparing the mercurial with other thermometers, to be affected in some measure by the pressure of the atmosphere in all degrees of heat and cold.

The making of metalline thermometers has been hinted at by many; particularly by Mr. Smeaton in his curious observations on the expansion of metals, who recommends zink or spelter as most capable of expansion, and fittest for the purpose. Mr. F. has endeavoured to make one on this principle, which he lays before the Society, with a description of its construction, and an account of the few observations he had been able to make on it.

The structure of this instrument is very intricate and delicate, and in use is hardly practicable. Mr. F. acknowledges, that since this instrument has been made, he found in the Phil. Trans., that Dr. Mortimer had in 1735 given the R. S. a description and drawing of an instrument he invented for the purpose; and that Mr. Johnson had also given a drawing of another, invented by Mr. Fothringham.

Mr. F. finds, by comparing this instrument with a Fahrenheit's and a spirit thermometer, that it keeps at a medium between both; not rising at first so quick as the mercury, and somewhat quicker than the spirit. On placing them together in the sun, when its heat became intense, it rose at last faster than the mercury, and not so fast as the spirit; and it continued to rise for some time after the others became stationary.

He tried the expansion of a few metal bars, from artificial freezing, with pounded ice, and water that it dissolved into; on which was poured half an ounce of spirit of tartar, in which Fahrenheit's thermometer descended to within one degree only of the freezing point: to boiling water, in which it rose to 211°, though the water did but scarcely boil, for want of a sufficient number of lamps. The barometer stood at 30 inches, and the natural heat of the weather at 60° of Fahrenheit.

	Divisions.
A bar of spelter 2 feet long, marked by the minute index . . .	1570
Spelter 18 parts, and copper 2 parts	1150
Brass	1120
Iron	785
Steel	695

Note. Each division marks the 73840th part of an inch expansion per foot.

Mercury cannot be useful in trying any degrees of heat above what makes it boil; and it appears by Dr. Hinsell's account of the experiments lately made at

Petersburg, that it may be frozen by extreme cold; which makes it unfit for ascertaining the extreme degrees of either.

LXXI. Of a Bird supposed to be bred between a Turkey and Pheasant. By Mr. George Edwards, F. R. S.* p. 833.

Mr. E. received this bird from Henry Seymer, Esq. of Handford, near Blandford, Dorsetshire, with his letter, dated April 9, 1760: wherein he says.—"I have taken the first safe opportunity of sending the two birds. The large one I verily believe is an accidental cross, as we sportsmen term it, between a pheasant and turkey. When the bird was just killed, the skin round the eyes was of a pale red-lead colour, and the eyes like a turkey's. As I live near the wood where they were found, I took great pains to get another of them, but was never so lucky as to find one. There were 3 at first, all of which I believe are now destroyed."

The bird is of a middle size between a pheasant and a turkey-hen, and shaped pretty much like a turkey: the bill, legs, and feet, are black, and shaped like a turkey's; it has a broad space of bare skin round the eyes, which, when the bird was living, was of a pale red-lead colour; the eyes like those of a turkey; the head and half the neck is covered with very short feathers, of a whitish clay colour, with transverse dusky bars, though the throat and fore part of the neck are wholly of a light clay colour. These short feathers occupy the head and that part of the neck which is naturally void of feathers in turkeys. On the lower part of the neck, the breast, and belly, the feathers are much longer, and of a black colour, with a purple and changeable gloss. The thighs and legs on their fore-part a little below the knees are covered with feathers transversely barred with clay colour and black. The back, coverted feathers of the wings and tail, are of a mixed colour, in very fine transverse lines of brown and black, though some of the coverts of the wings and tail have larger transverse bars of the abovesaid colours; the greater quills are dusky or black, powdered with small clay-coloured spots; the inner coverts of the wings have white tips, which hide their bottoms, that are dusky. He counted 16 feathers in the tail, the outer ones shorter by 2 inches than the middlemost; their colour is composed of brown and black, mixed transversely, like those on the back, though they are more dusky toward their tips; the very tips being of a bright brown: the outer borders of the side feathers of the tail are of a bay colour; the covert feathers beneath the tail are of an orange colour, crossed with black; about the vent the feathers are white with dusky spots. The whole upper side nearly resembles

* This bird is mentioned by most modern ornitholigists; who appear to acquiesce in the account and figure of Edwards, and consider the bird as a hybrid production between the common pheasant and turkey,

that of a hen pheasant, but darker coloured. The feathers of the body are all double; that is, two distinct feathers proceeding from one stem; the outer large, and of a firm texture; the inner smaller, and altogether downy.

Whether this bird be produced from a turkey-hen and a cock-pheasant, or from a turkey-cock and hen-pheasant, no one knows. Mr. E. thinks it rather from a hen-turkey and cock-pheasant; because their disparity in size is not near so great, as between the turkey-cock and hen-pheasant. Though the supposition that this bird is from an egg laid by a hen-turkey trodden by a cock-pheasant, is attended with a difficulty not easily reconciled; for it is not probable that a hen-turkey, a domestic fowl, should betake herself to the woods, and bring up her brood wild and unobserved; which is contrary to the habit of turkeys in our country, where they are not originally natives. Why these mixed generations so rarely happen, is because nature has fixed the inclination of every distinct species to the contrary sex of its own identical species, from which, in a wild and natural state, it will hardly ever stray. The reason of the mixtures that we meet with, contrary to the ordinary course of generation, may proceed from some hindrance of the male's meeting with his proper female, or female with male, at the seasons when they are by nature appointed to propagate their species, which rarely happens; for in a wild state of nature most animals are numerous, and at their breeding seasons easily meet with males or females of their own species. Disappointments of what they naturally seek, and accidental meetings of different species, near of kin to each other, cause these unnatural conjunctions, which produce uncommon mixed species of animals. He believes that two species widely different from each other, as water-fowl and land-birds, &c. cannot possibly conjoin, so as to produce a living mixed offspring. He had been informed, and believed it may be true, that a mixed species has been produced between our common poultry and partridges that harbour near farm-yards.

LXXII. *On a late Discovery of Asbestos in France. By Mr. Turberville Needham, F.R.S.* p. 837.

A singular discovery has accidentally been made in one of the French provinces, of the nature of asbestos, or amianthus. The proprietor of a certain forge, on taking down his furnaces to repair them, found a great quantity of this substance at the bottom. It answered effectually all the common uses of the native amianthus, either manufactured into linen or paper. In short, on a progress in this inquiry, he finds that both this which he obtained from the forge, and the native asbestos, is nothing more as he terms it, than calcined iron deprived of the phlogistic; and that by uniting the phlogistic, either with this or the fossile amianthus, he can restore it at any time to its primitive state of iron.

Does not this, with the discovery of lava, pumice-stones, iron in a perfect state, and many other traces of fire observed in most of the mountains, particularly in all the great chains, and remarkably in all those under the equator, which are the highest on the globe, seem to indicate, that the dry land, with all its eminencies, was originally raised out of the waters, by the force of subterraneous fire?

LXXIII. On the Hot Baths of Vinadio, in the Province of Coni in Piedmont. By Dr. Joseph Bruni, F. R. S. p. 839.

The warm baths, which have been so serviceable to the Chevalier Ossorio, run through the rocks, near the village Vinadio, in the province of Coni. The water is very clear, and so warm, one cannot bear the hand in it: the contents are sulphur diffused through it, and some salt almost like common salt. By evaporation, you get 5, sometimes 6 gr. from 1 lb. of water. Dr. B. sent a small quantity of the salt, gathered from the stones by which the water runs, before the rising of the sun, for in the day-time it is not found upon them, except in winter. Where these waters run, they deposit oily particles, which by degrees join together, and form a soft, spongeous, greenish-yellow substance, an inch almost in thickness, which is called muffa: this, when dry, is wrinkled, takes fire, crackles, and gives the smell of brimstone, and when entirely burnt, leaves a black ash behind. A piece of silver immersed in these waters in a few minutes became black. Their taste is neither salt nor acid, but disagreeable. The muffa left for 2 days in common water, swells 6 times thicker than it was, stinks, and throws up oily particles on the surface of the water. The salt does not ferment with acids. If you dissolve it in common water, and mix it with syrup of violets, it gives some appearance of a green colour: the same water poured on a solution of silver, it soon throws down a white sediment. Some say these waters contain nitre, and particles of other bodies; but this has not been demonstrated.

The disorder of the Chevalier Ossorio was, that he had lost the feeling of his fingers, had a weakness in his hands and legs, so that sometimes he could not walk in a straight line, but tottered from side to side. He could not extend his toes, and the soles of his feet seemed as if hard strings were drawn across them. He tried many medicines to no purpose: but is now perfectly free from the above complaints by the use of these warm baths. He bathed in the waters 40 times, when the stomach was empty, in a morning; and staid in them at first $\frac{1}{2}$ an hour, but lengthened the time gradually at last to a full hour. After each bathing he was dried with cloths, and put into a warm bed, where a plentiful sweat came on for about $\frac{1}{2}$ an hour; during which, the pulse beat like that in a high fever, but became quieter as the sweating abated. When the sweating was almost over, and the pulse quite regular, he was dried again with cloths, his

shirt was changed, and sitting up in bed, he was refreshed with a glass of strong wine and a piece of bread. After this he rose and dressed, and took a gentle walk.

Dr. B. observed, that no patient who came thither received the least prejudice by the waters, though all did not receive a like benefit for their respective disorders. But one gentleman in particular, who came paralytic in the whole inferior extremities of his body, occasioned by hard drinking, was so far relieved as to walk without help. Others were cured of disorders in the skin, and relieved in rheumatic and many other complaints.

LXXIV. A Specimen of the Labour of a Kind of Bees, which lay up their Young in Cases of Leaves, which they Bury in Rotten Wood. By Sir Fran. Eyles Styles, Bart., F. R. S.,* p. 844.

Mr. S. makes no doubt but these bees are the same as described in the Phil. Trans. by Sir Edm. King, Mr. F. Willoughby, and Dr. Lister. The specimen was found in some park pales near Windsor in the latter end of the summer. One of the bees hatched, and crawled from its case, on Whitsunday, and by an empty case Mr. S. saw, that was broken open much in the same manner, he imagined another had hatched, and flown away a little before. The remainder he presumed would not come to life, as he observed that some foreign insect had made its way into some of the cases; and others might have been chilled in the winter by the fracture of the wood in which they were inclosed.

LXXV. On a Case of a Luxated Thigh Bone reduced. By Mr. Charles Young, Surgeon at Plymouth. p. 846.

As John Down, a middle sized man, aged about 40, was, on the 21st August 1759, harnessing his master's horses, they suddenly took fright and ran away with the chaise. He had his back towards the chaise, the wheel of which as it rolled very swiftly along struck him on the upper and hinder part of the right thigh. He fell to the ground and was unable to rise again, and complained immediately of a violent pain in his right hip. Mr. Y. came to him soon after the accident, and caused him to be put to bed; when on examination, he found his only complaint was the violent pain about the articulation of the femur with the ischium, which was increased by any, even the least, motion of the limb. The toe was turned in toward the heel of the left foot, and the heel of course out-

* The bees of this kind are generally referred by authors to the species called by Linneus *apis centuncularis*; but from more accurate modern observations, and more particularly from those of the ingenious Mr. Kirby, in his work entitled Monographia Apum Angliæ, it appears that 3 or 4 species have been usually confounded together under one common name. It is probable however that the species here intended is the *apis willugbiella* of Mr. Kirby.

ward; and the whole limb, from the head of the femur to the toes end, distorted in proportion.

Mr. Y. thought it was very evident at first sight, that there must be either a fracture of the femur, or a dislocation of its articulation with the ischium. The former he thinks would have been easily discoverable. But as by laying his hand on the great trochanter, while an assistant turned the foot inward and outward alternately, he could perceive that the motion of the great trochanter corresponded exactly to the motion of the lowest end of the femur, he concluded that had there been a fracture, it must have been between the great trochanter and the head of the bone. And had this been the case, he expected to have been able to discover it, by the grating that is always to be felt, when the 2 broken ends of a bone are moved against each other. But no such thing being perceivable, and yet the limb so much distorted, and the pain so violent, and confined to the parts about the joint, he took it for granted, and pronounced the case to be a dislocation of the femur; and consequently endeavoured to reduce it by the usual method of extension. To this end, 2 men extended the limb, by pulling on napkins tied round the ancle, while others counteracted them, by pulling at a sheet passed between his legs, and secured at the bed's head, turning the foot outward as they made the extension. This gave him great pain; but the limb soon became in every respect parallel to the other. It appeared as long, and on laying it down on the bed, the great toes and heels of both feet lay exactly in the same position; and the only difference he could perceive in the 2 limbs was, that there was a little flatness about the hip of the right side. In short, the difference between the 2 limbs was so little, that he began to think he had been mistaken in his opinion of a dislocation (for it was evident there was no reduction made by the extension, for that could not but have been perceived both by the patient and himself) and that the distortion of the limb was owing to nothing else than an involuntary contraction of the muscles, occasioned by the violence of the blow. He therefore bled him, confined him to lie on his back, and charged him to move as little as possible, imagining that rest would be his most effectual remedy. He continued in much pain for some days; but by degrees grew tolerably easy, except when the limb was moved; and at about 12 days after the accident, he could suffer the limb to be lifted to and fro gently, with little or no pain at all. Notwithstanding which, he could no more lift it of itself than at first, when it was much more painful. This embarrassed Mr. Y. good deal. He was convinced there was no fracture of the limb in any part; and thought from the circumstances above related there could hardly be a dislocation. He therefore desired the opinion of 2 other surgeons, who on seeing the position of the limb, and inquiring into all the circumstances, which did and had attended it, agreed with him in opinion, that it was no fracture, and

that it was equally unlikely under these circumstances there should be any dislocation. For the right leg, when placed by the side of the other leg, was exactly parallel to it, and continued so, unless the patient, either when asleep or at any other time, moved his body, so as to drag his leg: in that case the toe was always found inclining inwards and the heel outward; but never so but that it might be replaced, without the least difficulty or pain to him, but just so as one might have done by a limb that was paralytic. They therefore recommended rest, hoping that further rest and time would recover the perfect ease and strength of the limb. But some days passing without any alteration, Mr. Y. gave him a strong purgative, and repeated it every other or every 3d day, for several times, in order to reduce the muscles, that he might the more plainly feel any thing through them. For though he was in point of height but middle sized, he was pretty fleshy, and the glutæi muscles consequently too thick to suffer any thing to be felt with any degree of distinctness through them. This answered his expectation fully; for by repeating the purgative often and at short intervals, his living sparingly, and being confined to his bed, he became much emaciated, insomuch that the head of the femur was plainly felt through the muscles, dislocated backward, and lying in the space between the os ischium and os sacrum.

Of this he acquainted the gentlemen who had examined it before, and desired them to examine it a 2d time, which they did Sept. 25, and were immediately convinced that the os femoris was dislocated, and that it was the head of the bone which was felt through the glutæi muscles, in the space between the ischium and sacrum.

To the head of the bone's lying in this place, it was probably owing that Mr. Y. was at first deceived; since its being there allowed a greater latitude of motion, than could possibly have been the case any where else; which may serve to account for the parallelism of the limb to that of the other, notwithstanding a luxation. But on turning the foot inward and outward by turns, while an assistant laid his hand on the head of the bone, a grating was perceived, both by them and the patient. This somewhat surprised them at first; but as this grating was never to be perceived without pressing pretty hardly on the head of the bone at the time the limb was turned round, and as the head of the bone was plainly felt to turn round, whenever the thigh had that motion given to it, they concluded it could be nothing but the side of the head of the femur against the edge of the ischium.

Convinced of this, they determined to make an extension: and to that end, brought him to the foot of the bed, and placed him on his back, with the bed's post between his thighs, which was wrapped round with cloths, to prevent its galling him. A napkin was tied round his leg at the ancle, which two assistants pulled by, while a 3d turned the knee outward, and he had his hand on the

head of the bone, pressing it downward. As soon as the extension began Mr. Y. perceived plainly the bone sink under his hand, which he had hardly time to say before it gave a snap, which was felt by the patient, and heard by them all, and the bone was reduced.

In about 6 or 7 days he was easy, and able to walk over the room with crutches, and bear a considerable weight of his body on the right leg: and from this time he recovered strength very fast, and had long been as strong in that leg and thigh as in the other, without any even the least difference in length, or any other respect.

Mr. Y. was induced to send to Dr. Huxham the above case of John Down, because it is asserted by some surgeons, and among those of the greatest character too, that a luxation of the head of the femur is little less than impossible; and that what is generally taken for a luxation of this joint, is a fracture of the bone at its neck.

A fracture of the neck of the bone probably happens more often than a dislocation. But the above case has proved that it may happen, and that without any extraordinary violence, provided the force is aptly applied. Indeed any force applied in the direction of the thigh downward can hardly have any tendency to dislocate it at all: and any force from below upward will be sustained by the head of the femur bearing against the upper part of the acetabulum, till the neck, the weakest part, gives way.

But though it may not be possible that the femur should be luxated by any force applied in a direction parallel to that of the thigh, in an erect posture of the body, it is not equally impossible it should be dislocated by a force applied in a contrary direction. For in the above case the blow was received on the upper and hinder part of the thigh, in a direction forward, from the wheel of the chaise, which must necessarily have a tendency to drive it round forward, and consequently cant the head of the femur out of the acetabulum backwards, where it is less deep than it is either above or below. On the fore part of the acetabulum it is yet more shallow, and therefore less force is required to dislocate it that way, and more especially as there is on that side less strength of muscles to resist.

It was probably a fortunate circumstance for this man, that Mr. Y. thought himself mistaken in his first opinion of its being a dislocation; for had that been clearly the case, he would have used every method, and every assistance to be had, to have reduced it immediately; and most likely, while the muscles remained in their full strength, and contracting involuntarily, and that violently too, as they will sometimes do on attempting an extension of them, and under which circumstance, the muscular fibres oftentimes rather break than give way, should have failed of being able to reduce it; and in that case the man must have re-

mained a cripple as long as he lived: whereas now, though 25 days from the time of the accident, the muscles were so much weakened by his being confined to his bed, and wasted by his frequently repeated purges, that they very easily gave way, and the reduction was effected with as little difficulty as ever he saw in a dislocation of the humerus.

Might not the giving strong purgatives, and frequently repeating them, so as to render the muscles of strong muscular subjects more lax and weak, be a means of reducing luxations of the humerus, which are not reducible by any method of extension, as is often found to be the case?

LXXVI. On a Samnite Etruscan Coin, never before fully explained. By the Rev. John Swinton, B. D., of Christ-Church, Oxon. F. R. S. p. 853.

This, according to Mr. S. is a silver Etruscan coin, of the size of the consular denarii, similar to some published by Oliviari and others, and struck about the year of Rome 663.

*LXXVII. On the next approaching Transit of Venus over the Sun. By Roger Joseph Boscovich.** p. 865.

Such prognostications of the approaching transit are useless now. We shall soon have occasion to contemplate the observations made on the real appearances of it in the year ensuing.

* This celebrated astronomer and mathematician was born at Ragusa in Dalmatia in 1711, and died at Milan in 1787: consequently at 76 years of age. He entered the order of the Jesuits at Rome in 1725: in whose college there he was appointed professor of mathematics in 1740; where he soon distinguished himself by a number of excellent astronomical and mathematical dissertations. In 1750, assisted hy his brother Jesuit F. Maire, he conducted the measurement of a degree in the ecclesiastical state. And through his influence with the ministers of other courts, it is said he procured to be made similar measurements, by Liesganig in Austria and Hungary, by Beccaria in Piedmont, and even by Mason and Dixon in America. He likewise effected the restoration of the celebrated gnomon at Florence. And in 1759 he published at Vienna his Philosophiæ Naturalis Theoria. From Vienna he was called to Milan, where he taught astronomy and optics during 3 years. And he may be considered as the founder of the observatory of the Jesuits in that city; from which afterwards arose the Imperial observatory of Brera.

On the dissolution of the order of Jesuits in 1773, B. was invited to Paris, where he was naturalized, and appointed a director of the optical instruments of the marine. But feeling disgusted here at some envious attacks from the literati, he quitted France in 1783, and repaired once more to Italy. B. was an elegant general scholar, and even a poet, as appears by his excellent poem on the eclipses of the luminaries. The consideration which he enjoyed in several European courts implicated him also in politics. Hence the Republic of Lucca successfully settled through him a most important state affair.

In 1786 he published at Bassano a collection of all his works, in 4 volumes 4to. entitled Opera ad Opticam et Astronomiam Pertinentia: also the Nautical Astronomy in a 5th vol. was published in 1787. B. wrote also Elements of Mathematics and Physics, and a treatise on Dioptrical Telescopes. In the same year 1786 he went to Milan; where, at the desire of the emperor Joseph, he undertook the superintendance of the measurement of a degree, and the formation of a new map of Lombardy. But a stroke of the palsy put a period to all his useful labours the beginning of the year following.

LXXVIII. A Proposal for discovering the Annual Parallax of Sirius. By the Rev. Nevil Maskelyne, A. M., F. R. S.* p. 889.

The Royal Society had resolved to send persons of ability to proper places, to observe the approaching passage of Venus over the sun, the 6th of June in the year following; (as first proposed by Dr. Halley 44 years ago, as a proper means of determining the sun's parallax to a great degree of exactness, Dr. M. recommends a very important object in astronomy, which he apprehends may be cleared up at the same time, by the astronomers sent to one of those places which will probably be judged convenient for the observation of Venus's transit, viz. the island of St. Helena.†

This object is the determination of the annual parallax of the Orbis Magnus; the finding out of which, from observation, would be the fullest and most direct proof of the Copernican system, as the want of this proof hitherto has been the strongest argument made use of by those who have withheld their assent to an hypothesis, which so fully satisfies all the other phenomena.

No one indeed will now venture to assert that, even if no annual parallax could be found, after the greatest exertion of human art and industry, the Copernican system was not therefore true; since the quantity of this parallax may be so small as to escape the reach of our sight, though assisted to the utmost. But though the defect of it would be no just argument against the Copernican system, yet the actual demonstration of it, from observation, would be a direct and convincing proof of the truth of that system. It remains then to be considered, what hope there is now left, after astronomy has been brought to such a great degree of perfection, of being able to find out an annual parallax in any of the fixed stars.

Mr. M. is sensible he may here seem to be presumptuous, in venturing to treat on this subject, after the many accurate observations made by Dr. Bradley, with an instrument constructed for this very purpose. He would just beg leave to take notice, that the stars which this astronomer observed, were such only as lay within a few degrees of his zenith; and though his observations do not seem to show a sensible parallax in any of them, yet we cannot thence absolutely conclude, that among the great number of visible stars, there are none in which it may be perceptible, till they have all of them, especially those of the greatest lustre, been observed in proper places near the zenith, with the like care and accuracy which he has used: for, as Dr. Bradley has himself remarked, where any stars are remote from the zenith, the uncertainties of refraction, and the irre-

* The present astronomer royal, 1807.

† Mr. M. was himself the person sent to St. Helena, to make the observations, as will appear hereafter in due place.

gular motions of the air, become so great, as to take away from us all hopes of observing them to an equal degree of exactness.

The particular star which Mr. M. proposes should be carefully observed, with a view of discovering its annual parallax, if sensible, is Sirius, the brightest of all the stars in the firmament, and which is therefore probably the nearest to us of them all. With us, this star passes the meridian, at the altitude of 22°, where the refractions are too irregular to admit of our discovering a very minute quantity, by observation: but, at the island of St. Helena, Sirius passes only half a degree south of the zenith; and, on this account, he has for some time considered that as the most proper place to make observations at, for this purpose.

Mr. M. mentions a particular argument, which had for 2 years induced him to think it probable, that the annual parallax of Sirius is not so small, as to elude the nice discernment of our modern astronomers; and he thinks it affords a sufficient presumption to undertake a careful and assiduous series of observations of the distances of Sirius, from the zenith of the island of St. Helena. This argument is drawn from an examination which he had made of the observations of the zenith distances of Sirius, taken at the Cape of Good Hope, in the years 1751 and 1752, by the Abbé de la Caille. Every particular observation of the same star was reduced to one epoch, that of the beginning of the year 1750, by applying the equations of aberration, precession, and deviation to the observed places; so that the places corrected ought all to agree together, if the observations were perfectly exact, and the star was affected with no sensible motion that was unaccounted for. Being satisfied with the excellence of the observations, Mr. M. was tempted to examine those of some of the principal fixed stars, in hopes of discovering some sensible differences in the observations made at different times of the year, when a parallax, if there had been any, would have had the greatest effect. But he found very few stars, the observations of which were sufficient in number, or taken at proper seasons of the year, to give room for any inference at all. Fortunately however the observations of the zenith distances of Sirius were more in number, and, what is still of more consequence in this case, made in various, and some in opposite seasons of the year: and, on looking them over, he was agreeably surprized to find a very sensible difference in the observations made at different times, agreeing in direction with what a parallax ought to produce, the zenith distance of Sirius in July coming out no less than 8″ greater than in the opposite season of the year, viz. December and January; the zenith distance also in March and April being of an intermediate quantity, as it ought to be: for Sirius being in conjunction with the sun in June and July, it is evident he must be then farther from the earth than in December and January, when he is in opposition

to the sun, and consequently his latitude and declination, which are both south, must be less in the former case than in the latter; and therefore, as he passes north of the zenith at the Cape of Good Hope, his zenith distance must be greater in the former season than in the latter, as the observations indicated: but, in March and April, when Sirius is in quadrature with the sun, and equally distant from the sun and earth, his apparent latitude and zenith disance must be the same as the true, or that which would obtain, if the earth was translated to the sun, and consequently, a mean between the zenith distances in July and December, agreeably to the observations.

Mr. M. then lays down the observations themselves, with the calculations which he made of of the values of the parallax for each, the maximum being assumed 9″, which he found would best reconcile the observations with one another.

	Zenith distance of Sirius at Cape, observed and reduced.	Annual parallax to reduce observat. to mean.	Zenith distance corrected for annual parallax.	Difference between each observation corrected, and the mean of the distances corrected.
1751, July 12	17° 31′ 19″.1	— 4.″3	17° 31′. 14″.8	— 1.1″
July 13	17 31 19.3	— 4.3	17 31 15.0	— 0.9
July 14	21.7	— 4.3	17.4	+ 1.5
July 15	19.7	— 4.3	15.4	— 0.5
July 16	19.0	— 4.3	14.7	— 1.2
1752, April 5	16.7	— 1.0	15.7	— 0.2
April 3	15.7	— 0.8	14.9	— 1.0
March 28	14.0	— 0.4	13.6	— 2.3
March 14	13.1	+ 0.7	13.8	— 2.1
March 8	15.2	+ 1.2	16.4	+ 0.5
March 5	18.8	+ 1.4	20.2	+ 4.3
March 4	17.3	+ 1.5	18.8	+ 2.9
March 1	15.9	+ 1.6	17.5	+ 1.6
Feb. 17	12.8	+ 2.6	15.4	— 0.5
Jan. 21	11.1	+ 4.0	15.1	— 0.8
1751. Dec. 31	11.6	+ 4.5	16.1	+ 0.2
Mean of zenith distances corrected for parallax..................	17° 31′ 15.″9			

The first column shows the year and day of the observations; the 2d the zenith distances of Sirius at the Cape of Good Hope, as delivered in Abbé de la Caille's recital; the 3d contains the computed values of the parallax at different times, taking that of the maximum 9″; the 4th column gives the observations reduced to the mean, by applying the parallax computed in the 3d column to the observations in the 2d; which quantities ought all to agree together, if the observations were liable to no error, and the parallax was rightly assumed: but, taking a mean of them all, the last column shows how much each of them differs from that mean, which in general is very small, and scarcely exceeds 2″,

except in two observations, in one of which it amounts to 3″, and the other to 4″: but these differ as much from the mean of the six other observations made at the same season of the year. Thus, assuming a parallax, the observations will be found to agree as well with that supposition, as they do with one another. But if the observations are considered in themselves without any allowance for parallax, they will differ sensibly from one another: nor is this difference to be found only in two observations, in which case it may easily be attributed to the account of the unavoidable errors; but five observations in July opposed to two in December and January, make the zenith distances vary 8″, in the direction which a parallax ought to produce.

Mr. M. is aware, that it may be objected, that two observations made in the winter season in December and January, at one of the maxima of the parallax, are too few to determine a point of such consequence, and readily agrees that the argument is weakened in proportion to the paucity of the observations: but then, it should also be considered, that the observations made in March and April concur with the rest in supporting the supposition of a parallax; and, on the whole, the observations will perhaps be judged to afford a sufficient presumption of the existence of a parallax, to encourage the undertaking of a careful trial.

LXXIX. Further Experiments in Electricity. By Mr. Benjamin Wilson, F.R.S. p. 896.

Being provided with a large square of glass, polished on both sides and fixed upright on one edge, Mr. W. placed, for a conductor, a slender piece of ivory, about one foot long, having one end within $\frac{2}{10}$ of an inch from the centre of the glass: at the other end were suspended 2 small balls of pith, by threads 4 inches long. The ivory was supported horizontally by a stand made of baked wood. When the glass was made a little warmer than the external air, his finger rubbed that side which was furthest from, and opposite to, the ivory. On which, the two sides of the glass were electrified plus, as were likewise the balls; which continued plus, even after they were removed from the glass into any part of the room. That the fluid here flowed from the finger into the glass he thinks may be inferred from the following experiment.

A piece of silver, being fixed on a slender rod of prepared wood, he rubbed the same glass with it, as he had done before with the finger; on which the silver was electrified minus, and both sides of the glass, with the conductor and balls, plus. There are therefore, he thinks, certain circumstances under which the electric fluid passes through glass; he says in certain circumstances, because in others, for instance, the Leyden bottle, the fluid does not

pass through the glass, but electrifies one side plus, and the other side minus, as Dr. Franklin has shown in his letters on electricity.

Hence he collects, that the three different effects, viz. the electrifying glass plus on both sides; or plus on one side, and minus on the other; or, lastly, minus on both sides, are occasioned by the different degrees of the same power and resistance in the respective experiments with the same glass.

Mr. W. concludes with an experiment made by Mr. Hamilton, professor of philosophy in the university of Dublin, as it seems to illustrate the doctrine of resistances, at least, so far as respects the air.

Let a slender brass, or iron wire, 5 or 6 inches long, and finely pointed at each end, be fitted in the middle, with a brass cap, void of angles; then let half an inch at each extremity be bent in opposite directions, till they are perpendicular to the rest of the wire, and in such a manner, that when the wire is suspended, by means of its cap, on a point of metal, it may lie in a plane parallel to the horizon. The pointed metal, which supports this wire, must be 2 or 3 inches long, and have its other end fixed into a small block of wood. Now, if this block, with a wire suspended, be set upon an electrified body, the wire will turn round with a very great velocity, moving always in a direction contrary to that in which the electric fluid issues from its points, without having any conducting substance near it, save that of the air: and if the wire be made to turn round by any other force, in the opposite direction, so that its points go foremost, it will when electrified, soon be deprived of that motion, and be made to turn round the contrary way.

This experiment, he says, was contrived, to try whether the electric fluid, which issues so freely from pointed bodies, would have any effect to move these bodies by its reaction; and that it has such an effect seems sufficiently manifest from the event. Mr. Hamilton apprehends, that the electric particles, by their elastic force, issue directly forwards from the points, and endeavour to expand themselves; but meeting with some resistance from the air, force the wire to move backward in a contrary direction, much in the same manner that a Catherine-wheel is made to turn round in a direction, contrary to that in which the small rockets affixed to its periphery discharge themselves. And therefore, he is inclined to think, that it might be made use of as an electrometer, by having it to turn round in a plane perpendicular to the horizon, and loading the wire with small weights near one of its extremities, which will be raised to a greater distance from the perpendicular line, as the electric fluid is stronger.

LXXX. Abstract of a Letter to Mr. Benjamin Wilson, F. R. S. concerning Electricity; from Mr. Torbern Bergman, of Upsal.* p. 907. *From the Latin.*

Dr. Franklin, in the 28th article of his 3d letter, wonders that a piece of ice, or an icicle, does not transmit the electric virtue, since water does it so well. But I, says Mr. B., have made some experiments, which show that a small quantity of water is not sufficient for this purpose. Thus, take a glass tube of 3 or 4 feet long, such as a barometer tube; fill it with water, and stop both ends by a perforated metal thread entering a little way into the water, as appears in the annexed figure. If now

two persons, disposed as in a circle for making an explosion, hold the ends of the metal threads A, A, and try to make the evacuation, yet this will not perfectly take place, for hardly the wrist, and very seldom the elbow will be shocked by this method. But augmenting the capacity of the tube, it transmits more, until it thus increases, so as to permit a full and free passage. It follows,

* This distinguished philosopher, who so greatly enlarged the bounds of chemical knowledge, and introduced into chemical analysis a degree of exactness unknown before, was a native of Sweden, and was born in 1735. He studied at Upsal, where he qualified himself for the medical profession, and took the degree of M. D. It does not appear however that he ever engaged in the exercise of the medical profession; but devoted himself to the pursuit of general science, and, after some years, to the study of chemistry. In 1761 he was appointed professor of mathematics and natural philosophy at Upsal; and about 6 years after, he succeeded Wallerius as professor of chemistry and mineralogy in the same university. About this time (1767) he published in the Stockholm Transactions his memoir on the crystallization of alum; and afterwards, for the space of 17 years, he continued to publish from time to time (till within a year of his death, which happened in 1784) various chemical dissertations, either separately, or in the Swedish Transactions before mentioned. To the inexpressible regret of the philosophical world, his life, (like that of his contemporary Scheele) was shortened by his unceasing application to chemical pursuits, he being only in his 49th year, when he died.

Among his principal works may be mentioned his Opuscula Physica et Chemica, 3 vols. 8vo; his Sciagraphia Regni Mineralis; his treatise De Attractionibus Electivis, (of all which there are English translations) and his Physical Description of the Earth, 2 vols. 8vo. published in the Swedish language.

Ingenuity, accuracy, industry and method are conspicuous in all the writings of Bergman; his labours were extended to a vast variety of subjects, in all of which he made large additions to the stock of chemical knowledge. Had he done no more than publish his Treatises on Elective Attraction, on the Blowpipe, and on the Method of Assaying in the Humid Way, he would still have ranked high among the chemists of the 18th century; but when we call to mind his experiments on earths, gems, and metals (including those on metallic precipitates), and his analyses of mineral waters, and the methods of preparing them artificially, (not to mention his experiments and observations on the acid of sugar, the aerial acid, &c. and his improvements in nomenclature) we cannot but regard him as one of the greatest chemists which any country or age has produced.

therefore, that water, as well as ice, when in small quantities, with difficulty transmits the concussion. Hence I suspected that a great quantity of ice would permit an easier transit, which was indeed proved by experience, though hitherto without using a piece larger than to give a shock to the elbows. But, of equal quantities of water and ice, the ice transmits the less.

LXXXI. Some Considerations on a late Treatise intitled, A new Set of Logarithmic Solar Tables, &c. intended for a more Commodious Method of finding the Latitude at Sea, by Two Observations of the Sun. By H. Pemberton, M. D. R. S. Lond. et R. A. Berol S. p. 910.

As it happens not unfrequently, at sea, for the unseasonable intervention of clouds to prevent the ordinary method of determining the ship's latitude by the sun's meridian altitude, even when it is of primary consequence that the true latitude should be known; a problem for remedying this disappointment is stated in many treatises of navigation, for finding the latitude of a place by any two altitudes of the sun, with the interval of time between them. A problem similar to this is proposed, and solved instrumentally on a globe, by a very early writer, Petrus Nonius, namely, to find the latitude by two altitudes of the sun, and the angle made by the azimuth circles passing through the sun, when the altitudes are taken. And since more commodious and accurate instruments for measuring time have been invented than were known to this author, the other problem has been proposed for the same purpose, of which a construction on the principles of the stereographic projection of the sphere is exhibitted by Mr. Collins, in his Mariner's Plain Scale new planed. And as the direct method of solving both these problems by numbers requires a diversity of trigonometrical operations, a set of tables has lately been published for a more compendious way of computation in the problem, where the interval of time was given, by which the ship's true latitude may be very expeditiously derived from the ship's dead reckoning, provided the observations are made within certain limits of time.

But however worthy of notice this method may be, new tables for the purpose are altogether unnecessary. It consists of two parts: the first computes, from the latitude exhibited by the dead reckoning of the ship, the distance from noon of the middle time between the observations, and thence the time of either: the 2d operation computes, from one of these observations, what should be the sun's meridian altitude, had the ship's reckoning given the true latitude; but if the latitude assumed from that reckoning is erroneous, the altitude thus computed will not be comformable to it; however, if the times for the observations are properly chosen, it will much better agree to the true latitude, and thence the assumed latitude may be more or less corrected.

But both these operations are an immediate consequence from the proposition in spherical trigonometry, usually delivered under the name of the 4th axiom, which is this: that the square of the radius is to the rectangle under the sines of the sides containing any angle, as the versed sine of that angle is to the difference between the versed sines of the third side, and of the difference between the sides containing the angle.

Hence Dr. P. institutes a rather long and laboured geometrical dissertation, to show how the set of tables in question are, or might have been constructed.

LXXXII. Of the Plants Halesia and Gardenia. In a Letter from John Ellis, Esq. R. R. S., to Philip Carteret Webb, Esq., F. R. S. p. 929.

The intent of this letter is to exhibit the characters of two new genera of plants, growing in Mr. Webb's garden, which Mr. Ellis calls after Dr. Hales of Teddington, and Dr. Garden, of Charlestown, South Carolina.* The first of these is thus described by Dr. Garden, when he sent the specimens and seeds.

"This beautiful tree grows commonly along the banks of Santee river, and rises often to the size of middling mulberry-trees. I have seen it sometimes more southerly, near the small rills of water; but of a much smaller size than that which grows on Santee. The wood is hard and veined; the bark is of a darkish colour, with many irregular shallow fissures. The leaves are ovated and sharp pointed, with the middle depressed, growing alternately on short footstalks. The flowers hang in small bunches all along the branches, each gem producing from 4 to 8 or 9 flowers, bell-shaped, and of a pure snowy whiteness. As they blow early in the spring before the leaves appear, and continue for 2 or 3 weeks, they make a most elegant appearance. They are followed by pretty large four-winged fruit, which likewise hang in bunches, each containing 4 kernels that are very agreeable to the taste."

This tree is mentioned by Catesby, vol. 1. p. 64, and called Frutex padi foliis non serratis, floribus monopetalis albis, campaniformibus, &c. &c.

Mr. E. had not heard that it was cultivated in England, till about 4 years before, when the Doctor sent him over from Carolina a large parcel of the seeds, which he distributed among many curious gardeners, and others: but he could not find any one that it had succeeded so well with as Mr. Gordon, gardener at Mile-end, a man who seems to be possessed of a knowledge peculiar to himself, in raising all the rarer and most difficult exotics from seeds, layers, or cuttings. He informs Mr. E. that it stands our winter in the open air, without shelter.

* It is the *Halesia Tetraptera* of *Linneus*.

The other plant is known by the name of the Cape Jasmine,* and is the most rare and beautiful shrub, that has yet been introduced into the European gardens, as well for the refreshing aromatic smell of its milk-white flowers, as the perpetual verdure of its leaves, which are like those of the lemon-tree. It promises, from the thickness and woodiness of its stem, together with its free manner of growing, to become a shrub of 6 or 7 feet high. It bears but one flower at the end of a branch; and the leaves grow opposite to each other on the branches. We are indebted to Capt. Hutchinson, of the Godolphin Indiaman, for this curious discovery, who, about 6 years ago, found it growing near the Cape of Good Hope, and, on his arrival here, presented it to Richard Warner, Esq. of Woodford, Essex; who finding great difficulty in propagating this valuable plant, either from cuttings, or by inarching it on the yellow Indian jasmine, as he had been advised, Mr. E. recommended him to try Mr. James Gordon, gardener at Mile-end; and, at the same time (August 1757,) by the interest of Gustavus Brander, Esq. F.R.S., he procured two cuttings of it for Mr. Gordon. These, with two more, which he afterwards received, he increased to so considerable a number, that, in order to dispose of them, he advertised for sale at 5 guineas a plant; and has had such success in the sale, that, reckoning the value of the plants on hands (with a proper allowance for the falling of the price, as they become more plenty,) he computes this plant will be worth at least £500 sterling to him.

Having dissected many dried as well as fresh specimens of this rare plant, Mr. E. found sufficient evidence (notwithstanding the flowers being double) to prove, that it belonged to quite another class of plants, as different from the jasmine as the rose is from the peony: that the fruit was below the receptacle, instead of being above it. Linneus says this plant belongs to the natural order of contorted flowers, that is, to those monopetalous flowers whose lobes, or sections of the limb of their petals, turn all to the right hand; such as the Nerium, Plumeria, Cerbera, Cameraria, Vinca, &c. and that it should be placed next to the Cerbera.

Mr. George Dionysius Ehret, F. R. S., published a most elegant plate of this plant, by the name of, Jasminum? ramo uniflore pleno, petalis coriaceis, with a note of interrogation, as a quære, before the word jasminum; leaving the determination, whether it is a jasmine or not, to a future inquiry.

Fig. A, pl. 15, represents the gardenia with a single flower, drawn from a dried specimen in the British Museum. B, the same in fruit. C, a capsula with five divisions in the calyx. D, the same cut across to show the seeds lying in the two loculaments. E, the seeds.

* The Cape Jasmine, as it is commonly called, is the *gardenia florida* of *Linneus*.

LXXXIII. An Eclipse of the Moon, Nov. 22, 1760, observed in Surrey-Street, Strand, London. By James Short, M. A., F. R. S. p. 936.

Penumbra very sensible at	7^h	26^m
Beginning of the eclipse at	7^h	39^m
Quantity of obscuration = 17′ 36″ at	8	49
Moon's diameter almost parallel to the horizon = 3′ 51″ at	8	53
End of the eclipse at	10	8

LXXXIV. The Case of a young Man, who had lost the Use of his Hands by Cleansing Brass Wire. By Mr. Samuel More, Apothecary in Jermyn-street. p. 936.

The disorders to which mechanics are unavoidably subjected by their employments, have exercised the pens of several ingenious writers; among whom Mr. M. would not presume to appear, but that he hopes the publishing the following case may be of some service: for he is certain, every one, who has been in the least conversant with the labouring people of London, must frequently have observed their hands in a condition something resembling, though perhaps not quite so bad as, that of the young man, whose disorder is the subject of the following paper.

And as there is great reason to believe that the complaint, here treated of, frequently happens to persons employed in the dyeing business, they will hereafter have a method of cure laid before them, which, with little expence, will probably be of great service to them. And, on that consideration only, he had been induced to submit this account of the disease to the public.

It is proper first to premise, that in drawing brass wire for the pin-makers, the frequent passing it through the fire to anneal it, covers it with a crust, which it is necessary to take off, before they can make use of it; and, for this purpose, it is sent to the dyers, who letting it lie for some time in the liquor with which they have dyed what they call Saxon colours, (which liquor is composed of water, oil of vitriol, alum, tartar, &c.) and then throwing it forcibly 3 or 4 times against the gronnd, the crust is, by degrees, broken off, and the wire rendered bright and fit for use. The gratuity given for this is generally allowed to the apprentices; and in this work Francis Newman had frequently at his leisure hours employed himself, till about the month of August 1759, when the cuticle on the palms of his hands, and the inside of his fingers, was become so hard and rigid, that he was no longer capable of doing either this or any other business.

For relief of this disorder, he applied to the person who attended the family in capacity of apothecary, who gave him several doses of purging physic, but without success. He was next admitted an out-patient of St. Thomas's hospital,

where he attended 6 or 7 weeks, but without receiving any benefit. Somebody then told him, his complaint was owing to the scurvy, (to which he had been subject) and he accordingly applied himself to several persons, who advertise remedies for curing that distemper, and among the rest, to Mr. Ward, of whom he had some pills; and once, by mistake, took 2 of them for a dose, which operated so violently, that the family imagined he could not survive it: however he still continued in the same condition. And now thinking that if he was admitted an in-patient at the hospital, he should be more likely to obtain a cure, he got himself admitted, and was there about 2 months longer; at the end of which time he was discharged, but in no better condition than before.

About a fortnight after this, and 12 months from the beginning of his disorder, viz. August 10, 1760, the person who was foreman to Mr. Newman, desired leave to write to Mr. M. for his opinion of the case; which being very readily granted, he desired him by letter, to come and see a young man, who, as he expressed it "had poisoned his hands with brass and oil of vitriol."

Mr. M. found him with his hands quite stiff, and utterly incapable of any business whatever; and having already had so much advice, and taken so many medicines, he concluded his disorder was incurable, and that he should entirely lose the use of his hands, the skin on the palms of them (the right hand rather the worst of the 2) having the exact appearance of parchment, full of chaps; and when he endeavoured by force to straighten the fingers, the blood started from every joint of them.

After hearing the best account he could get of the cause of his complaint, he imagined that as the disease had been contracted by his frequently dipping his hands into a violently acid liquor, the most probable method of relieving him would be by the application of an emollient liniment, mixed with an alkaline lixivium. For this purpose he ordered as follows: ℞ Ol. olivar. ℥iv. Lixivii salis alkalin. fix. ʒii. M. F. linimentum. With this he was ordered to anoint his hands frequently, especially going to bed; and to prevent the liniment being too soon rubbed off, constantly to wear a pair of gloves.

About 4 days after Mr. M. found the skin a little softened, and he could extend the fingers with less pain than before; and no blood issued on endeavouring to move them. This would have encouraged him to have continued the use of the same liniment; but as he complained much of its making his hands smart every time he used it, (and indeed this was the first application among the many he had tried that ever gave him any uneasiness) he concluded that the addition of some yolk of egg might lessen the acrimony of the alkaline salt, without at all abating the efficacy of the liniment: he therefore composed the liniment thus: ℞ Ol. olivar. ℥iv. Lixivii salis alk. fix. ℥ii. Vitel. ovor. N° ii. f. linimentum; to be used as before. This mixture not giving him so much pain as the former, he

had used it all in 3 days; and then coming to him for more, he found his hands still continue to mend; the skin that had grown hard scaling off, and a new flexible one appearing underneath; the chaps were many of them healed; and he began to have some use of his fingers. Encouraged by this success, he continued the use of the last prescribed liniment; and as from his not having had the proper use of his fingers for so long a time, their joints in a great degree lost their motion, he advised him alternately to clench his fist, and to stretch out his fingers many times a day.

The disorder had been so long on him, and had taken so deep root, that though he began very sensibly to amend from the first application of the liniment, yet it was full 2 months before he thought it advisable to leave off the use of it; and then to prevent a relapse he gave him the following ointment: ℞ Axung. porcin. ℥ii. Vitel. ovi. Ol. lavend. gt. v. f. unguentum. with orders to anoint his hands with it every night going to bed. This ointment he had continued to use about a month; and was then perfectly restored to the use of his hands, and began again to work at his business. During this course of anointing he took no internal medicines, except 3 doses of purging physic.

LXXXV. A Further Account of some Experiments made on the Bovey Coal. By Dr. Miller. p. 941.

Salt of hartshorn mixed with the phlegm that distilled first from the Bovey coal produced no ebullition nor air bubbles; but when mixed with the watery liquor, which arose with the thick oil in the latter part of the process, after it had stood some weeks in a glass bottle, close stopped, and was become perfectly fine, caused a very considerable ebullition, and the mixture immediately became foul and red. In some days after it got much thicker, and had the colour of tar. Its surface was covered with a bituminous pellicle, as were the sides and bottom of the glass. Eighteen grains of salt of hartshorn were not more than sufficient to saturate the acid salts contained in an ounce of the liquor, which was but very little sour to the taste.

Spirit of nitre dropped into this bituminous liquor, soon after it was distilled, and before it had deposited the oily particles (which rendered it cloudy) changed its colour to a deep brown; but had not that effect after the liquor was become transparent.

The black gritty powder, which remained after the former process, was put into a coated retort, and distilled by a naked fire; so that the whole body of the retort continued red-hot for more than 2 hours. This brought over to the receiver near an oz. of a watery bituminous liquor, rather stronger than that which distilled with a sand heat, and a few drops only of a thick bituminous matter, which stuck to that part of the receiver on which they fell. The neck of the

retort was thinly incrusted with something that resembled a saline concrete; but was found to be only bituminous matter. In the bottom of the retort there remained a very black gritty powder. Of this powder, one ounce was put into a crucible, set in a melting furnace, and kept in a pretty strong fire for an hour. The powder after it was cold appeared on the surface to be of a pale reddish colour; but was not in the least altered underneath. It lost however in weight near 3 drs. Some of the black powder taken out of the crucible, and thrown upon a red-hot iron, burnt without flame; but emitted plentifully a heavy black smoke. Two ounces of the black powder, which had been twice distilled, were set on a clear fire in an iron ladle, and continually stirred from the time that the ladle grew red-hot, and the matter began to emit a heavy black stinking smoke, till no more smoke arose from it. The calcined matter remaining in the ladle weighed 2 scruples, and seemed to be a kind of bole earth. This earth was evaporated in 2 ounces of rain water to one ounce, which some days after was poured off clean from the sediment. This water had not the least saline taste, nor did it give any sign of effervescence when spirit of nitre was dropped into it.

From the preceding experiments it appears that the substance called fossil-wood consists, for the most part of water, and that a considerable quantity of this principle is separated from it by a gentle heat; which seems to be the reason why such fossil-wood, on being exposed to the sun and air, or kept in a dry place, soon becomes full of superficial cracks, resembling a piece of timber, which by lying long on the ground in the open air, has begun to decay: that though the fossil-wood does not, like amber or pit-coal, yield by distillation a light oil floating on its phlegm, and a volatile acid salt in a concrete form, yet that a light oil and a volatile acid salt, in a considerable quantity, are intimately mixed with the water which distils from it: that this fossil-wood differs in several particulars from all wood belonging to the vegetable kingdom, which has been examined by fire after the same manner.

1st, Its powder burnt in a close vessel, and kept red-hot for a much longer time than is sufficient to reduce the like quantity of vegetable charcoal, emits (when sprinkled on a red-hot iron) a thick heavy black smoke. 2dly, The same powder, burnt as before mentioned, does not easily take fire, nor burn of itself, nor consume to ashes, even when exposed in an open crucible to a strong fire, and kept in it ignited, and almost white for a considerable time. 3dly, The matter left by this powder, after its phlogistic principle has been separated from it by time and air, contains no alkaline salt, and appears to be an astringent mineral earth; whereas charcoal easily takes fire, burns freely without smoke, and continues burning till it consumes to an ash; which consists of an alkaline salt, and a pure earth fit for making cuppels, and by these marks is sufficiently distinguished from all mineral substances.

LXXXVI. On the Aberration of Light Refracted at Spherical Surfaces and Lenses. By S. Klingenstiern of Stockholm. p. 944.

Too much complicated with intricate symbols to be of any real and practical use.

END OF THE FIFTY-FIRST VOLUME OF THE ORIGINAL.

I. On the Use of Furze in Fencing the Banks of Rivers. By the Rev. David Wark. p. 1. vol. 52.

Mr. W. and several of his acquaintances having experienced, on many ordinary occasions, the usefulness of furze bushes in stopping water, in pursuing the scheme, he found that locks and damheads might be raised, at one 10th of the ordinary expence, by the help of furze, as a very thin perpendicular wall of stone and lime, or one of deal boards, 2 inches thick, is the principal part of the expence. Close to this wall, on the other side, is a mound of furze intermixed with gravel, and along the top of the wall a strong tree, equal with the highest part of the mound. It is plain that this wall cannot be hurt by the weight of the water or force of the current, as it is defended by the contiguous mound, which is 6 or 7 yards broad; nor can the pressure of the mud and gravel make it give way, as their weight is suspended by the interweavings of the furze. If therefore the tree on the top of the wall can be made to keep its place, the whole is firm.

It is well known that the sea-dykes in Holland are made with faggots of any sort of brush-wood; and it must appear to any one who examines the net-work formed by the crossings of the branches and prickles of furze, that it is far more effectual for this purpose, both as it detains the collected earth, and is much more cheaply procured than faggots.

II. Of a Remarkable Halo. By Tho. Barker, Esq. p. 3.

This halo Mr. B. observed May 20, 1737, a quarter before eleven in the morning, and which continued half an hour, in a clear hot sky.

III. Of a Meteor seen in New England, and of a Whirlwind felt in that Country. By Mr. John Winthrop, Prof. of Phil. at Cambridge in New England. p. 6.

The southern parts of the province were greatly alarmed on Thursday the 10th of May last, about 35^m after 9 in the morning by a meteor. The weather being then fair and calm, the people at Bridgewater, and the towns near it, about 25 miles south from hence, were surprized with a noise, like the report of a cannon,

or volley of small arms, which seemed to come from the west. This report was followed by a rumbling noise, which most took for the roar of an earthquake; and when it had lasted about a minute, there was another explosion like that of a cannon; and about as long after a third; the roaring noise in the mean time increasing, so as to fill the air all around. After this third explosion, the noise gradually abated, seeming to go off toward the south-east; having lasted in the whole, as was judged, about 5^m. These noises were heard as far north as Roxbury and Boston: east, a league beyond Cape Cod; south, at Martha's Vineyard and Rhode Island; and west at Providence and Mendon; filling a circle of about 80 miles in diameter, the centre of which was at Bridgewater or near it.

The meteor which produced these noises, was not seen near the centre of this circle, but only near the circumference. A creditable person at Roxbury, a town adjoining on Boston, informed Mr. W. that about 10 o'clock that morning he saw in the air a ball of fire, about 4 or 5 inches in diameter, drawing a train of light after it. The ball was of a white brightness, exceeding that of the sun. Though the sun then shone out clear, this fire-ball was bright enough to cast a shade, by which he first perceived it in the south-east, passing below the sun. For he was standing with his back toward that and the sun; but this shade put him on turning round to discover what might be the cause of it. He says the ball moved parallel to the horizon from the north-east toward the south-west, not above half so fast as shooting-stars generally do, and disappeared while he was looking on it; and that about 4 or 5^m after he heard a kind of rumbling noise, somewhat like that of an earthquake; which was also heard by many others in Roxbury. From a vessel about a league south-west from Cape Cod, and from Martha's Vineyard, he received like accounts of a bright ball in the heavens, sufficient to ascertain the reality of the meteor, but not to determine its height and course.

Mr. W. also mentions one of the most extraordinary whirlwinds ever known in that country. The morning of July 10 at Cambridge was fair and hot, with a brisk gale at south-west. The afternoon was cloudy. About 5 it began to rain, and thundered once. At Leicester, 40 miles westward, about 5 o'clock the sky looked strangely; clouds from the south-west and north-west seemed to rush together very swiftly, and immediately on their meeting commenced a circular motion; presently after which a terrible noise was heard. The whirlwind passed along from south-west to north-west. Its first effects were discernible on a hill, where several trees were thrown down at considerable distances from each other. In this manner it proceeded the distance of 6 miles with the most destructive violence, tearing up and scattering about the trees, stones, fences, and every thing else in its way, forming a continued lane of ruins, of a few rods wide.

It met with only one dwelling house in its course, that of one David Lynde.

on which it fell with the utmost fury, and in a moment effected its complete destruction.

The house was of wood, 2 stories high, and both the chimneys of stone. Near the house were a shop and small shed; and the barn stood on the opposite side of the road, about 10 rods distant. As soon as they perceived the storm coming near the house some men within endeavoured to shut the south door; but before they could effect it they were surprized by the falling of stones around them, from the top of that chimney which was in the middle of the house. All the people in the house were in that instant thrown into such a consternation, that they can give no account of what passed during this scene of confusion, which was indeed very short. Where the house stood nothing remained but the sills, and the greater part of the lower floor, with part of the two stacks of chimneys, one about 10 feet, and the other not quite so high; the stones which had composed the upper part lying all around them. Except these sills, there were only 3 pieces of timber, and those very large, left entire; one of which, about 16 feet long, and 10 inches by 8, was found on the opposite side of the road, nearly south, about 20 rods distant from the house. The rest of the timbers, from the greatest to the least, lay broken and twisted to pieces between N.N.E. and E. for 70 or 80 rods from the house; some on the ground, others sticking into it a foot and two feet deep in all directions. Part of one of the main posts, about 10 feet long, with part of one of the plats of nearly the same length, and a brace which holds them together, were left sticking in the ground, nearly perpendicular, to a great depth, in a field southerly from the house about 8 rods distant. The boards and shingles of the house, with 3 or 4000 new boards which lay by it, were so entirely shattered, that scarcely a piece could be found above 4 or 5 inches wide, and vast numbers were not more than 2 fingers wide; some within the course of the wind and some without, at great distances on both sides of it. What has been said of the boards and shingles was likewise true of the wooden furniture of the house: the tables, chairs, desks, &c. shared the same fate; not a whole stick was to be found of any of them. Some of the beds that were found were hanging on high trees at a distance. Of the heavy utensils, pewter, kettles, and iron pots, scarcely any was found. Some nails that were in a cask in the east chamber were driven in great numbers into the trees on the eastern side of the house. The shop and shed before mentioned were torn in pieces, nothing of the shop remaining but the sills and floor; and a horse standing under the shed was killed. Only one person was killed.

From the whole, it seems highly probable that the house was suddenly plucked off from the sills (to which the upright posts are not fastened), and taken up into the air, not only above the heads of the persons who were on the lower floor, but to the height of those parts of the chimneys which were left standing,

where, by the violent circular motion of the air, it was immediately hurled into ten thousand pieces, and scattered to great distances on all quarters, except that from which the wind proceeded. And it further appears, that the violence of the wind in that place was over as soon as the house was taken up; otherwise no person could have been left on the floor.

IV. A Theorem on the Aberration of the Rays of Light Refracted through a Lens, on Account of the Imperfection of the Spherical Figure. By the Rev. Nevil Maskelyne, F. R. S. Dated from Prince Henry, St. Helen's Road, Jan. 16, 1761. p. 17.

About 2 years since, becoming acquainted with Mr. Dollond's curious discovery in optics, of correcting the aberration of the rays of light arising from the different refrangibility of the different sorts of rays, by a combination of 2 different kinds of glass; and learning from him in conversation that he had invented a theorem, showing the quantity of the aberration of the rays refracted through a lens, on account of the imperfection of the spherical figure; by the application of which, he was able to make the aberrations of the combined concave and convex object lenses perfectly equal to, and consequently to correct each other; Mr. M. was desirous of being more minutely acquainted with this further great improvement in optics; and Mr. Dollond accordingly readily offered to gratify his curiosity. But in the mean while that Mr. D. was looking over his papers, in order to lay them before Mr. M., Mr. M. having leisure, set about the investigation of a similar theorem himself; which having completed, he interchanged with Mr. Dollond for his theorem. The theorems, though similar, were not exactly the same; but by reduction to the same form Mr. M. inferred Mr. D.'s theorem from his, which gave him a further confidence of the exactness of both.

Let the form of the lens assumed, in the investigation of the theorem, be a meniscus, the radius of whose convex surface is greater than that of its concave surface; and the centre of the two surfaces lie on the same side of the lens as the radiant point, from which the rays diverge that fall on it. The ray falling on the extreme part of the lens will, after refraction, diverge from a point before the lens, nearer to it than the geometrical focus of rays diverging from the same radiant point, and passing indefinitely near the vertex.

Let Q express the distance of the radiant point, before the lens, from its vertex; R, the radius of concavity of the surface on which the rays first fall; and r, the radius of convexity of the second surface; F, the principal focus, or the focus of parallel rays, which will be on the same side of the lens as the incident rays; because R, the radius of the concave surface, is supposed less than r, the radius of the convex surface. Let the ratio of m to n be the same with that of the sine of incidence to the sine of refraction of rays passing out of air into glass, and

let Y express the semidiameter of the aperture of the lens; the angular aberration of the ray falling on the extremity of the lens, or the angle made between this ray, after being refracted through the extremity of the lens, and another ray or line, supposed to be drawn from the same extremity of the lens, to the geometrical focus of rays diverging from the same radiant point, and passing indefinitely near the vertex of the lens, expressed in measures of the arc of a circle to the radius unity, will be $\frac{(m^3 - 2m^2n + 2n^3) \times Y^3}{(m-n)^2 \times 2m \times F^3} + \frac{(mn + 4n^2 - 2m^2) \times Y^3}{(m-n) \times 2m \times F^2r} + \frac{(m+2n) \times Y^3}{2m \times Fr^2} - \frac{(4n^2 + 3mn - 3m^2) \times Y^3}{(m-n) \times 2m \times QF^2} - \frac{(2m+2n) \times Y^3}{m \times QFr} + \frac{(3m+2n) \times Y^3}{2m \times Q^2F}$. Where R, the radius of the first surface, is exterminated; and r, the radius of the second surface, is retained:

Or, exterminating r, the radius of the second surface, and retaining R, the radius of the first surface, the angular aberration is also expressed by $\frac{m^2 \times Y^3}{(m-n)^2 \times 2F^3} - \frac{(2m+n) \times Y^3}{(m-n) \times 2F^2R} + \frac{(m+2n) \times Y^3}{2m \times FR^2} + \frac{(3m+n) \times Y^3}{(m-n) \times 2QF^2} - \frac{(2m+2n) \times Y^3}{m \times QFR} + \frac{(3m+2n) \times Y^3}{2m \times Q^2F}$.

As in these theorems, the principal focus is supposed to lie before the glass, as well as the radiant point, to adapt the theorem to other cases, if the lens be of such a form as that its principal focus lies behind the glass, F must be taken negative: likewise, if the rays fall converging on the lens, or the point to which they converge lie behind the glass, Q must be taken negative: lastly, if the first surface be convex, R must be taken negative; and if the second surface be concave, r must be taken negative; and if, after all these circumstances are allowed for, the value of the theorem comes out positive, the aberration is of such a nature, as to make the focus of the extreme rays fall nearer the lens before it than the geometrical focus, or farther from the lens behind it: but if the value of the theorem comes out negative, the aberration is of such a kind as to make the focus of the extreme rays fall farther from the lens before it than the geometrical focus.

With respect to the application of this theorem to Mr. Dollond's combined object glasses, it is evident that if the aberrations of the convex and concave lenses added together (paying due regard to the signs of the theorem) are made equal to nothing, the two lenses will perfectly correct one another: but as there are two unknown quantities unlimited in the equation, namely, the radius of one surface of each glass (for F and Q are given, as well as m and n) there is room for an arbitrary assumption of one of them, at the discretion of the theorist or artist; which being done, there will remain a quadratic equation, whence there will result two values of the radius, which remains unknown, either of which will produce an aberration equal to that of the other lens.

V. Extract of a Letter from the Abbé De la Caille, of Paris, and F. R. S. to William Watson, M. D., F. R. S., recommending to the Rev. Nevil Maskelyne, F. R. S., to make at St. Helena a Series of Observations for discovering the Parallax of the Moon. p. 21.

The Abbé takes notice, "That though the parallax of the moon seems sufficiently well determined, by the observations made in 1751, in Europe and at the Cape of Good Hope; yet an element of this importance cannot be too well ascertained. He is of opinion that Mr. Maskelyne's continuance in St. Helena may be advantageously employed in making new observations; since the base on which these parallaxes should be calculated, ought to exceed the earth's radius. That if the R. S. approve of his proposition, and recommend to Mr. Maskelyne the execution of the scheme of correspondence which he has drawn up, he promises to comply with it punctually on his part.

The Abbé has accordingly sent Dr. Watson a series of observations, which he recommends to Mr. Maskelyne to make, from the 13th of June 1761, a few days after the transit of Venus, till the 9th of May 1762. This paper Mr. Maskelyne has transcribed, and proposes to make these observations in concert with the Abbé de la Caille. And if a copy of this paper, which Dr. Watson proposes to lay before the Society at their next meeting was put into the hands of Dr. Bradley, that gentleman might likewise make correspondent observations.

The Abbé likewise adds, 'that he has supposed that the sector, which Mr. Maskelyne takes with him to St. Helena, would take in $5\frac{1}{2}$ degrees on each side the zenith; and that his clock would be regulated by sydereal time.' This sector extends much beyond the Abbé's expectation, as it takes in $8\frac{1}{2}$ degrees on each side of the zenith.

The Observations recommended by the Abbé De la Caille to Mr. Maskelyne.

1761.	Sydera Observ.	Culmin. H.	M.	S.	Decl. A. G.	M.
Jun. 13.	λ ♍	14	6	15	12	16
Vesp.	α ♎	14	37	42	15	2
	☾	14	39	0	13	54
	γ ♎	15	22	13	13	59
Jun. 14.	☾	15	30	0	18	27
Vesp.	β ♏	15	51	37	19	7
	ν ♏	15	57	41	18	48
Jun. 15.	☾	16	19	0	22	3
Vesp.	μ ♐	17	59	31	21	6
Jun. 22.	γ ♑	21	26	52	17	44
Mane.	δ ♑	21	33	53	17	12
	☾	21	37	0	19	0
Jul. 10.	λ ♍	14	6	15	12	16
Vesp.	☾	14	26	0	12	30
	α ♎ ...	14	37	42	15	2
Jul. 11.	☾	15	12	0	17	12
Vesp.	β ♏	15	51	36	19	7
	ν ♏	15	57	40	18	48
Jul. 12.	β ♏	15	51	36	19	7
Vesp.	☾	16	2	0	21	0
	μ ♐	17	59	30	21	6
Jul. 19.	☾	21	26	0	20	20
Mane.	γ ♑	21	26	52	17	44
	δ ♑	21	33	53	17	12
July 20.	γ ♑	21	26	52	17	44
Mane.	δ ♑	21	33	53	17	12
	☾	22	16	0	15	24
	δ ♒	22	42	0	17	7
Aug. 6.	☾	14	7	0	10	40
Vesp.	ζ Ophi..	16	24	4	10	4
Aug. 7.	☾	14	57	0	15	50
Vesp.	η Ophi..	16	56	45	15	24
Aug. 8.	☾	15	48	0	20	5
Vesp.	μ ♐	17	59	30	21	6
Aug. 14.	μ ♐ ...	17	59	30	21	6
Vesp.	π ♐	18	55	35	21	23
	☾	21	9	0	21	35
Aug. 15.	γ ♑	21	26	52	17	44
Vesp.	δ ♑	21	33	53	17	12
16. Mane.	☾	21	59	0	17	45
Aug. 17.	δ ♒	22	42	0	17	7
Mane.	☾	22	49	0	13	0
Sept. 11.	γ ♑	21	26	52	17	44
Vesp.	δ ♑	21	33	53	17	12
	☾	21	40	0	19	20

1761.	Sydera Observ.	Culmin. H.	M.	S.	Decl. A. G.	M.
Sept. 12.	☾	22	30	0	14	50
Vesp.	δ ♒	22	42	0	17	7
Oct. 8.	π ♐	18	55	35	21	23
Vesp.	☾	21	18	0	21	3
Oct. 9.	γ ♑	21	26	52	17	44
Vesp.	δ ♑	21	33	53	17	12
	☾	22	11	0	16	55
	δ ♒	22	42	0	17	7
Oct. 10.	α ♑	20	4	26	13	6
Vesp.	☾	22	58	0	11	55
Nov. 5.	γ ♑	21	26	52	17	44
Vesp.	♑	21	33	53	17	12
	☾	21	51	0	18	50
Nov. 6.	δ ♑	21	33	53	17	12
Vesp.	☾	22	37	0	14	16
	δ ♒	22	42	0	17	7
Dec. 2.	☾	21	28	0	20	50
Vesp.	β Ceti ..	0	31	38	19	18
Dec. 3.	☾	22	17	0	16	12
Vesp.	β Ceti ..	0	31	38	19	18
Dec. 4.	☾	23	5	0	11	20
Vesp.	ε Ceti ..	2	28	5	12	54
Feb. 14.	λ ♍	14	6	17	12	16
Mane.	☾	14	17	0	13	0
Feb. 15.	α ♎	14	37	44	15	2
Mane.	☾	15	11	0	18	5
Mart. 13.	☾	13	54	0	10	25
Mane.	λ ♍	14	6	17	12	16
Mart. 14.	α ♎	14	37	44	15	2
Mane.	☾	14	46	0	16	6
Mart. 15.	☾	15	40	0	20	0
Mane.	β ♏	15	51	37	19	7
Apr. 10.	λ ♍	14	6	17	12	16
Mane.	☾	14	18	0	13	25
	α ♎	14	37	44	15	2
Apr. 11.	☾	15	16	0	18	48
Mane.	β ♏	15	51	37	19	7
	ν ♏	15	57	41	18	48
Maii 6.	☾	13	57	0	10	55
Vesp.	λ ♍	14	6	17	12	16
Maii 7.	α ♎	14	37	44	15	2
Vesp.	☾	14	50	0	16	35
Maii 9.	☾	15	44	0	21	20
Mane.	β ♏	15	51	37	19	7

VI. A Letter from the Rev. Nevil Maskelyne, M.A., F.R.S., to Wm. Watson M. D., F.R.S. Dated Prince Henry, St. Helen's Road, Jan. 17, 1761. p. 26.

In a letter written to you from this place the beginning of this week I desired

you would, in your answer to Abbé de la Caille, acquaint him that I had proposed to the R.S. the observations of the moon's parallax before his letter came; and that Dr. Bradley was to make observations at Greenwich correspondent to mine at St. Helena; and that I was drawing up a list of the proper observations to be made, and the proper stars with which the moon was to be compared, which I proposed to transmit to the Abbé de la Caille, in order that he might attend to the same observations if he thought proper. But as he has made out a list of proper opportunities of observing, I shall only set down 5 observations to be added to it, which I beg you will transmit to the Abbé de la Caille; and likewise deliver a copy of the same to Dr. Bradley.

I also desired in my letter that you would request the Abbé de la Caille, and the other French astronomers by him, to attend to the observations of the eclipses of Jupiter's satellites; especially the first, from May 1761 to June 1762 inclusive, in order to settle the difference of longitude between Paris and St. Helena; which if it came in the name of the Society it would be better; and that you would also deliver it as my request to the Society, that they would recommend it to my Lord Macclesfield, Dr. Bradley, Mr. Raper and Mr. Short, and any other gentlemen they know propose to attend carefully to the observation of the transit of Venus, to make as many observations of the eclipses of the satellites as they conveniently can, in order to settle the difference of longitude between their place of observation and St. Helena in the most exact manner; which is of the utmost importance with respect to the use to be made of the observations of the transit of Venus.

1762.	Sydera Observ.	Culmin.	Decl. A.
Jun. 2.	☾	13ʰ 37ᵐ	8° 45′
Vesp.	γ ♍ ..	14 0	9 7
Jun. 3.	☾	14 30	14 36
Vesp.	α ♎ ..	14 37	15 1
Jun. 4.	☾	15 23	19 29
Vesp.	♏ ..	15 51	19 6
Jun. 30.	β	14 16	12 59
Vesp.	α ♎ ..	14 37	15 11
Jul. 1.	☾	15 8	18 13
Vesp.	β ♏ ..	15 51	19 6

VII. On a Samnite Denarius, never before published. By the Rev. John Swinton, B.D., of Christ-Church, Oxon., F.R.S. p. 28.

This is a silver coin of the size of the larger denarii, and with two Etruscan inscriptions. On one side is a galeated head, with an inscription in Etruscan letters answering to the Roman FITEEIV. On the reverse are 3 human figures with an Etruscan inscription answering to the Roman C. PAAPII. C.

VIII. Account of an Eruption of Mount Vesuvius. By Sir Francis Haskins Eyles Stiles, Bart., F. R. S. Dated Naples, 23d Dec. 1760. p. 39.

The mountain, which was quiet this morning, with scarcely any visible smoke, threw up on a sudden about noon a vast column of black smoke, which rose to a very considerable height; and before it had diffused itself made a splendid and glorious appearance, as the sun, which was then shining, gilded the superior

part of it; but soon after it dispersed and covered all the mountain, and a great portion of the sky in that quarter. The ashes that fell from it resembled the falling of a heavy shower, seen at a distance, and must have done great mischief, if any living thing was under them, as is but too probable. The drift of this storm was towards the south-east, the wind being nearly north west. Portici might be within its influence, but the body of the smoke seemed to go beyond it. At the same time that this smoke broke out, they observed two large columns of smoke arising at the foot of the mountain, on the south-east side of it, which bespoke eruptions in that part: and this has proved true; for the first smoke from the top soon after decreased, probably from the event obtained at the foot; and ever since sun-set they have seen the foot all on fire. The direction of the line of fire was from the mountain towards the sea.

IX. Another Account of the same Eruption of Mount Vesuvius. By Sir Francis Haskins Eyles Stiles, Bart., F. R. S. Dated Naples, 29th Dec. 1760. p. 41.

He went with others on the 24th to take a nearer view of the eruption; they took the great road to Salerno, and about 10 miles from Naples, about mid-way between Torre del Greco and Torre del Annuntiata, they were stopped by the stream of lava which had crossed the road, and was making for the sea. The mouths of the eruption were about a mile and half, or better, to the left, and were raging in a very frightful manner, as the noise of the explosions, which succeeded each other, at the interval of only a second or two, was equal to a storm of thunder. The flames were very bright after it was dark; and the accensèd stones, which were thrown up in vast quantities at every explosion, resembled the springing of a mine, as they call it, in a fire-work. The lava has not yet reached the sea, though it was said to be within half a mile of it when they were there. A small rising of the ground before it has obliged it to spread in breadth, and its progress for the shore is very slow. The mouths are said to have been 14 in all at first, afterwards reduced to 8, and now much fewer. There are 3 hillocks, large enough to be distinguished at Naples, that are formed by the stones and matter thrown up at these mouths, and one of them is already a young mountain. Some imagine the eruption will last many months, as the lower eruptions have generally lasted longest; and this is it seems a great deal lower than any that ever happened.

X. Extract of a Letter from Mr. Robert Mackinlay, dated at Rome, Jan. 9, 1761, *concerning the late Eruption of Mount Vesuvius, and the Discovery of an Ancient Statue of Venus at Rome.* p. 44.

There has been a most terrible eruption lately of mount Vesuvius, about the latter end of last month, but the accounts hitherto arrived are not very distinct; however, they all agree that there were 9 new mouths or openings towards the

Torre del Greco and Annoneiada; that very considerable shocks of an earthquake were felt all over Naples: that neither fire nor smoke eame out of the old crater; that the lava had run into the sea: and that beyond Portiei, on the high road, the lava was 17 palms in height, and some of the streams 400 yards broad. Much damage has been done to houses and vineyards; and it is said the palace of Portici has suffered somewhat.

In the month of September last a Venus, of most exquisite workmanship, was dug up here in the Mons Cœlius near the plaee ealled Clivo Scauri. It is in the possession of the Marquis Carnavallia, who gave 50 scudi to the workmen, their full demand, as the half of the value, according to agreement, though it is worth some thousands. It is full 6 feet high, in the same attitude with the Venus of Medicis, with this differenee, her right hand before her breast, and her left supporting a light drapery before the pudenda. On the base, which is of one piece with the statue, and quite entire, is the following inscription:

Α Π Ο Τ Η C
Є Ν. Τ Ρ Ω Ι Α Δ Ι
Α Φ Ρ Ο Δ Ι Τ Η C
Μ Η Ν Ο Φ Α Ν Τ Ο C
Є Π Ο Ι Є Ι.

XI. On the Term and Period of Human Life. And on Dr. Halley's and other Tables of Lives. By T. W., A. M. p. 46.

Mr. T. W. is dissatisfied with all the tables of lives, chiefly beeause they begin with too small a number of persons, 100 or 1000, &c. by whieh it happens that long before the 100th year they are all dead, and the tables become useless for all ages after 80 or 90, though many persons live to mueh higher ages. He would therefore have the tables to begin with at least 100,000 persons born. If we look baek, says he, we shall find the first sketeh, that of Capt. John Graunt, alias Sir William Petty, was formed on 100 only, and sueh a table earried the aceount to the 80th year, or upwards. Next were introdueed those of 1000, which extended the computation of life to between 84 and 100; tables formed on 10,000 would advance to above 105; and on 100,000, duly proportioned from the materials we have, might eontinue the aeeount to 115 years and upwards. If in the first sketch, the supposed term of life was closed too soon, and it was an improvement to carry on an account of the gradual decay beyond the 90th year, why are we to rest here, having additional observations made for more than 60 years, whieh furnish materials for a further progress? If there is room, and good foundation to advanee but 20 years beyond the compass of the present tables, should not this be done? And will it not make a considerable

yet necessary, alteration in all computed values, on annuities to be granted to persons in the latter part of life?

XII. Experiments on Checking the too Luxuriant Growth of Fruit Trees, tending to dispose them to produce Fruit. By K. Fitzgerald, Esq., F.R.S. p. 71.

Mr. F. had observed a method taken to bring young trees to bear, when planted in too rich a soil, by cutting away part of the bark from some of the main branches. This method had brought them soon to bear plentifully; but it leaves an ugly wound, the wood continuing bare, and apt to rot in that part. He had some young plumb and cherry trees planted against a north pale, in a very rich soil. The plumb trees had in 3 years shot forth the extremities of their branches to 15 or 16 feet distance, and had quite covered and overtopped the pale. As the cutting away of any of these branches would make the rest shoot the stronger, he made the following experiments about the middle of August 1758.

He made a circular incision on the main arms of an Orleans plumb tree, near the stem, quite through the bark, where it was smooth, and free from knots. About 3 or 4 inches higher, he made another incision, in the same manner; then making an incision lengthwise, from the upper to the under circumcision, he separated the bark entirely from the intermediate wood, covering it, and also the bare part of the wood, to keep the air from the wound; and letting them remain so for about a quarter of an hour, when the wound began to bleed, he replaced the bark as exactly as he could, and bound it round pretty tightly with bass, so as to cover the wound entirely, and also about half an inch above and below the circumcisions. He treated the entire stem of a duke cherry tree in the same manner, about 10 inches from the ground, and below all its branches. Also several branches of a morelli cherry tree; and the main arms of two perdrigon plumb trees. These last two were old trees, which had been cut to the ground about 4 years before, and had shot forth very luxuriant branches, but had not since borne any fruit. In about a month's time the bark of these began to swell, both above and below the binding, when he unbound each of them, and found the several parts, that had been replaced, all fairly healed except one, which was on the main arm of the perdrigon plumb tree, part of which was healed, and about an inch in breadth of the bark on one side of the longitudinal incision, remained loose, and afterwards dropped off. He bound them all again lightly with bass, and let them remain so till the beginning of the summer following: when he took off the binding entirely, and found them all healthy and flourishing. Each of these trees bore plentifully that season, though in general reckoned a bad year for fruit.

This induced him, in the beginning of August 1759, to make similar experi-

ments on several other young trees; some that had not yet borne any fruit, and others that had borne but a small quantity; particularly two young pear-trees, that never yet had any bloom. He treated the main arms of one of these in the manner already described, and also several of the branches that grew on these arms; likewise one of the arms of the other pear-tree. The first of these bore a surprizing quantity of fruit last summer; and the circumcised arm of the other bore a moderate quantity, though no other part of the tree had any appearance of bloom.

He made also the following experiments on two branches of different young apple-trees, as nearly of the same size as he could find. He cut off the bark of these as exactly as he could by a guage; changing them, and putting the bark of the branch of one tree on the branch of the other. A small slip of wood came off with the bark of one, and the bark of the other had a leaf-bud on it; which branch had also 2 apples growing on it. The bark of each of these healed perfectly, and the apples remained on, and ripened with the rest: the leaf-bud pushed forth leaves, and both the branches bore so very plentifully the last summer, that one broke down with its load; and the other would also probably have suffered the same fate, but that he had it supported. These were both nonpareil apple-trees planted in asparagus beds.

He changed the barks of the branches of a peach and a nectarine tree: that which was placed on the peach-tree healed perfectly, and the branch produced a quantity of bloom last season; but the bloom of the whole tree, as well as of several others against the same wall was entirely blasted. The gardener cut off the branch of the nectarine when he was pruning, and nailing the trees, as he did of several others, on which had been made experiments of the same kind; against which he declared his opinion strongly at the time of making.

About the beginning of November last, Mr. F. cut off one of the arms of the perdrigon plumb tree, which had the experiment made on it in 1758, to examine what effect it had on the wood; to which he found the bark between the circumcisions more firmly united than in any other part. There was a dark vein, which ran through the wood in that part which appeared of a harder texture than the rest of the branch.

XIII. *Of the Urtica Marina.** *By Joseph Gaertner, M.D.* p. 75.

Having lately visited the southern coasts of Cornwall, Dr. G. met with several new and undescribed sorts of the urticæ marinæ, called by Mr. Hughs the animal flowers. The name of urtica, as the celebrated M. de Reaumur justly ob-

* The animals described in this paper belong to the genus *Actinia;* and are in some degree allied to hydræ or polypes.

serves, has been very improperly applied to this kind of animals; for it is certain that not a single species of them is possessed of that stinging quality like a nettle, which the ancients ascribed to them, and that only their tentacula feel rough and clammy when touched with the finger. Even this roughness is not perceptible, but when the animal attempts to lay hold of the finger: it then throws out of the whole surface of the feeler a number of extremely minute suckers, which sticking fast to the small protuberances of the skin, produce the sensation of a roughness, which is so far from being painful, that it even cannot be called disagreeable.

The genus which these sea-nettles belong to is that of the hydra of Linneus, commonly called the polype. This will appear from the following characters: from the gelatinous substance, of which this whole tribe of animals consists; from their having only one opening in their bodies, which gives a passage to the food as well as to the excrements of the animal; and from a set of feelers, which surround this opening, and serve for claws to catch their prey and convey it to their mouths. As the sea-nettles agree perfectly in those general characters with the hydra, so do they also answer to many of its less essential, or merely accidental qualities: they live for instance constantly in the water, in which they never swim, but always adhere to some fixed body in it; and when they change their place, most of them crawl along so very slowly, that their progressive motions cannot be perceived with the eye. To this may be added, that they likewise bring forth their young ones alive, and that they grow again after considerable parts of their bodies have been cut off: all which proves still further, that these animal flowers, or sea-nettles, are of the same nature, have the same characters, and do consequently belong to the same genus with the hydra. The polypes in general may be divided into 2 classes, the one containing those polypes, that cannot conceal their feelers, though ever so much irritated; and the other, those that on the least irritation contract themselves, draw in their feelers, and frequently hide them under a membranaceous cover made for that purpose. The first class, on account of the small number of species belonging to it, needs no subdivisions; but to distinguish properly the several sorts of the 2d class, it is necessary to divide it according to the various positions of the feelers, which are inserted either in the membranaceous cover itself, or into a flower-like production of the body, or lastly, in the very top part, or the disk of the polype: hence arise the three following subdivisions of the 2d class: 1. Hydra calyciflora. 2. Hydra corolliflora; and lastly, Hydra disciflora.

The first class consists but of a single sort, whose specifical character may be thus expressed:

Hydra tentaculis denudatis, numerosissimis; corpore longitudinaliter sulcato.*

The body of this polype is of a light chestnut colour, and feels perfectly smooth,

though it be lengthways sulcated by a number of sulci, that are frequently divided into 3 smaller ones, and are continued into the dentated margin, that surrounds the upper periphery of the body, just beneath the insertion of the feelers. These feelers, rising from the disk of the polype, are, according to the age of the animal, between 120 and 200 in number; they exceed the body when expanded by more than an inch in length, and are of a beautiful sea-green colour, except toward their extremities, which are coloured with a lively red, like that of the rose. The disk is of the same brown colour with the rest of the body, and contains in its centre the mouth of the animal, which is an aperture of various shape and diameter. The two varieties of this species, which Dr. G. met with, differ but little from the already described animal. The feelers of the one, instead of being green, are throughout of a red colour, like that of mahogany wood. The other variety has pale ash-coloured feelers, marked with a small white line running along their back; its body is of the same chestnut colour with that of the first species; but the sulci are not divided, nor has it a dentated margin surrounding its upper periphery.

The polype belonging to the 2d class, concealing their feelers when irritated, are the following: Hydra calyciflora, tentaculis retractilibus variegatis, corpore verrucoso.†

From its small basis rises a cylindric stalk, which supports the roundish body of the animal, whence afterwards the calyx, being a continued membrane of the body, draws its origin. The stalk, or the penduneulus of the polype is quite smooth, and its colour inclines towards carnation. The outside of the calyx, and the body of this animal, are marked with a number of small white protuberances, resembling warts, to which fragments of shells, sand-grains, &c. adhere, and hide the beautiful colour of these parts, which, from that of carnation, is insensibly changed towards the border of the calyx, first into purple, then violet, and at last into a dark brown. The inside of the calyx is covered with the feelers, that grow in several ranges upon it: they differ considerably in length; those that are near the edge of the calyx being but small papillæ, in proportion to those that surround the disk, or the central part of the body. They are almost transparent; and some of them are of a pale ash colour, with brown spots; others, on the contrary, are of a chestnut colour, marked with white spots. The disk is formed like a star, which, according to the figure that is traced out by the innermost row of the feelers, consists of many angles. The colour of this part of the body is a beautiful mixture of brown, yellow, ash-colour, and white, which together form variegated rays, that from the cen-

* This is the *Actinia Cereus* of Ellis.

† This is the *Actinia Bellis* of Ellis.

tre, or the mouth of the animal, are spread over the whole surface of the disk.

This polype contracting itself, changes its body into an irregular hemisphere, which is so covered with the several extraneous bodies that stick to it, that it is extremely difficult to know the animal in this state, and to discern it from the rubbish that commonly surrounds it.

These animals are frequently found in the pools about Mount's-Bay. It is rare to meet with a single one in a place, there being most commonly 4 or 5 of them living so near together in the same fissure of the rock, which they constantly inhabit, that their expanded calyces form a row of flowers like bodies, that seem to grow upon the cliffs under water.

The 3d species, is the Hydra corolliflora, tentaculis retractilibus frondosis.* This animal, in its contracted state has more the appearance of a caterpillar than of a polype. Its body is covered with a dusky white skin, in which a large opening appears at the thicker extremity of the body, and at the opposite end of it are 5 small denticles, that surround a cavity placed in their middle. The surface of this cylindrical body is marked with 6 double rows of perforated knots, which the animal can transform into as many legs, if occasion requires, by extending each tuberculum into a small transparent cylinder, whose extremity, like that of the suckers of the star-fish, sticks fast to every thing which the animal gets hold of, and consequently serves it for an instrument, not only to fix its body with, but also to push it forward, by the help of many of these suckers that are formed of the several knots of different rows. The head of the polype coming out of the above-mentioned opening in the skin, is of an oval, and sometimes of an hemispherical figure, somewhat like the coralla of an asarum, but much larger in size. It is quite hollow within, and consists of a dark brown, yet almost transparent membrane, which, after having found the head, produces the feelers that surround the large aperture at the top of it. These feelers are 8 or 10 in number, and of the same substance and colour with the head; they are divided into several branches, to which, as well as to the principal stems, many clusters of very minute papillæ adhere, which make them exactly resemble small branches of trees covered with their leaves. These leaves, or papillæ, not only contribute to the beauty of the feelers, being of a pale yellow, mixed with a shining white like silver, but they also render the feelers more useful to the animal, in filling up the interstices between them, through which smaller insects else might pass, without being perceived by the animal, whose natural food they are. This polype seems to live at the bottom of the sea, distant from the land. Dr. G. met but once with it on the shore, between Penzance

* This animal is a species of Holothuria, and is the *Holothuria pentactes* of Linneus.

and Newland, where it is thrown up by the sea, inclosed in a large hollow root of the fucus palmatus.

The 3d species, is the hydra disciflora, tentaculis retractilibus subdiaphanis; corpore cylindrico, miliaribus glandulis longitudinaliter striato.* Its body when extended, is of a cylindrical figure, and constantly marked with some rows of small knots, or glandulæ, that are placed in straight lines from the top to the basis of this cylindrical stalk. Each row is composed of 3 files of glandulæ, of which the middle one is remarkably larger than the two others; their number is uncertain, yet he never met with less than 8 rows in an animal grown to its full age. The colour of the stock near its basis is a pale red, and the rest is of a yellow, mixed with a grey ash-colour. The glandulæ are almost of the same colour with the body, except those of the middle file of each row, which is white. Out of the top part, or the disk of the polype, grow the feelers, from 18 to 36 in number; they are of a half-transparent substance, and of a whitish colour, variegated only at the upper part of the feeler, like the back of some snakes, with several cross-lines, and brown spots of an irregular figure. The disk of this polype is always convex, and chiefly of an orange colour, except towards its periphery, which is marked with many dark brown spots, that surround the insertion of the feelers. At the least irritation, this animal contracts its body, and changes its cylindrical figure into a conoidal one. The fissures of the rocks in the sea are the only place where he met with this sort of polypes, which is not common on the coasts of Cornwall.

Of this species he found two varieties. The top parts of the one are in shape and colour much the same with those of the already described animal; the stalk only is of a deep green colour. The 2d variety has likewise a green stalk; but its feelers are not variegated, being throughout of a pale and transparent red colour. The animal flowers of Mr. Hughes, and the sea-nettle, with a shagreen skin of Mr. de Reaumur, may perhaps belong to this subdivision.

The last species of these polypes is the hydra disciflora, tentaculis retractilibus, extimo disci margine tuberculato.† The colour of its body is always red in the summer, but changes into dusky green, or brown, towards the latter end of autumn. The outside of it is quite smooth, some few animals of this sort excepted, which are marked, like the first species of this class, with small protuberances, to which several extraneous bodies likewise adhere. The feelers are constantly inserted into the disk of the polype, but they are of various colours, viz. red, blue, white, and sometimes even variegated. Between these feelers and the membranaceous cover of the animal, is a row of small hemispherical tu-

* This is the *Actinia gemmacea* of Ellis.

† This is the *Actinia mesembryanthemum* of Ellis.

bercula, which, though they vary in colour as much as the feelers, yet are constantly found to be placed on the edge or periphery of the disk, and consequently afford, together with the insertion of the feelers, a certain mark, by which this animal, so variable in its colour and shape, may be at all times known and distinguished from any other sort belonging to this tribe.

XIV. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the year 1760, *&c. By John Wilmer, M.D. &c.* p. 85.

This is the 39th presentation of this kind, completing to the number of 1950 different plants.

XV. An Account of the Cicuta, recommended by Dr. Storke, By William Watson, M.D., F.R.S.* p. 89.

Dr. W. observes that in a paper he had a short time before presented to the R.S. he had endeavoured to demonstrate that the Cicuta major which, since the publication of Dr. Storke's work at Vienna, had been used medicinally in England, was the plant intended by that gentleman; and not the Cicuta aquatica, as had been suggested by some practitioners here. And Dr. Storke had removed every doubt, which could remain, by transmitting hither some leaves of the Cicuta major, or common hemlock, which grew at Vienna, and was of the same species with the plant so denominated here.

As Dr. Storke informs us, that, since the publication of his treatise, he has received letters from almost every part of Europe, confirming his good opinion of the virtues of the Cicuta, and as he is about to publish a 2d treatise on the same subject, containing still more extraordinary accounts of cures, brought about by administering that plant; there is no doubt therefore, but that endeavours will be made here, to confirm the truth of the doctor's assertions; more especially as some of the diseases in which Dr. Storke found the Cicuta attended with great success, are such as are of all others the most shocking to human nature, and have, by too long experience, been found to give way to no other means. Hence it is highly important to every one, more particularly to physicians, that the very plant directed by Dr. Storke be administered, and no other instead of it, either through inattention or want of knowledge; as judgment in the physician is of no real service, unless his prescriptions are faithfully prepared.

For these reasons, it may not be improper to inform those medical practitioners, who are not conversant in botany, and who may yet be desirous of trying the effects of the Cicuta, that at this time of the year there is another plant, growing

* *Conium maculatum*, Linn.

in the same places, and often mixed with it, so much resembling it in appearance, as not, without some attention, to be distinguished from it; which however greatly differs from it in sensible qualities. Great care therefore ought to be taken, that the one of these should be selected from the other. The plant so much resembling hemlock, is the Cicutaria vulgaris of the botanists, which in some parts of England is called cow-weed, in others wild cicely. Its greatest resemblance to hemlock is in the spring, before the stalks of the leaves of the hemlock are interspersed with purple spots; and therefore at that season more easily mistaken for it; though even then the leaves of the hemlock smell much stronger, are more minutely divided, and are of a deeper green colour, than those of the cow-weed. Afterwards indeed they are more easily distinguished, as the Cicutaria flowers at the end of April and beginning of May, and the Cicuta not till June, when the other is past; to say nothing of the flowering stalk of the cow-weed being furrowed, and somewhat downy; and that of the hemlock, smooth, even, and always spotted. These plants differ likewise very essentially in their seeds, which in the cow-weed are long, smooth, and black, when ripe; whereas those of the hemlock are small, channelled, and swelling towards the middle.

Besides the cow-weed, there is another plant in appearance very like the hemlock, though evidently differing from it in other respects; and it seems quantities of this have been collected, and sold in London for the hemlock. This is more likely to be taken for the hemlock in summer or autumn, as it is an annual plant, and is produced and flowers late in the season. The plant here meant is the Cicuta minor of Parkinson, or Cicutaria tenuifolia of Ray. This however is easily distinguished from hemlock, by its leaves being of the colour and shape of parsley, its flowering stalks having no purple or other spots, and not having the strong smell peculiar to hemlock.

To the two plants before-mentioned, may be added a third, which very frequently, more especially about London, grows along, and is mixed, with the hemlock. This plant is called, by the late excellent Mr. Ray, Small hemlock-chervil with rough seeds; and is denominated by Caspar Bauhin, in his Pinax, Myrrhis sylvestris seminibus asperis. This, like the cow-weed before-mentioned, can only be mistaken for hemlock in the spring. It may be distinguished then from it, by the leaves of the myrrhis being more finely cut, of a paler green colour, and, though they have somewhat of the hemlock smell, are far less strong, and have no spots. This plant flowers in April, and the seeds are ripe before the hemlock begins to flower; and these seeds are cylindrical, rough, and terminate in an oblong point.

The leaves of hemlock are more fit for medical purposes, as being in their greatest perfection, when collected in dry weather, from the middle of May to

the time that their flowering stems begin to shoot; as by that time the plants will have felt the effects of the warm sun, have acquired a highly virose smell, and the stems of the leaves are covered with purple spots, an argument of the exaltation of their juices: and we should be attentive here to give them all these advantages, as 3 degrees of latitude, and other circumstances of soil and situation, may occasion a very sensible difference in the qualities of the same plant; an instance of which occurs in the plant under consideration, and may be one of the causes, why the effects of the hemlock have not been such here, as we are assured they are at Vienna; viz. Dr. Storke says, that the root of hemlock, when cut into slices, pours forth a milky juice, which Dr. W. had never seen it do here in England.

There are several vegetables which, though they thrive apparently well, their productions are yet not the same as in other parts of the world, where the heat is more intense, and the summers are of longer continuance. It would be extremely difficult here, though the plants thrive very well, to produce from the white poppy, or Cistus ladanifera, either the opium or the labdanum, the known production of these vegetables in other parts of the world. No art can make here the tragacantha pour forth its gum, the lentiscus its mastic, or the candleberry myrtle of North America its sebaceous concrete. To these might be added many others, too tedious to mention.

In such mild winters as the last, the leaves of hemlock may be procured in any part of them; but they are not to be depended on, as their specific smell is then comparatively weak, their juices poor and watery, and they are wholly without spots.

XVI. Of an Anthelion observed near Oxford, by the Rev. John Swinton, B. D. of Christ-Church, Oxon. F. R. S. p. 94.

Near the top of Shotover-hill, about 12^m past 7 o'clock in the evening, Mr. S. accidentally discovered a luminous appearance, not much unlike the sun when seen through clouds, about 4 or 5 times as large as the solar disk. [See fig. 1. pl. 14.] The sun was then pretty resplendent, though a full exertion of its rays was somewhat obstructed by a thin waterish cloud. Soon after a very distinguishable Mock-Sun, opposite to the true one, which he took to have been an anthelion, appeared. This was not however completely formed, that part of its disk remotest from the sun being indistinct and but ill defined. Nor could the figure of the lucid tract round it, though approaching a circle, be with any precision ascertained. This uncommon meteor was seated in the E, but the sun had a westerly situation. From 7^h 12^m to 7^h 18^m the phenomenon shone very conspicuously, though almost surrounded by dark thickish clouds. The disk of the spurious sun seemed as large and bright as that of the true one, but was not so well de-

fined. Between 7^h 18^m and 7^h 28^m the meteor was more than once partially obscured, by the circumjacent clouds; a very thick black one, which had been visible from the moment he first perceived the phenomenon, then extending itself almost from the western limb or edge of it to the sun. From the beginning to the end of the mock-sun's appearance, about 18^m, there was much clear sky above the sun, even up to the zenith, and thick dusky clouds below it; but the tract both above and beneath the meteor was, for the most part, covered with such clouds. When in its most refulgent state, the anthelion was as yellow as the sun, but the lucid tract surrounding it was of a paler yellow, or whitish cast, interspersed with a few reddish and subfuscous spots. The whole, when least affected by the neighbouring clouds, seemed in extent to be 4 or 5 times the space occupied by the disk of the sun. In fine, the phenomenon was sometimes brighter, and sometimes more obscure; varying, through the whole course of its duration, according to the variation of the atmosphere and the clouds. At last, after several short successive intervals of brightness and partial obscurity, it was absorbed by the black cloud above-mentioned, nearly connecting it with the sun; and about 7^h 30^m totally disappeared.

Instances of anthelia are extremely rare. Mr. S. had hitherto been able to meet with only 2 of them, viz. that observed near Dantzic by Hevelius, Sep. 6th, N. S. 1661; and that seen at Wittemberg in Saxony, Jan. 18th, N. S. 1738, a description of which was soon after communicated to the Royal Society by J. Frid. Weidler, Professor of Mathematics there.

XVII. On a Production of Nature at Dunbar in Scotland, like that of the Giants-Causeway in Ireland. By the Right Rev. Richard Lord Bishop of Ossory, F.R.S. p. 98.

The passage into the harbour of Dunbar is very narrow, between two rocks: one of them is the east side of the harbour; the other is a promontory, stretching out about 100 yards to the north, and is about 20 yards wide, having the sea on each side of it, when the tide is in. This head is a most extraordinary natural curiosity: it is of a red stone, which is not a lime-stone, but appears rather like a very hard free-stone. It looks on both sides like the Giant's-causeway in Ireland: the stones on the west side are from a foot to 2 feet over; on the east side they are large, from 2 feet to 4 feet. The pillars from 3 to 8 sides; but only one or two of the first and last: they may be said to be in joints, but are strongly cemented together by a red and white sparry substance, which is formed in laminæ round the pillars, and between the joints, 2 or 3 inches in thickness. The interstices between the large pillars, which are but few, are filled with small pillars, without joints. The pillars consist of horizontal laminæ: the joints are not concave and convex when separated, but uneven and irregular: they lie sloping from

east to west: on the west side, towards the end, the pillars become very large and confused, as those to the east of the Giant's-causeway, and in the isle of Mull; except that these are divided by such a sparry substanee into a great number of small figures, whieh seem to go down through them. There are spots and veins of a whitish stone in the pillars. There is no sign of any thing of this kind in any of the rocks near, that he eould observe, or hear of.

XVIII. Of a remarkable Meteor seen at Oxford. By the Rev. John Swinton, B.D., of Christ-Church, Oxon. F.R.S. p. 99.

On Sunday, Sept. 21, 1760, from $6^h 49^m$ to $7^h 25^m$ P. M. sueh a meteor appeared at Oxford as Mr. S. had never seen before. [See fig. 2. pl. 14.] A dark cloud, like a pillar or column of thick black smoke, and perpendicular to the horizon, appeared in the N. W. pushing gradually forward towards the zenith, and at last extending itself almost to the opposite part of the heavens. It was at first several degrees broad, but grew broader and broader, as it approaehed the zenith; through which it passed, and nearly bisseeted the hemisphere, in a wonderful manner. At 7^h this surprising arch, falling little short of a semieirele, that would have resembled an iris, had not the colours of it been different, seemed to be completely formed. He says, "had not the colours of it been different;' because the lower part was exceedingly black, but the other subfuseus only and white. The exterior limb of this areh as far as the vertex was tinged with a pale yellow, that gave it no disagreeable appearance. The edges of it were at first tolerably smooth, and pretty well defined, but afterwards beeame rugged and irregular. The whole moved with the wind, from the first to the last moment of its existenee. For a few minutes, it rendered the moon absolutely invisible. That planet had, for a considerable time before its approach, been somewhat darkened by the thick hazy air; which however did not totally obscure it. The tract near the northern part of the horizon, contiguous to the meteor, was interspersed with fuscous ealiginous elouds, and that near the zenith with some of a whitish colour. All of them were very distinguishable from the phenomenon itself. They beeame gradually paler and paler, till they were entirely dispersed. About $7^h 25^m$ P. M. all remains of the meteor were so perfectly dissipated, that not the faintest traces of them were to be seen. That this phenomenon was a water-spout, or rather the first appearance of one, though the proper spout itself was not visible, will perhaps not be denied, Mr. S. says, by any person moderately versed in natural history. The weather was mild, or rather warm, the whole day. The wind, during the eontinuance of the phenomenon, and almost ever since, was W. S. W. though it did not then exceed a very gentle gale. Indeed the weather for 3 months before was, with very little intermission, hot and exceedingly dry, such as generally precedes meteors of this kind. As the phe-

nomenon was seen by the Rev. Dr. Neve, Fellow of Corpus-Christi College, at Middleton-Stoney, 12 miles from hence, and at Sandford, N. W. of that village, a few miles farther from this place, at the very time that Mr. S. observed it, and attended by circumstances nearly the same with those that occurred to him; it must have been of a pretty considerable height. Mr. S. adds, that a most terrible storm of rain and hail followed it, which continued from a little past 3 to near 5 o'clock the next morning; and that they had much of such stormy weather there, and in the neighbourhood of that city long after.

XIX. On some Productions of Nature in Scotland resembling the Giant's-causeway in Ireland. By Emanuel Mendez Dacosta, F.R.S. p. 103.

Mr. Murdoch Mackenzie, who, by order of the Lords of the Admiralty, surveyed the coasts of Scotland, communicated to Mr. D. the following. In Cana island, which is 4 English miles long, to the southward of Skye, and near the island of Rum, the rocks, about a quarter of a mile above the harbour, rise into polygon pillars southward. About 2 miles from the west end of Cana, is a low rock, or small island, where is a very regular pavement of hexagon stones, each about a foot deep, and about 9 inches over. They form a smooth uniform pavement; and the sides of all the stones lie extremely contiguous, or close. Immediately below this upper pavement, lies another exactly like it. The pillars are jointed exactly like those of the Giant's-causeway, and are laid with their concavities downward, and their convexities upward; and their hollows are as much in proportion to these pillars, which are smaller, as they are in those of the Giant's-causeway. These places are about 200 miles northward distant from the Giant's-causeway.

XX. Elements of New Tables of the Motions of Jupiter's Satellites. By Mr. Richard Dunthorne. Dated Cambridge, March 3, 1761. p. 105.

The public employment*, wherein I am at present, and for several years past have been engaged, not permitting me to make new tables of the motions of Jupiter's satellites, according to the last corrections I had (from a comparison of more than 800 observations) made in the places and orbits of those planets, I am at last persuaded to communicate to the Royal Society, the elements of those tables, hoping they will prove no unacceptable present to astronomers.

The tables are designed on the plan of those of Mr. Pound for the first satellite, published in the Philos. Trans. N° 361; except that I have not deducted the greatest equations from the epochs, as is done by Mr Pound. The epochs of the conjunctions of the several satellites with Jupiter, fitted to the Julian year (before

* That of Surveyor to the Corporation of the great level of the fens.—Orig.

the alteration of the style in England), and to the meridian of the Royal Observatory at Greenwich, are as follow:

Jul. years current.	Conj. 1st sat. D. H.	Num. A.	Num. B.	Num. C.	Conj. 2d. sat. D. H.	Conj. 3d. sat. D. H.	Conj. 4th. sat. D. H.
1728	0 21 58′ 16″	630	651	484	0 21 20′ 0″	6 4 57′ 0″	2 3 25′ 0″
1748	0 3 7 18	316	962	175	3 2 32 24	3 18 7 54	1 16 37 41
1768	1 2 44 57	2	278	869	1 18 26 54	1 7 18 48	1 5 50 22

Number C is the period of 437 days, in which the 3 innermost satellites return very nearly to the same situation in respect of one another, and of Jupiter's shadow, in millesimals of a circle; and must be corrected by the equation of number B, under a contrary title. The second satellite has a synodical equation of 16′ or 17′ in time, whose revolution is, in this period, to be subtracted, if number C be less than 500; added, if greater. The first and third satellites have also small synodical equations, returning in the same period, that of the first satellite being about 3′, of the third about 2′ in time; both to be added, if number C be less than 500; subtracted, if greater. The orbit of the third satellite is manifestly eccentric, as well as that of the 4th. Its apojovium in 1728 was about 10° of ♈, and moves forward 35° in 20 years: its greatest equation is about 15′ in the satellite's orbit, or 7′ in time. The apojovium of the fourth satellite in 1728, was in 12° 30′ of ♓, and moves forward about 12° in 20 years: its greatest equation is 53′ in the satellite's orbit, or 59′ in time.

I found no reason to make any alteration in the semi-durations of the eclipses of the first satellite from Mr. Pound's tables.

The greatest semi-durations of the eclipses of the 2d, 3d, and 4th satellites in the nodes, are $1^h\ 27^m$, $1^h\ 47^m$, and $2^h\ 24^m$* respectively. The nodes of the 2d satellite seem to be at rest in about 50° of ♒ and ♌; but the inclination of its orbit varies from 2° 50′ to 3° 52′: it was least in 1668, greatest in 1715, and seems to have been at its greatest and least once in the intermediate years. I suppose it at the least in 1730. The nodes of the 3d satellite in 1727, were in $16\frac{1}{4}$° of ♒ and ♌, and move forward about $2\frac{1}{4}$° in 20 years: the inclination of its orbit in 1695 was 3°, and has been increasing ever since: it seems as if it would get to its maximum about 1765, and would then be about 3° 24′. The nodes of the 4th satellite in 1730 were in $13\frac{1}{2}$° of ♒ and ♌, and move forward 2° in 12 years: the inclination of its orbit is about 2° 40′, and does not seem to vary above one or two minutes either way. From these elements it will be easy

* The semi-durations of the eclipses of the 4th satellite will be about 2′ more at the ascending, and 2′ less at the descending node, on account of the eccentricity of its orbit.—Orig.

for any person moderately skilled in such matters to construct tables of the motions of the satellites in the method of Mr. Pound, which may be seen in the latter part of Halley's tables.

XXI. Dissertationem hanc de Zoophytis, Regiæ Societati Scientiarum Angliæ legendam et judicandam præbet Job Baster, M.D., &c. p. 108.

Dr. Baster's notions relative to corals and corallines being now entirely exploded, it is unnecessary to insert the present dissertation. We have therefore only preserved the part relative to the figures represented on the accompanying plate.

Fig. 6, pl. 14, shows a branch of a zoophyte called corallina abietis forma. About this branch, which was kept from Sept. 1758 to February 1759, was collected a kind of bark or concretion of sordes. For at least five months fresh water was supplied once a day, the old being thrown away; and although it had not grown much, yet it emitted small lateral branches in various parts which were all beset with polypes.

Fig. 7 shows the same branch viewed by a lens. The letters of reference are the same in both figures; viz. A, the trunk of the sertularia, which was seated on an oyster-shell; B, some lateral branches, which were here and there produced while he kept the coralline, and which were from the beginning beset with polypes; C, the tip of a branch, recently produced, and perfectly free both from sordes and polypes; D, a larger kind of polypes, being the beginnings of the zoophyte called corallina tubularia; E, a very small species of polype shown in a magnified state in the first part of Dr. B.'s work called Opuscula Subseciva, at tab. 3. f. A, B, C. Fig. 8, the summit C of the two former figures viewed microscopically. A, the place where it has been pulled from the trunk; B, two lateral branches with polypes proceeding from them, as if from cells, and expanding their arms at d; e, cellules, in which the polypes, when their arms are contracted, entirely hide themselves, appearing at that time like white spots.

XXII. Of an Uncommon Phenomenon in Dorsetshire. By John Stephens, M.A. p. 119.

In August 1751, the air, having been for some time remarkably hot and dry, was changed of a sudden by a heavy fall of rain, and a high south-west wind; the cliffs near Charmouth, in the western part of Dorsetshire, presently after this alteration of the atmosphere, began to smoke, and soon after they burned, with a visible though a subtle flame for several days successively; and continued to smoke, and sometimes to burn at intervals, till the approach of winter: nay, ever since that time, especially after any great fall of rain, thunder and lightning, or a high south-west wind (which drives the sea with great violence against the

cliffs, and beats off large pieces of them) the cliffs continue to smoke, and sometimes to burn with a visible flame; which during the summer-months is frequently observed in the night-time. On examining these cliffs, in the year 1759, Mr. S. discovered a great quantity of pyrites, not in any regular strata, but interspersed in large masses through the earth, and which proved to be martial; of marcasites, which yielded near one tenth part of common sulphur; of cornua ammonis of different sizes, and other shells, but of the bivalve class, which were crusted over, and as it were mineralized with the pyritical matter; of belemnites, also crusted over with the like substance: and the cliffs, for near 2 miles long, and from the surface, to 35 or 40 feet deep, even to the rocks at high water mark, were one bed of a dark coloured loam, strongly charged with bitumen. He found also a dark coloured substance, resembling coal-cinder; some of which being powdered, and washed in distilled rain water, on filtrating the water, and evaporating it slowly to a pellicule, its salts shoot into fine crystals, and appear to be no more than a martial vitriol: one ounce of this cinder-like substance yields one drachm of salt. He gathered up about 100lb. weight of the different kinds of those pyritæ, marcasites, &c. which were laid in a heap, exposed to the air, and every day sprinkled with water: the consequence was, that in about 10 days time they grew hot, soon after caught fire, burned for several hours, and fell into dust. Hence therefore it is imagined that these martial and sulphureous fossils, by being exposed to the air and wet, by being agitated by the beating of the sea, and by being electrified as it were by the subtle flame of the lightning, take fire, which is favoured by the bituminous particles contained in the loam, and burn till all their phlogiston is consumed, and their iron or martial earth is dissolved in the acid of sulphur; which constitutes the martial vitriol, found to be near the one-eighth part of this cinder-like matter.

When the cliffs were observed to burn in the night-time, the flame was plainly perceived by a spectator at a distance; but when he drew near to the place seemingly on fire, he could perceive a smoke, but no flame. In the day-time nothing but a smoke was perceived, except the sun shined, when the cliffs appeared at a distance as if they were covered with pieces of glass, which reflected the sun's meridional rays; but on drawing near to the places where these luminous appearances were perceived, they disappeared, and the cliffs seemed to be covered with smoke, which stunk of a bituminous and sulphureous matter.

Mr. S. had also been an eye witness of the same kind of flame arising from the Lodes in Cornwall, especially such as contained a great quantity of mundic and martial pyrites. Three times he had seen this flame arise from the earth in the night, and once in the middle of the day. In the night a person standing at a little distance, would imagine that the place was all on fire, and even on drawing near the same he perceives himself surrounded with flame, but is not

6

hurt; and in 4 or 5 minutes time, he perceives this flame to decrease, and fall into the earth. In the day-time the flame is of a different colour, and not much unlike the flame which arises from a furnace. There are several mines discovered in this county by these mineral fires, where there were no symptoms of such mines before: but it is generally observed that they abound with marcasite and pyrites. These mineral flames, arising from ignited pyrites, are frequently discovered in the bottom of mines and coal-pits; and are often detrimental, and sometimes destructive to the miners; which made the late Dr. Woodward and others imagine that they were vapours arising from an abyss.

From what has been said Mr. S. draws the following conclusions.

1. That all subterraneous fires, even those of Hecla, Vesuvius, and Ætna, together with those observed in the mines and coal-pits, are caused by the heat and fixing of pyrites and marcasites. 2. That the waters of our hot baths derive their heat from passing over a bed of ignited pyrites. Indeed the solid contents of those waters do evidently prove this assertion, being nothing more than such particles of the pyrites as are soluble in water. 3. That these mineral flames will be more or less subtle, according to the minuteness of the particles of the combustible matter, and the quantity of phlogiston which they contain. 4. That the convulsive motions and tremblings of the earth are caused by the heat of the burning pyrites expanding the air contained in its bowels. This is clearly proved by their causing, immediately after, an eruption of the earth, which generally discharges a dark coloured, cinder-like and frothy matter. And, 5. That those places, where the earth contains the greatest quantity of pyrites and marcasites, will be most liable to these convulsive motions and tremblings, no other natural cause contradictory.

XXIII. Additional Observations on some Plates of White Glass found at Herculaneum, and on the Use of Glass in Windows. By J. Nixon, A. M., F. R. S. p. 123.

In a paper which Mr. N. presented to the Society about 2 years before, he offered his thoughts on some plates of white glass found in the ruins of Herculaneum. He now adds some more observations, with a view partly to explain and support what he then delivered, and partly to communicate such new informations as he has since received relating to the same subject.

Mr. N. observed, on the authorities produced by Mons. Renaudot, that glass plates were not applied for magnifying objects in optical experiments, till the beginning of the 13th century: but on reviewing his dissertation, Mr. N. finds he sinks the antiquity of that usage a century lower than this. Mons. Renaudot

observes that Mabillon mentions a manuscript he saw in an abbey in the diocese of Freisingen, where Ptolemy was represented observing the stars with a tube, like our modern perspective glasses. This manuscript is said to have been written in the beginning of the 13th century. Mabillon does not mention that the tube had glasses; neither indeed was that circumstance easily discoverable. Perhaps such tubes were then used only to preserve and direct the sight, or to render it more distinct, by singling out the particular object looked at, and shutting out all the rays reflected from others, whose proximity might have rendered the image less precise.

As it appears that neither the lapis specularis nor glass was used for windows before Seneca's time; and it cannot be supposed that the Romans, a people of so refined a taste in other instances, would suffer their apartments to be exposed to the free entrance of winds, &c. it may be reasonably asked, what supplied the place of those materials before? To satisfy this inquiry, it is to be observed that several other materials are mentioned by ancient writers, as serving the purpose before us; such as thin hides or skins, like our parchment, mentioned by Philoponus. Pliny likewise informs us that the horns of the urus being cut into thin laminæ, were transparent, and supplied in some measure the use of our lanterns; and we may probably conclude, from the analogy of things, that they served for window-lights also; especially as we meet with windows made of horn (corneum specular) in Tertullian, who wrote within less than 200 years after Pliny. To these may be added the vela, made of hair-cloth, or pieces of hides, which Pitiscus, on the authority of Ulpian, says were in use before the invention of windows of the lapis specularis or glass.

Mr. N. took notice of the natural connection there seemed to subsist between the using of plates of glass for adorning the inside of apartments in ancient times, and the employing them for introducing light into those apartments. This observation has been supported by a letter he received from the Abbate Venuti at Rome, dated December 30, 1759, wherein he informs him that he had lately read in some anecdotes of Cardinal Maximi, ‘That as they were digging among the ruins on mount Cælius in the last century, they found a room belonging to an antique dwelling-house, that had all its sides within ornamented with plates of glass, some of them tinged with various colours, others of their own natural hue, which was dusky, occasioned by the thickness of the mass of which they consisted. There were likewise in the same apartment window-frames composed of marble, and glazed with laminæ of glass.’

Presuming the evidence to be undeniable which Mr. N. has produced in his dissertation, to prove the use of glass in windows to have been as early as the 3d century, if not before, he deems it not unacceptable to the curious in antiquity to observe the slow progress this very commodious invention made towards the

west; since it appears, that it did not reach our island till the 7th century; when it was brought hither from France, either by Benedict abbot of Winal, or Wilfrid archbishop of York; as lanterns of horn were introduced by King Alfred about the same time, viz. 680.

XXIV. A Description of the Cepphus. By D. Lysons, M. D.* p. 135.

This bird is the cepphus of Aldrovandus, though it does not agree in all points: perhaps that which he saw might be a male, this a female. In his the sides of the mandibles were of a dusky red, in this not. The eyes of his were partly red, which was not observed in this. Round the eyes in his was a whitish circle, in this a variegated semicircle. The legs and shanks in his greenish, in this of a dilute blue. The feet, and membranes connecting the toes, in his were dusky, in this partly black.

XXV. Extract from the Register of the Parish of Holy Cross in Salop, from Michaelmas 1750 *to Michaelmas* 1760. *Communicated by Robert More, Esq., F. R. S.* p. 140.

Baptized, males 168; females 163; in all 331. Buried, males 137; females 153; in all 290.

XXVI. An Account of the Earthquake at Lisbon, March 31, 1761*: in a Letter from thence, dated April* 2, 1761. p. 141.

This earthquake happened the 31st, precisely at 12 o'clock, and lasted full 5 minutes, with a smart and equal vibration. It exceeded all the others, except that of Nov. 1, 1755. It was attended with no other consequences but that of alarming the inhabitants, throwing down some ruins, and rending some houses. About an hour and a quarter afterwards, the sea began to flow and ebb, about 8 feet perpendicular, every 6 minutes, and continued till night. Some small shocks were felt before and after, but of no moment.

XXVII. Another Account of the same Earthquake; in a Letter from Mr. Molloy, dated there April 3, 1761. p. 142.

In this letter there are but few additional circumstances. The shock seemed to Mr. M. to spring from the bowels of the earth, and the motion to be directly up and down. It is the general opinion that if it had run from west to east, or from any quarter of the globe to the other, as the great one the 1st of November 1755 did, there would not have been a house left standing in this unfortunate

* This species is the *larus crepidatus* of the Gmelinian edition of the Systema Naturæ, and the *black-toed gull* of Pennant and Latham. Its specific character is thus given by Latham. *Larus luteo fuscoque varius subtus pallidior, macula alarum alba.*

place, as all the gentlemen that reside here say, it was more severe and constant for the time than the former. Many buildings have tumbled down, but few people were killed. The earth groaned in so dreadful a manner, that we expected every moment it would open, and swallow this place and all its inhabitants. We have had several slight shocks since, and one this morning about 2 o'clock, which was very severe; our house shook like a bull-rush. There was another more slight about 5.

XXVIII. A Further Account of the Case of William Carey, whose Muscles began to be Ossified. By the Rev. Wm. Henry, D. D., F. R. S. p. 143.

In March 1759, I had this young man brought up to Dublin, and admitted into Mercer's hospital. The physicians and surgeons put him under a salivation; and afterwards applied to his arms and joints mercurial plasters. The good effects of this process, was the drying up the great discharge of humour, which he had at his elbows and wrists, and an immediate check to the progress of the ossification. In June following he was discharged from the hospital, being furnished with mercurial plasters and directions. By the advice of the physicians, he went to his own place near Ballyshanon, on the western ocean; and there, in pursuance of their directions, bathed in the ocean twice a day, during that whole summer and harvest, and constantly rubbed his whole body and limbs over with the juice of the quercus marina, immediately after coming out of the sea. In consequence of this course, he happily exchanged his ghastly hectic countenance for a healthy and athletic complexion, which continued till March 1760.

About this time his cough returned, his sores began to run, and the ossification to return. In this distress he came to me to Dublin. With some difficulty I got him admitted again into Mercer's hospital, where he continued for some months, and was again treated with mercurial medicines and applications as before. After being discharged, he returned to his former course of bathing in the ocean, and anointing his body with the quercus marina. This process restored his health, and entirely stopped the progress of the ossification. He also recovered the use of some of the ossified joints, particularly of his wrists and fingers; and his knees and legs were so relaxed by the dissolution of the callus, that he was able to walk 20 miles in a day.

I feared that his disorder might return this spring, as it did in 1760, but it has not returned. That I might be the better certified, I wrote to Sir James Caldwell. The answer I received was, that he had been a few days ago at Castlecaldwell, and found himself so well and strong, as to importune Sir James to admit him into his body of the Enniskillen lighthorse. The poor man thinks the ossification entirely stopped; yet by the appearance of his arms and wrists he seems to be mistaken. The first hardness still continues, and all the muscles

from his elbow to the wrist seem to be one solid bone. It is very happy for him that it has been hitherto stopped from proceeding any farther; and that from his present state of good health, there is reason to hope that it will not increase.

XXIX. Description of a new Thermometer and Barometer. By Keane Fitzgerald, Esq., F. R. S. p. 146.

Mr. F. lately communicated a paper, with an account of an instrument, intended to answer in some measure the purposes of a thermometer and pyrometer. The degrees the index had pointed to, during the absence of an observer, were marked by a pencil applied to it. But he found great inconvenience from the friction of the pencil, which must be strong, or it does not mark distinctly; besides the trouble of rubbing out the mark, every time a new observation was intended. He now offers the description of an instrument on the same principle as a thermometer only, with registers to mark the least variation that can happen during the absence of an observer; but too complex to be useful.

XXX. Of the Earthquake felt in the Island of Madeira, March* 31, 1761. *By Thomas Heberden, M. D., F. R. S.* p. 155.

In the city of Funchal, on the island of Madeira, March 31, 1761, they were alarmed with the shock of an earthquake, preceded by the usual noise in the atmosphere, like heavy carriages passing hastily over rough pavements. It began at 35 minutes after 11 o'clock in the morning, and lasted, by the watch, full 3 minutes; the vibrations, which were very quick, remitting and increasing twice very sensibly during the shocks, which seemed to be progressive, from east to west. It has separated some rocks in the eastern part of the island, which have fallen from the cliffs into the sea. It has likewise damaged the walls of several buildings; particularly such walls as stand in a direction north and south.

During the earthquake the fountain of the city, the water of which is very clear at other times, ran turbid and whitish. The sea was agitated very sensibly, fluctuating several times between high-water and low-water mark. The fluctuation of the sea continued longer in the eastern parts of the island than in this part. The sun, which shone very bright before, immediately after the earthquake was surrounded by a very large halo, which lasted about an hour, and gradually disappeared.

XXXI. An Account of a Treatise in Latin, presented to the Royal Society, entitled De Admirando Frigore Artificiali, quo Mercurius est Congelatus, Dis-

* See the account of the same earthquake felt at Lisbon, p. 541.

sertatio, &c. a J. A. Braunio, Academiæ Scientiarum Membro, &c. By Wm. Watson, M. D., R. S. S. p. 156.

Very early last year, we were informed, says Dr. W., that at Petersburg, by the means of artificial cold, the mercury in thermometers had been condensed to so great a degree as to become perfectly fixed and solid; but as this information was received only in a loose way from the public gazettes, the opinions of philosophers here were suspended, in regard to their giving credit to this very extraordinary phenomenon, till the truth of it could be sufficiently authenticated. This has lately been done by professor Braun, who first made the experiments, and who presented an account of them to the Royal Academy at Petersburg.

Professor Braun observes, that every age has its inventions, and that the discovery of some things seem to be reserved for particular persons. To this, the history of sciences in all ages, more particularly of the late and the present, bears witness sufficiently, by the invention of the air-pump, barometers, thermometers, optical instruments, electricity, more particularly the natural, artificial magnets, phosphorus, the discovery of the aberration of light, and of many other things in natural philosophy. He does not know whether the congelation of mercury, which it was his good fortune to discover, may not be ranged among these: for who did not consider quicksilver as a body which would preserve its fluidity in every degree of cold? Neither was the fact otherwise, if this is understood of natural cold, such as it has been found in any part of the globe hitherto discovered. But if it should happen that the natural cold should ever be so intense as artificial cold has been found to be, the whole globe would have a different face, as men, animals, and plants, would certainly be destroyed. He hinted some time since, in a dissertation on the degrees of heat which certain liquors and certain fluids would bear before they boiled, and the degrees of cold they respectively bore, before they were converted into ice, that there was a suspicion, that the mercury in some of the barometers and thermometers made use of for experiments in Siberia, had been frozen; but since that in greater degrees of cold, the mercury continued fluid in other barometers and thermometers, the immobility and hardness observed in some of these instruments was attributed more probably to the lead of the bismuth, with which the mercury, had been adulterated, and was not considered as a real freezing of the mercury, but this has been since put out of all doubt, since it is certain, that pure mercury would not freeze under such small degrees of cold, great as they were for natural cold. The experiments which the Professor made, in order to congeal mercury, demonstrate this most evidently, besides which they exhibit new phenomena.

There happened at Petersburgh, on the 14th of December 1759, a very great frost, equal, if not more intense than any which had been observed there: for between 9 and 10 o'clock in the morning, Delisle's thermometer stood at 205;

at 7 o'clock at 201; which last was the greatest degree of cold that had been observed at Petersburg either by himself or others. At 1 o'clock at noon, the thermometer stood at 197. Mr. Braun had been employed several days before this, in observing the several degrees of cold which different fluids would bear, before they were converted into ice; partly to confirm those things which he had already laid before the academy, and partly to make experiments on liquors which had not yet been examined; as on the days between the 7th and 14th the cold was intense enough to be between the degrees of 181 and 191. When the natural cold was so intense as to be at 205, Professor Braun conjectured, that it was of all others the most proper occasion to try the effects of artificial cold, not doubting but that artificial cold would be increased in proportion as the natural was more intense. Aquafortis, which was found by the thermometer to be 204 degrees cold, was the greatest part of it frozen, the ice having the appearance of crystals of nitre, which however immediately dissolved in a small degree of heat. This aquafortis which, though frozen at the sides, was liquid in the middle, was poured on pounded ice, in that proportion which was directed by Fahrenheit, the first person who made artificial cold with spirit of nitre. But before the Professor made this experiment, he by examination found, that both the ice and aquafortis were of the temperature with the air, which was then 204. On the first pouring, the mercury fell 20 degrees; this spirit was poured off, and fresh put on several times; but it was possible by these means to introduce no more than 30 degrees of cold: so that the mercury in the thermometer fell no lower than 234. Since therefore Fahrenheit could not produce cold greater than that of 40 below the cypher of his thermometer, which corresponds with 210 of that employed by Professor Braun; nor Reaumur, nor Muschenbroek, who often repeated the same experiment, our author was on the point of giving up this pursuit, as considering this as the greatest degree to which artificial cold could be carried; thinking it sufficient honour to himself to have added 20 degrees to the cold formerly known.

But reflecting that this was not all the fruit he expected from these experiments, he determined to pursue them; but at the same time however to vary the manner of them. By good fortune his ice was all gone, and he was compelled to use snow in its stead, after having first tried, and found the snow of the same degree of cold with the air, at this time 203. The snow, the thermometer, and the aquafortis, being of the same temperature, he immersed the thermometer in snow contained in a glass; and at first only poured a few drops of the aquafortis on that part of the snow in which the thermometer was immersed: on which he observed the mercury to subside to 260. Elated by this remarkable success, he immediately conceived hopes that these experiments might be carried further: nor was he deceived in his expectations; for repeating the ex-

periment in the same simple manner, he poured on only some more aquafortis, and immediately the mercury fell to 380. On which he immersed the thermometer in another glass filled with snow, before it had lost any of this acquired cold; and at length by this 3d experiment the mercury subsided to 470 degrees. When he observed this enormous degree of cold he could scarcely give credit to his eyes, and believed his thermometer broken. But to his infinite satisfaction, on taking out his thermometer, he found it whole: though the mercury was immoveable, and continued so in the open air 12 minutes. He carried his thermometer into a chamber, where the temperature of the air was 125 degrees; and after some minutes the mercury, being restored to its fluidity, began to rise. But to be certain whether this thermometer had received any injury, and whether it would yet correspond with his thermometer which he keeps as a standard, he suspended them together, and in 20 minutes the thermometers corresponded one with the other.

The thermometers which our author usually employs have a spherical bulb, and their scale is divided into 1200 parts, of which 600 are above the cypher, which denotes the heat of boiling water, and 600 below that heat. A thermometer of this construction was used in investigating the heat of boiling mercury and oils. He had another thermometer, of which the scale went no lower than 360 degrees below the cypher, denoting the heat of boiling water. He repeated the former experiment with this, and the mercury very soon descended so that the whole was contained in the bulb, which however it did not quite fill. The mercury in this bulb was immoveable, even though he shook the thermometer: till after about a quarter of an hour, it began to ascend in the open air; and it continued to ascend till it became higher than the circumambient air seemed to indicate. He was struck with this extraordinary phenomenon, and very attentively looked at the mercury in this thermometer, and found certain air bubbles interspersed with the mercury, which were not in that of the other thermometer. From these, and other experiments, he was satisfied that the mercury in these thermometers had been fixed and congealed by the cold.

Hitherto our Professor had only seen the mercury fixed within the bulb of his thermometers. These he was unwilling to break. He was however desirous of examining the mercury in its fixed state, and therefore determined to break his thermometers in the next experiments. It was several days before he got other thermometers which exactly corresponded with those he had already employed. When these were procured the natural cold had somewhat relented. In the former experiment the thermometer stood at 204: it was now at 199. In making the experiment he varied the manner a little. He first put the bulb of the thermometer into a glass of snow, gently pressed down, before he poured on the aquafortis: he then, in another glass, poured the aquafortis on the snow;

before he immersed his thermometer in it: he then, in like manner, put the snow to the aquafortis, before he put his thermometer therein. In whichever of these ways he proceeded, he found the event exactly the same; as the whole depended on the aquafortis dissolving the snow. When he had proceeded so far as to find the mercury immoveable, he broke the bulb of the thermometer, which had already been cracked in the experiment, but the parts were not separated. He found the mercury solid, but not wholly so, as the middle part of the sphere was not yet fixed. The external convex surface of the mercury was perfectly smooth: but the internal concave one, after the small portion of mercury which remained fluid, was poured out, appeared rough and uneven, as though composed of small globules. He gave the mercury several strokes with the pestle of a mortar which stood near him. It had solidity enough to bear extension with these strokes ; its hardness was like that of lead, though somewhat softer; and on striking it sounded like lead. When the mercury was extended by these strokes, he cut it easily with a penknife. The mercury then becoming softer by degrees, in about 12 minutes it recovered its former fluidity, the air being then 197. The colour of the congealed mercury scarcely differed from that of the fluid: it looked like the most polished silver, as well in its convex part as where it was cut.

The next day the cold had increased to 212 degrees, which was 7 degrees beyond what it had ever before been observed at Petersburg. The season so much favouring, he thought it right to continue his pursuit, not only in further confirmation of what he had already observed, but to investigate new phenomena. In two thermometers he observed the same facts in regard to the congealing of mercury, as he did the preceding day. In the bulbs which he broke, the whole of the mercury was not fixed, as a very small portion, much less than that of the preceding day, continued fluid. He treated this mercury as he did the former: he beat it with a pestle, he cut it, and every thing was thus far the same. But he saw a very great difference in respect to the descending of the mercury in the thermometer, the like of which did not occur to him, neither in the former nor any of the subsequent experiments. From the former ones it appeared that the mercury in the first experiment had only descended to 470 when it became immoveable, though the glass bulb was not cracked. In the experiment of the 25th it descended to 530; and in two thermometers on the 26th, to 650. But as well in the thermometer which he used on the 25th, as in two of the 26th, the bulbs were cracked in the experiment: they cohered however; nor was the least part of the bulb separated, but the congealed mercury seemed to adhere to all parts of the bulb. In the following experiments, he invariably found that the mercury sunk lower, if the whole of it was congealed, than if any part of it remained fluid. It then generally descended to 680 and 700, but the bulbs

were never without cracks; moreover, it descended to 800, and beyond even to 1500; but in this last experiment the bulb was quite broken, so that the globe of mercury, thoroughly frozen, fell out, and by its fall, of about 3 feet, the globe of mercury became a little compressed, but in the former only some parts of the bulb fell off.

Mr. Braun always found that, cæteris paribus, the more intense the natural cold was, the more easy and more expeditiously these experiments succeeded. In continuing them he observed, that double aquafortis was more effectual than simple spirit of nitre; but that if both the aquafortis and Glauber's spirit of nitre, which he sometimes also used, were well prepared, the difference was not very considerable. When his aquafortis was frozen, which often happened, he found the same effects from the frozen parts, when thawed, as from that part of it which remained fluid in the middle of the bottle. Simple spirit of nitre, though it seldom brought the mercury lower than 300 degrees, by the following method he even froze mercury with it. He filled six glasses with snow, as usual, and put the thermometer in one of them, pouring on it the spirit of nitre. When the mercury would fall no lower in this, he in the same manner put it in a 2d, then in a 3d, and so in a 4th; in which 4th immersion the mercury was congealed. Another very considerable difference presented itself in pursuing these inquiries, with regard to the mode of descent of the mercury. He constantly and invariably observed, that the mercury descended at first gently, but afterwards very rapidly. But the point at which this impetus begins is not easy to ascertain; as in different experiments it begins very differently, sometimes at about 300, at other times about 350, and even further. In the experiment before-mentioned, in which the mercury fell to 800, it proceeded very regularly to 600: about which point it began to descend, with very great swiftness, and the bulb of the thermometer was broken. The mercury however was perfectly congealed. He frequently observed another remarkable phenomenon, which was, that though the spirit of nitre, the snow, and the mercury in the thermometer, were previously reduced to the same temperature, on pouring the spirit of nitre on the snow, the mercury in the thermometer rose. But as this did not always happen, he carefully attended to every circumstance: from which it appeared, that this effect arose from his pouring the aquafortis immediately on the bulb of the thermometer, not previously well immersed in the snow. He also observed another effect, twice only: and this was, that after the thermometer had been taken out of the snow and aquafortis, the mercury continued to subside, in the open air, down as far as the congealation of mercury.

In the course of these inquires, our professor found no difference, whether he made use of long or short thermometers; whether the tubes were made of the Bohemian, or the glass of Petersburg. Under the same circumstances, the same

effects were always produced, making an allowance for the different contraction of the different glasses, under so severe a degree of cold. But if these tubes were filled with different mercury, there was then a sensible difference; inasmuch as mercury revived from sublimate did not subside so fast in the thermometer as that did which was less pure. He has even found, that he has been able to congeal the less pure mercury, at a time when he could not bring the revived mercury lower than 300 degrees: but this he would, till further trials have been made, not have considered as a general axiom.

From these experiments our author conceives it demonstrated, that heat alone is the cause of the fluidity of mercury, as it is that of water and other fluids. If therefore any part of the world does exist, in which so great a degree of cold prevails, as to make mercury solid, there is no doubt but that mercury ought to appear there as a body equally firm and consistent, as the rest of the metals do here; that mercury, on congealing, becomes its own ice, however different the mercurial ice may be from that of water, or other liquids. The idea of freezing does or can comprehend nothing more than a transition of bodies from a state of fluidity to that of firmness, by the sole interposition of cold. The ice of oily and saline bodies differs greatly from that of water, which is friable and easily broken, whereas that of mercury is ductile. And M. Braun proceeds to consider all bodies, which liquify by heat, as so many species of ice; so that every metal, wax, tallow, and glass, comes within his view in this respect.

Mercury then is, in its natural state, a solid metal; but is fusible in a very small degree of heat. Every metal begins to flow in a certain degree of heat; but this degree is different in different metals. Pure tin begins to run at 420; lead, at 530; and bismuth, at 470, in Fahrenheit's thermometer: or, according to our author, lead liquifies at 320 above the cypher in his scale, which corresponds with 596 in Fahrenheit; lead at 170=416 of Fahrenheit; bismuth at 235=494; zinc requires a greater heat to melt it than will make mercury boil. Now, if it could be settled, at what point mercury would begin to be congealed, we should know the point at which it began to flow; as it has been long known, that water is either fluid or solid, as the heat of it is a very few degrees above or under 32 in Fahrenheit's thermometer. Just so metals become solid at almost the same degree of heat in which they become fluid. But in mercury, the congealing point is at too great a latitude to be exactly determined: but our author estimates it to be about 469 degrees in his thermometer; at a less degree than which, he has not been able to observe the slightest congealation. Hence it follows, that the condensation or contraction, and consequently the diminution of the volume of mercury, must be very great indeed. This is demonstrated by the great descent of the mercury in the thermometer while it is freezing. But how great this diminution of the volume of the mercury is, cannot exactly be deter

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mined; and hence arises no small difficulty in determining its specific gravity, as this last must increase, as the bulk of the mercury lessens. Hence as mercury, even in its fluid state, comes of all bodies, platina excepted, the nearest to gold; in its solid state, it must still approach much nearer.

Our author had 3 thermometers filled with the most highly rectified spirit of wine. These not only corresponded exactly with each other, but, in less severe trials, corresponded reasonably well with those filled with mercury. But by the mixture of snow and spirit of nitre, which froze the mercury, he never was able to bring the spirit thermometers lower than 300. Hence it appears, that the heat which will freeze mercury, will not freeze spirit of wine; and that therefore spirit thermometers are the most fit to determine the degree of coldness in frigorific mixtures, until we are in a situation to construct solid metallic thermometers with sufficient accuracy. He made many experiments to try the effects of different fluids in his frigorific mixtures. He invariably found, that Glauber's spirit of nitre and double aquafortis were the most powerful. With oil of vitriol, the most onderous of all acids, he was never able to congeal mercury. He likewise tried a great number of other fluids, both acid and spirituous, which though, when mixed with snow, produced cold, it was in very different degrees. He tried a series of experiments to this purpose; but it was in weather far less cold than the preceding experiments were tried in, viz. between 159 and 153, by his thermometer. By these it appears, that spirit of salt pounded on snow, increased the natural cold 30 degrees; spirit of sal ammoniac, 10; oil of vitriol, 35; Glauber's spirit of nitre, 58; aquafortis, 40; simple spirit of nitre, 30; spirit of vinegar, and lemon juice, made no remarkable difference; dulcified spirit of vitriol, 20; Huffman's liquor anodynus, 32; spirit of hartshorn, 10; spirit of sulphur, 10; spirit of wine rectified, 20; camphorated spirit, 15; French brandy, 12; and even several kinds of wine, increased the natural cold to 6, 7, or 8 degrees. That inflammable spirits should produce cold, seems very extraordinary, as rectified spirit seems to be liquid fire itself; and what still appears more paradoxical is, that inflammable spirits poured into water, cause heat; upon snow, cold: and what is water but melted snow?

Though not immediately relating to the principal purpose of this treatise, our author measured by his thermometer, when it stood in his study at 128 degrees, the heat occasioned by pouring different fluids into water. He found, that oil of vitriol produced 35 degrees; spirit of sea salt, 10; Huffinan's anodyne liquor rectified, 5; spirit of wine, 10. On the contrary, spirit of sal ammoniac mixed with snow, spirit of sulphur, and spirit of hartshorn, mixed likewise with snow, made no perceptible difference. Highly rectified chymical oils, mixed with water, produced no heat; nor with snow, no cold; as was tried in the oils of turpentine, amber, mint, and mother of thyme. And here it is to be remarked, notwithstand-

ing the contrary has been given out by some, that these chymical oils mixed with the most highly rectified spirit of wine, produce no cold, either on their mixture or half an hour after.

It results from these experiments, that though there are many liquids which can produce artifical cold, the nitrous acid is the most powerful; and mercury may be congealed by it, without any difficult process, at any time, when the heat of the atmosphere is not greater than 175 by the thermometer before-mentioned. And these experiments have not only succeeded with our author, but with many others; among whom, it may be sufficient to mention Messieurs Lomonosow, Zeiher, Aepinus, and Model, as these gentlemen have made themselves well known in the philosophical world. The nitrous acid was poured on the snow in no determinate quantity; sometimes a few drops were sufficient, sometimes it required a larger quantity. Snow seems to be more fit for these experiments than pounded ice; as the former, from its loose texture, is of more apt and easy solution.

Hence it appears, that mercury is no longer to be ranked with the semi-metals, but as a perfect one, fusible, though with a much less degree of heat than any of the others. It agrees likewise with other metals; as their parts like it, when in fusion, attract one another, and run into globules, and, from a state of fluidity, pass into a solid state, not all at once, but successively, and vice versa. But it is worth inquiring, whether this metal, which agrees with all others, both in a solid and fluid state, has not the particular property of boiling at a certain degree of heat, which is by no means to be observed in other metals. The degree of heat, in which mercury begins to boil, is not at 600 of Fahrenheit's scale, as is generally imagined; but at least at 709 of the same scale, which corresponds with 414 of our author's, whose cypher is at the heat of boiling water. Both the boiling and freezing of mercury have this in common; that when it begins to boil, it rises with rapidity; and descends rapidly, when it begins to freeze. If therefore the mean term of the congealation of mercury is fixed at 650 below the cypher, and the term of its boiling at 414 above the cypher; its greatest contraction to its greatest dilatation, will be 1064 degrees of our author's thermometer, and 1237 of Fahrenheit's; as 212 is the point of boiling water in this last, and 32 the freezing one; which corresponds with 150, under the term of boiling water, in our author's. Hence every one will see the great alteration of specific gravity in frozen and boiling mercury, as, between one and the other, the 10th part of the olume is lessened.

It may be asked, why the mixture of snow and nitrous acid does not run into a solid mass, and form itself into ice, but remain of a soft consistence, though actually much colder than what is required to freeze aquafortis? We have already mentioned that aquafortis freezes at 204 of our author's thermometer, which

corresponds with 34 below the cypher of Fahrenheit's. The frigorific mass, in a degree of cold far below this, remained soft like a poultice. The cause of this extraordinary phenomenon seems to be no other than a continuation of the solution of the snow, and its mixing with the nitrous acid. For as the production of cold depends solely on the solution and mixture, it cannot happen that this mass, which constitutes a fluid of a hard kind, should run into a solid consistence, so long as the solution and mixture continue.

XXXII. Observations on the Transit of Venus over the Sun, June 6th 1761. By the Rev. Nathaniel Bliss, M.A., Savilian Prof. of Geometry, Oxford, and F.R.S. p. 173.

The present bad state of health of Dr. Bradley * his Majesty's Astronomer, preventing him from making the proper observations of the transit of Venus, he desired Mr. B. to attend at the Royal Observatory to supply his place in making the necessary observations. The instruments they proposed to use, were a reflecting telescope, of 2 feet focal length, to which was fitted Mr. Dollond's micrometer, both executed by Mr. Short. There were some additions necessary to be made to this instrument, which could not be completed before the 2d inst. But it is absolutely necessary that the telescope should be nicely adjusted to distinct vision for the observer's eye, otherwise the apparent angle, measured by the micrometer, will not be exactly true; and as the eyes of different observers may vary very much; the weather was so very unfavourable, that Mr. B. had not so much as one opportunity of seeing any celestial object, by which he might fit it to the proper focus of parallel rays for his eye. Mr. Green therefore, Dr. Bradley's assistant, was the only person who could use that instrument, having adjusted it to his eye some time before. The instrument Mr. B. made use of, was an exquisite micrometer, of the old form, made by the late Mr. Graham, adapted to an excellent refracting telescope of 15 feet focal length. The sky was so cloudy the morning of the transit, and the apparent probability of its clearing up so small, that they almost despaired of being able to make any observation; for they had but one glimpse of the sun, and that only for about half a minute, till half an hour after 7 o'clock. They then prepared to observe the distance of Venus from each limb of the sun, on the chords parallel to the equator, by Mr. Green, with the reflecting telescope, and its micrometer; and Mr. B., with the refracting telescope, and the old micrometer, observed differences of right ascension and declination from the consequent and southernmost limb of the sun.

They observed the internal contact of Venus with the sun's limb, Mr. Green

* Dr. Bradley died, the year following (1762), and was succeeded by Mr. Bliss, as Astronomer Royal.

having taken off the micrometer, with the 2 feet reflector, Mr. Bird, mathematical instrument-maker in the Strand, with a reflector 18 inches focal length, of his own making, and Mr. B. with the refractor, the telescopes used by Mr. Bird and him magnifying about 55 times, that by Mr. Green 120 times, June 5th 1761, at $20^h\ 19^m\ 00^s$ apparent time, all three agreeing to the same second. The final egress by Mr. Green and Mr. Bliss, was only one second later than by Mr. Bird, at $20^h\ 37^m\ 9^s$ apparent time. At $20^h\ 26^m\ 56^s$, by the mean of 5 observations, the centre of Venus was north of the sun's south limb in declination, by his micrometer, 3′ 20″. The diameter of Venus was once measured by Mr. Green, with Dollond's micrometer, 57″; by Mr. Canton in Spital-Square, being the mean of 3 good observations, with the same kind of micrometer, 58″. The sun's horizontal diameter was observed by Mr. Bird, with the reflector, 31′ 36″, which Mr. B. suspects is 3 or 4 seconds too large, as the telescope was not accurately adjusted for parallel rays to his eye,

The internal contact was observed by Mr. Hornsby, on the north side of the observatory at Shirburn-Castle, with an excellent 12 feet telescope and micrometer, made by Mr. Bird, of the old form; and by Mr. Phelps, on the south side, with a 14-feet telescope; the telescope used by Mr. Hornsby magnifying 68 times, and that by Mr. Phelps about 55 times; by Mr. Hornsby at $20^h\ 15^m\ 10^s$ apparent time, by Mr. Phelps 4 seconds later, Mr. Bartlet counting the clock, which each observer could hear. Mr. Phelps lost the final contact by mistaking the teller of the clock. Mr. Hornsby makes it at $20^h\ 33^m\ 17^s$; but supposes it to have happened a few seconds later; for at $20^h\ 33^m\ 12^s$; it was not quite gone off the sun, when he was obliged to move his eye-stand, and at 20^s after, it was certainly totally emerged. They make the diameter of Venus 56″, and Mr. Hornsby, by a mean of 12 observations, made a little before and after the noon of the 5th, makes the diameter of the sun at right angles to the equator, with his micrometer, 31′ 32″. At $20^h\ 12^m$ apparent time, Mr. Hornsby, by one observation, makes the centre of Venus north of the sun's south limb in declination, 3′ 26″. The latitude of the observatory at Shirburn-Castle is 51° 39′ 22″, being to the north of the Royal Observatory 10′ 43″. The difference of longitude between them has been determined, by some former observations, to be 4′ 1″, Shirburn being to the west.

XXXIII. The Transit of Venus over the Sun, June 6, 1761, at Savile-House, about 8^s of Time West of St. Paul's, London. By Mr. Ja. Short. p. 178.

The instruments, made use of on this occasion, were a reflecting telescope of 18 inches focus, with a helioscope adapted to it, and having a field of more than the sun's diameter, proper for showing Venus on the sun's disk, with great ease

and satisfaction; and another reflector of 2 feet focus, with an achromic object-glass micrometer of 40 feet focus, being the same sort of instrument with those that were made, by order of the R. S., for Dr. Bradley, at the Royal Observatory at Greenwich; and for Mr. Maskelyne, who went to St. Helena; and Mr. Mason, who went to Bencoolen; differing in one particular from their instruments, which had only a common object-glass micrometer.

They had no sight of the sun till a quarter of an hour before 6 o'clock, when through an opening, which lasted for about 2 minutes, Dr. Blair, Dr. Bevis, and Mr. S. plainly and distinctly saw Venus on the sun, and concluded that she was then considerably past the middle of her transit. About a quarter after 6, Mr. S. made the first observation, which was, in measuring the diameter of Venus; and soon after he measured her distance from the sun's limb, in the direction of a line going through the sun's centre; and so continued measuring in the same manner, and sometimes measuring the diameter of Venus, till near the internal contact; only about a quarter after 7, he measured the distance of Venus from the sun's limb, in a supposed direction of her transit line, or path over the sun.

About half an hour after 7 the clouds dispersed, and they had the sun perfectly clear during the remainder of the transit. When Venus approached the internal contact, Mr. S. took off the micrometer, and changed the magnifying power of the telescope, which during the measurements had been that of 70 times, into another of 140 times, and with this magnifying power he observed the internal contact; in which he thinks he cannot have erred so much as 2 seconds, for the air was extremely clear, and at rest. With the same magnifying power he observed the total exit; and does not think he erred in this above 5 seconds, though this is a more uncertain observation than the former, and can by no means be determined so accurately as the internal contact; and what he erred in this last observation is rather in excess, in making the exit too late.

Times and measurements taken at Savile-house, June 6, 1761. Mr. Short observing, and Dr. Bevis marking down the times.

Internal contact by Mr. Short, through a reflector of 2 feet focus, magnifying 140 times . . . . 8^h 18^m $21^s\frac{1}{4}$

Total exit by Dr. Blair, through a reflector of 18 inches focus, magnifying 35 times . . . . 8 36 $12\frac{1}{4}$

Total exit by Mr. Short, through a reflector of 2 feet focus, magnifying 140 times . . . . 8 37 $05\frac{1}{4}$

The diameter of the sun in a horizontal direction was measured just after the transit, and found to be $= 31' \ 30.8''$. The clock at Savile-house was several times compared with Mr. S.'s clock in Surry-street, from Friday evening the 5th

June to Monday evening the 8th June; so that he was as sure of the time at Savile-house, as if the observation had been made at his own house in Surry-street.

XXXIV. On the Transit of Venus, June 6, 1761, made in Spital-Square; the Longitude of which is 4′ 11″ West of the Royal Observatory at Greenwich, and the Latitude 51° 31′ 15″ North. By John Canton, M.A., F. R. S. p. 182.

Having measured the diameter of Venus on the sun 3 times, with the object-glass micrometer, the mean was found to be 58 seconds; and but $\frac{6}{10}$ of a second, the difference of the extremes.*

The diameter of the sun, from 4 observations very nearly agreeing with each other, was 31′ 33″ 24‴.

The time, by the clock, of the internal contact, was	8^h	17^m	4^s
Of the external contact	8	35	27
Of noon	11	58	$24\frac{1}{2}$
Therefore the apparent time of the first contact was	8	18	41
Of the last contact	8	37	4

These observations were all made with a reflecting telescope of 18 inches focal length, which magnified about 55 times.

XXXV. Observations of the Planet Venus, on the Sun's Disk, June 6, 1761; and Certain Reasons for an Atmosphere about Venus. By Samuel Dunn. p. 184.

The latitude of his place was 51° 29′ 5″ N. and 41^s of time west of the observatory at Greenwich, between the physic-garden and Chelsea hospital.

With the 6-feet Newtonian reflector, and its magnifying power of 110, and also of 220 times, he carefully examined the sun's disk, to discover a satellite of Venus, but saw none; for he had a very clear dark glass next his eye, and the sun's limb appeared most perfectly defined; but a very narrow waterish penumbra appeared round Venus, by which its limb was not perfectly defined, and at the distance of about a 6th part of Venus's diameter from its edge, was the darkest part of Venus's phasis, from which to the centre an imperfect light increased, and illuminated about the centre.

At 8^h 16^m by the clock, he was prepared to observe the internal contact; and as Venus drew nearer to the sun's limb, the penumbra near the limb of Venus became darker, and threatened to obscure the point of contact at the instant it would happen.

* With the same micrometer, the diameter of Venus was measured, off the sun, 12 times, March 29th, 1758, about noon; and the mean was 1′ 1″ 42‴; whence the diameter, at the time of the transit, ought by computation to have been 1′ 0″ 19‴.—Orig.

At 8^h	16^m	41^s	No diminution of light between the limbs of Venus and the sun.
8	16	42	Slight penumbra, or diminution of light, near where the contact was to be.
8	16	43	Penumbra of a grey colour, near the same place.
8	16	44	Penumbra almost brown, and the thread of light very narrow.
8	16	45	Penumbra brown, and the thread of light in the contact point indistinct, or lost.
8	16	46	Penumbra more brown, and the touch the smallest possible.
8	16	47	Penumbra almost black, and the touch a little broader.
8	16	48	Slight black in the point of contact, and the edges a little broader.
8	16	49	True black in the point of contact, and the edges a little broader.

Here he guessed that observers would differ in their judgment about the moment of contact some seconds of time, or that some would estimate the contact sooner than others.

From these observations he concluded that the thread of light in the point of contact was so obscured as to be undiscernible at $8^h\ 16^m\ 46^s$, and that true black did not succeed in the same point till 3^s after, namely, $8^h\ 16^m\ 49^s$; and from* both of these properties he concluded that the real internal contact was at $8^h\ 16^m\ 47^s$ by the clock; which makes $8^h\ 16^m\ 11^s$ equal time, and $8^h\ 18^m\ 2^s$ apparent time at Chelsea; and $8^h\ 18^m\ 43^s$ apparent time at Greenwich. While Venus was on the sun's limb no other penumbra appeared between the limb of Venus and the sun, than had appeared before on the sun's disk; and therefore he concluded there must be an atmosphere about Venus, which receiving weak impressions of light between the limbs of Venus and the sun, occasioned the uncertainty of ascertaining the exact instant of the internal contact as above described.

* As the 6-feet Newtonian telescope magnified 4 times as much as that of the 2-feet Gregorian telescope, and the vanishing of the thread of light, from its least degree of duskishness to a true black, was about 3 seconds of time by the 6-feet telescope, the time in which the thread of light was vanishing from the least degree of duskishness to a true black, by a 2-feet Gregorian reflector, may be supposed to have been 4 times 3 = 12 seconds of time; and hence an error, or rather difference of pronunciation, but not of judgment, may have arisen among good observers, if some estimated the contact by the invisibility of the thread of light, and others by an apparent blackness in the point of contact; or, which is the same thing, the time when the planet had made the least apparent dent in the sun's limb, of the same colour, through a dark glass, as the sky This was verified by a 2-feet Gregorian reflector, in the contact above mentioned, and possibly may have occasioned greater differences in estimating the contact, with lesser telescopes, to no less than half a minute of time.—Orig.

At 8^h 35^m per clock, the external contact was near, and not incumbered with such a penumbra or partial light as the internal contact had been. At 8^h 35^m 4^s the least dent possible, quite black, appeared in the sun's limb. And at 8^h 35^m 6^s, the limb was restored to its perfect form, there having been a small trembling light between the narrow watery border of Venus, and the vanishing point of contact in the sun's limb, for these 2 seconds of time. From which the external contact at Chelsea was 8^h 34^m 30^s equal time, and 8^h 36^m 21^s apparent time; which makes 8^h 37^m 2^s apparent time at Greenwich.

From the foregoing circumstances, it appeared that the external contact was more easily to be determined than the internal one, which was contrary to what he had before expected. And considering the aforesaid penumbra, or border of partial light, surrounding Venus as an atmosphere of that planet, with the time of its vanishing, $2\frac{1}{2}$ seconds of time; and reducing this to the diameter of Venus, with due allowance for the oblique direction over the sun's limb, the atmosphere of Venus comes out $8\frac{1}{2}$ thirds of a degree, which is nearly about $\frac{1}{400}$ part of Venus's diameter; which diameter being nearly equal to the earth's, the atmosphere of Venus comes out nearly 50 geographical miles.

XXXVI. Account of the Observations made on the Transit of Venus, June 6, 1761, in the Island of St. Helena. By the Rev. Nevil Maskelyne, M.A., F.R.S. p. 196.

From the very cloudy weather which prevailed here for the whole month preceding the transit, Mr. M. almost despaired of obtaining any sight of it at all. He was however fortunate enough to obtain two fair views, though but of short continuance, of this curious celestial phenomenon. The first was a few minutes after sun-rise, when he was surprized not only at seeing Venus so very large, but also so much nearer the sun's limb than he had reason to expect from the best-grounded calculations; which last circumstance foreboded that she would make a more speedy exit from the sun's body than the same calculations allowed; which accordingly happened. At this time her limb, as well as the sun's, appeared exceedingly ill defined, which was no more than what one might naturally expect, from their great proximity to the horizon.

This alone was sufficient to prevent making any observations at that time, which could admit of any exactness, if the clouds had not presently come up, and totally deprived him of the sight both of Venus and the sun. In this manner the skies continued unfavourable for about an hour, when they became again extremely clear, and he had the pleasure of seeing Venus appear as an intensely black spot on the sun's body, and perfectly well defined. At this time he measured the distance of the nearest limbs of Venus and the sun from each other, with the curious object-glass micrometer adapted to the reflecting telescope, ac-

cording to Mr. Dollond's ingenious invention. This distance was $1' \, 44\frac{3}{4}''$, at 7^h 31^m 7^s apparent time, or 7^h 29^m 15^s mean time. Mr. M. remarks, that though Venus's limb and the sun's appeared as well defined as could be desired; yet when the artificial internal contact of Venus's limb with the sun's was made, in order to measure their distance, Venus's limb alternately dilated itself over, and contracted itself within the sun's limb by a small space. He endeavoured to take it in the middle of this vibration; but he doubts whether, if the real internal contact had happened at this time, it could have been observed, in such circumstances, to that degree of exactness which Dr. Halley hoped for; and whether on occasion of the next transit, which is to happen 8 years hence, it might not be convenient that the observers should endeavour to place themselves on such parts of the globe, as that they may not see Venus on the sun's body, very near the horizon, but rather when they are both elevated to considerable heights; which will afford them a greater chance of making their observations free from clouds, which usually skirt the horizon, as well as of making them to advantage.

Presently after measuring the distance of Venus from the sun's limb, the clouds returned again, and prevented him not only from making any more observations of the same kind, or measuring Venus's diameter, but also, what was of much more consequence, from observing the last internal contact of Venus with the sun's limb, which was the principal observation of all. About 23 minutes after 8 the clouds separated again, and the sun appeared very bright and clear; but there was not the least appearance to be seen of Venus, though he thought himself in a manner sure of observing at least the external contact, as all the calculations make the end to happen much later.

Mr. Waddington took the passages of Venus and the sun's limbs across the horizontal and vertical wire of the equal altitude instrument. All the observations which he was able to make are as follow:

Mean time, in the morning, June 6.

7^h	24^m	1^s	☉'s lower limb at horizontal wire.
	24	18	♀'s centre at vertical wire.
	27	$43\frac{1}{2}$	♀'s centre at the horizontal wire.
	28	$50\frac{1}{2}$	♀'s preceding limb touches vertical wire.
	29	$9\frac{1}{2}$	♀'s subsequent limb at vertical wire.
	31	56	☉'s lower limb at horizontal wire.
	32	17	♀'s centre at the same.
	33	5	☉'s western and subsequent limb at vertical wire.
	34	53	☉'s upper limb at horizontal wire. The observation of this limb of the sun was but indifferent.

N.B. As the telescope inverts, the observations, as usual, are set down according to the appearance.

As it is to be feared that our other observers, Mr. Mason and Mr. Dixon, by the misfortunes they have met with, have not been able to make their observations at Bencoolen, as was proposed; Mr. M. humbly hazards an opinion, whether the difference in the total duration of the transit of Venus over the sun's disk, observed in any two places, where it is likely observations have been made, will be great enough to enable us to infer the sun's parallax with sufficient exactness, or even nearer than it is known already. So that he is afraid we must wait till the next transit in 1769, which is, on many accounts, better circumstanced than this, before astronomers will be able to do justice to Dr. Halley's noble proposal, and to settle, with the last and greatest degree of exactness, that curious and nice element in astronomy, the sun's parallax, and thence determine the true distance of all the planets from the sun, and from each other.

Mr. Maskelyne excuses himself that he does not at present attempt to deduce any consequences from the above observations, not only as he was in want of others corresponding to them made in other places, but also as he was not yet able to settle the longitude of this place to sufficient exactness; though he was of opinion it cannot differ much. Mr. M. had not been able to get one observation of an eclipse of Jupiter's satellites, though he was ready to seize any opportunity, if it had offered; the very cloudy weather, which prevailed at the time, which is the winter there, depriving him not only of these, but almost all other observations.

XXXVII. Account of the same Transit. By the Rev. Richard Haydon. p. 202.

Mr. Haydon's latitude, at Leskeard, was 50° 26′ 55″, and his longitude west of London in time, $16^m\ 10^s$; though he, from a memorandum he made some years ago, supposed it near 2 minutes more.

By correspondent altitudes of the upper limb of the sun, June 5th, 1761, his clock was too fast in apparent time $9^m\ 30^s$. By the same, June 6, it was $9^m\ 40^s$.

June 6th, 1761, the diameter of ⊙ was . ... 31′ 31″.5
and the diameter of ♀ .. .. . 0 59″.0

The nearest distances of Venus from the sun's limb; the diameter of Venus included.

By the clock.	Angular value.	
At $5^h\ 34^m\ 54^s$	5′ 51″.6	Semidiameter of Venus to be deducted for the path of the centre.
5 53 0	5′ 38 .7	

By the clock.	Angular value.
At $6^h\ 13^m\ 2^s$	$5^m\ 15^s.1$
6 31 24	4 40.8
6 54 54	4 22.0
7 28 19	3 0.5

7

8^h	10^m	0^s	Internal contact.
8	29	3	Total egress.

Mr. Haydon, in a subsequent letter, says that on comparing his observations with those made in London, his interval between the internal contact and total egress, was considerably longer than any of the others. Therefore he examined his notes again, but could not find he had made any mistake in transcribing them, but thinks he may at first have set down 1^m too many in the total egress.

XXXVIII. Observations on the same Transit; and on an Eclipse of the Moon, May 8, *and of the Sun, June* 3, 1761. *By Mr. Peter Wargentin, F.R.S., and Sec. R. Acad. of Sciences, Sweden. From the Latin.* p. 208.

Mr. W. says the lat. of Stockholm, his situation, is 59° 20′ 31″, and its long. east of Greenwich $1^h\ 12^m\ 1^s$.

A Total Eclipse of the Moon observed May 18, 1761.

At	9^h	21^m	30^s	A dense penumbra in the moon's margin.
	9	32	30	Beginning of the true eclipse.
	10	41	0	Total immersion of the moon.
	12	15	0	Beginning of the true emersion.
	13	21	8	End of the true eclipse.
	13	23	0	End of the penumbra.

A Solar Eclipse observed June, 5, 1761.

At	3^h	0^m	0^s	Sun rose eclipsed.
	3	12	32	The end as much as could be seen.

The Transit of Venus over the Sun, June 6, *in the Morning.*

At	3^h	21^m	37^s	Some part of Venus on the sun.
	3	39	23	Interior contact, or total immersion.
	9	30	8	Beginning of emersion, or interior contact.
	9	48	9	End of the emersion, or exterior contact.

The beginning of the first contact could not be taken, on account of the undulation of the sun's border. The observations were made with a tube of 20 Swedish feet, of 3 inches focal distance. It was remarkable that on the exit, when a part of Venus was quite emerged, it was visible faintly illuminated. But whether this was owing to an inflection of the sun's rays, or to a refraction in the atmosphere of Venus, he leaves to others to determine.

XXXIX. An Account of the Observations made on the same Transit in Sweden. In a Letter from Mr. Peter Wargentin. Translated from the French. p. 213.

At Torneo in Lapland, Messrs. Lagerborn and Hellant very happily observed

both the entrance and exit of Venus, with telescopes of 32 and 20 feet focal lengths. The principal times observed were as follow:

	Exterior contact at the entrance.	Interior contact at the entrance.	Interior contact at the exit.	Total exit.
Lagerborn	$3^h\ 45^m\ 44^s$	$4^h\ 4^m\ 1^s$	$9^h\ 54^m\ 22^s$	$10^h\ 12^m\ 18^s$
Hellant	3 45 51	4 3 59	9 54 8	10 12 22

Mr. Hellant is esteemed a very good observer. The difference between the meridians of Paris and Torneo is computed to be $1^h\ 27^m\ 28^s$, very nearly.

At Abo, the capital of Finland, situated in latitude 60° 27′, longitude east of Paris $1^h\ 19^m\ 17^s$, Mr. Justander observed with a telescope of 20 feet;

The interior contact, at the entrance, to be at	3^h	55^m	50^s
Beginning of the exit, at	9	46	59
Total emersion, at	10	4	42

At Hernosand, a city in Sweden, in latitude 60° 38′ and longitude $1^h\ 2^m\ 12^s$ east of the meridian of Paris, Messrs. Gister and Strom observed, with telescopes of 20 feet.

Mr. Gister ..	h m s	$3^h\ 38^m\ 26^s$	$9^h\ 29^m\ 21^s$	$9^h\ 46^m\ 40^s$
Mr. Strom ..	3 20 40	3 38 35	.. .. ..	9 46 47

At the observatory at Upsal, Messrs Stromer, Metlander Mallet, and Bergman, made the following observations, with three telescopes of 20 feet, and a reflector of 18 inches. The difference of meridians between Upsal and Paris is $1^h\ 1^m\ 10^s$.

Mr. Mallet	$3^h\ 20^m\ 45^s$	$3^h\ 37^m\ 56^s$	$9^h\ 28^m\ 3^s$	$9^h\ 46^m\ 29^s$
Mr. Stromer	.. ..	3 38 5	9 28 7	9 46 13
Mr. Bergman	.. ..	3 37 43	9 28 9	9 46 30

At Lund in Scanie, Mr. Schenmark observed, with a telescope of 21 feet, the interior contact of the exit was $9^h\ 10^m\ 44^s$, doubtful, being cloudy; total emersion $9^h\ 29^m\ 14^s$. This city is 43′ 50″ to the east of the meridian of Paris.

According to the observations made at the observatory at Stockholm, by Mr. Klingenstiern and Mr. Wargentin;

Mr. Klingenstiern ..	h m s	$3^h\ 39^m\ 29^s$	$9^h\ 30^m\ 11^s$	$9^h\ 48^m\ 8^h$
Mine	3 21 37	3 39 23	9 30 8	9 48 9

The difference of meridians between Paris and Stockholm is 1 $2^m\ 50^s$ or 52^s at most.

In these observations, Mr. W. made use of an excellent telescope, of 21 Swedish feet, and Mr. Klingenstiern observed with one of Mr. Dollond's telescopes, of 10 feet, with an eye-glass fitted to it, which magnified the object more than 140 times.

In comparing these observations together, you will perceive that they do not agree so near as was hoped for; and those which were made at Paris agree but little better.

XL. Observations of Venus on the Sun, made at Paris, June 6, 1761. *By Jerome Lalande. From the Latin.* p. 216.

Clouds in the east obscured the sun from 4 till 7 o'clock, at one interval between which, he took some observations of the limbs of the sun and Venus at the vertical wire of the telescope, whence he concluded that the longitude of Venus, freed of parallax, was 2′ 35″ west of the sun's centre, and the latitude of Venus 9′ 58″; viz. at $6^h\ 31^m\ 46^s$. After many other observations of the same kind, as well as of the distances of Venus from the sun's limb, Mr. L. at the planet's quitting the sun's disk, observed the internal contact at $8^h\ 28^m\ 25^s$, and the external at $8^h\ 46^m\ 54^s$, apparent or true time. Hence he infers, that the apparent time of the conjunction was $5^h\ 52^m$, the planet's latitude 9′ 30″, the longitude of the node being $8^s\ 14°\ 32'\ 20''$.

XLI. An Account of the Observations on the same Transit made in and near Paris. In a Letter from Mr. Benedict Ferner, Professor of Astronomy at Upsal, and F. R. S. From the French. p. 221.

Messrs. Maraldi, De la Lande, De Lisle, and Messier, remained in town, at the Royal Observatory in the Luxembourg palace, and at the Hotel de Clugny; Messrs. de la Caille, Le Monnier, De Fouchy, and Ferner, went out to Conflans, St. Hubert, and to the Chateau de la Muette, where the King's philosophical and optical chamber is. It was in this last place, which is situated $14\frac{1}{2}''$ of time to the west of the Royal Observatory, that Mr. F. made his observations, in company with Mons. de Fouchy.

To take the distances of Venus from the limbs of the sun, for want of a good micrometer. Mr. F. made use of a quadrant of $2\frac{1}{2}$ feet; and for observing the egress, he had a good reflecting telescope of 28 inches focus and 5 inches aperture, which magnified about 80 times. Having calculated and reduced his observations to the Royal Observatory at Paris, he found that the western limb of Venus touched the western limb of the sun, or that the luminous thread of the sun was broke by her,

At	8^h	28^m	29^s	True time, morn.
Last contact, at	8	46	43	
Conjunction of ☉ with ♀..	5	52	20	
Southern latitude of	0	9	32	
Longitude of ☋ being . 2^s	14°	32′	23″	

During the whole time of observing with the telescope, he perceived a light

6

round about Venus, which followed her like a luminous atmosphere, more or less lively, according as the air was more or less clear: its extent altered in the same manner; Nor was it well terminated, throwing out as it were some feeble rays on all sides.

As for the last contact, he would not be surprized if there was a difference of 10″ or 12″ between 2 observers, who had instruments and eyes of equal goodness, and made their observations by the same clock; so difficult a matter he thinks it to determine the exact moment. But for the first, he certainly believes that the difference could scarcely amount to more than 2″ under the same circumstances.

However, the following are greater differences for the first contact than were expected.

Mr. Maraldi observed .. 1st contact at	8^h	28^m	42^s	: 2d contact at	8^h	46^m	54^s
L'Abbé de la Caille	8	28	37	:	8	46	$49\frac{1}{2}$
Mr. Messier observed	8	28	27	:	8	46	37
Pere Noel	8	28	27				
Mr. Fouchy					8	46	42
Mr. Ferner	8	28	29	:	8	46	43
Mr. de la Lande	8	28	25	:	8	46	54

Considering the quickness with which the luminous thread of the sun's limb was broken, by the approach of Venus's limb, Mr. F. has sufficient foundation for supposing that almost all the difference between these observations, for the first contact, depends solely on the different goodness of the instruments, and particularly the measuring the time. It seems to him that the observations of the last contact agree, for the same reasons that the others differ from each other.

P. S. I hope Mons. Baudouin's pieces upon the satellite of Venus is come to your hands. Notwithstanding all the care taken here to discover this satellite on the disk of the sun, on the 6th past, we could see nothing of it.

XLII. Observations on the same Transit of Venus made at Constantinople. By James Porter, Esq., his Majesty's Ambassador there. p. 226.

The sun rises at Stanbole, Constantinople, the 6th June, 4^h 32^m. Venus entered the sun much earlier, and is supposed to have entered its disk above an hour and more, when seen at its rising here. Venus, at emerging out of the sun's disk, touched the interior limb of the sun S.E. at 10^h 15^m. Emerged totally the point of contact, at its going out, at 10^h 32^m 20^s. Observed with a Hadley's reflector 18 inches only, and a good pendulum, with seconds.

XLIII. The Observations made on the same Transit at Upsal in Sweden. By Mr. Torbern Bergman of Upsal. From the Latin. p. 227.

The first appulse to the sun's border could not be observed; at $3^h\ 21^m$, in the morning, when first seen, the transit had commenced. The first internal contact, or the complete immersion, was at $3^h\ 37^m\ 43^s$. At the exit the internal contact was at $9^h\ 28^m\ 9^s$, and the external contact, or end of the transit, was at $9^h\ 46^m\ 30^s$. The observed diameter of Venus between $57''$ and $58''$.

On this occasion the observers thought they perceived symptoms of the atmosphere of Venus; viz. because when the planet was partly on the sun only, the extremity of the exterior part was faintly illuminated and visible; also, on the edge of the planet quitting or approaching the sun's border, something like a drop of fluid seemed to connect them for some time.

XLIV. An Account of the Observations made on the Transit of Venus over the Sun, June 6, 1761, at Cajaneburg in Sweden. By Mons. Planman. Translated from the French. p. 231.

Some new observations have shown that the difference of meridians between Stockholm and Cajanebourg is only $38' 40''$ to $45''$, that is, half a minute less than he before thought it. This correction has convinced Mr. Planman, that in his observation on the interior contact of the limbs of Venus and the sun, at that place, there has happened an error of a minute; not through the fault of the observer, but that of his assistant, who counted the seconds at the clock: so that instead of $10^h\ 8^m\ 58^s$, it should be $10^h\ 7^m\ 58^s$. Mr. Wargentin thinks it extremely probable, that this is the fact; for on this correction all the observations of Mr. Planman agree very well with each other, and with those of the other astronomers.

XLV. A Second Account of the Transit of Venus over the Sun, June 6, 1761. By the Rev. Nathanael Bliss, M. A., Savilian Professor of Geometry in the University of Oxford, and F. R. S. p. 232.

The interior conjunctions of the planets Mercury and Venus, that happened near the ecliptic limits, have always engaged the attention of astronomers, as they furnish the best means of determining some of the most important elements in the theory of those planets. The transits of the former have been often and carefully observed by the most eminent astronomers, ever since the invention of the telescope; and it may be presumed that the elements of Mercury's theory are established as accurately as can be expected. The opportunities of observing Venus on the sun's disk occur so seldom, that the astronomers of these days have reason to think themselves peculiarly happy in being eye-witnesses of so rare

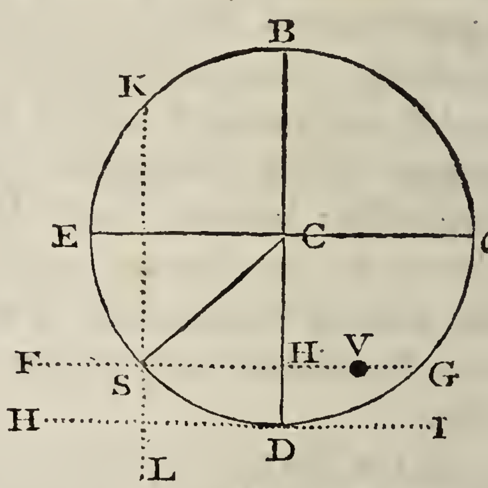
a phenomenon; more particularly too, as the advantages resulting from the observations of this transit, are probably of the greatest moment. The first and only observation of this kind was made by our ingenious countryman the Rev. Jeremiah Horrox, a young gentleman of very distinguished abilities, who, by his own observations, with instruments constructed under his own inspection, and finished by his own hands, was enabled to correct the so much boasted tables of Lansberg, and to predict, with a degree of precision unknown to those times, a phenomenon, which he himself thought to be of great consequence. He immediately communicated this important discovery to his friend and companion in his astronomical studies, Mr. Wm. Crabtree, and earnestly exhorted him to prepare for the observation. The state of the heavens on that day was not very favourable: however, both Mr. Horrox and his friend were lucky enough to observe it; the former, at a time when the limbs of the sun and Venus were in the point of contact, viz. on the 24th of November 1639, o.s. And these two were the first and only persons that ever saw Venus in the sun before the present year.

By the Rudolphine tables, constructed from the observations of Tycho Brahe, Kepler was enabled to predict, in the year 1629, that Venus would pass over the sun's disk in the year 1761. Dr. Halley, in a memoir published in the Phil. Trans. N° 348, exhorted the astronomers of all countries to attend to this rare phenomenon with all possible diligence; as it would furnish them with the best means of determining the parallax and distance of the sun, and consequently the dimensions of the whole solar system. How far the method proposed by him will enable us to solve this difficult problem, must be left to time to discover, when the observations, made in places properly situated, can be compared with those made here, and in other famous observatories. But as the tables which Dr. Halley made use of were very imperfect, his own not being then constructed, and did not represent the place of Venus on the sun with that accuracy which the method in this case required: and as that eminent philosopher committed a small mistake in his calculations, by placing the axis of Venus's path, and the axis of the equator, on the same side of the axis of the ecliptic; a mistake which the most accurate calculator might easily fall into: from these considerations, the honour of determining the sun's true parallax is probably reserved for the present occasion.

The method of determining the right ascension and declination of the centre of Venus from that of the sun, in his late observations, was the same which Dr. Bradley used in observing a former transit of Mercury. The planet was made to run down the fixed wire of the micrometer, and the difference of the time of passage was observed between it and that part of the sun's limb which was cut by that wire; and the moveable wire was brought to touch the sun's lower limb.

If the sun's lower limb had been made to run down the fixed wire, and the moveable wire brought to the planet, and the difference of the time of passage had been observed between it and the sun's consequent limb, on the supposition that the wire was not exactly parallel to the diurnal motion, it would have caused a considerable error in the difference of right ascension, observed at the distance of the sun's semidiameter. But the method made use of requires some calculation, to determine the position of Venus on the sun's disk.

Let therefore the circle EDQB represent the sun's disk, in which let EQ be parallel to the equator, and BD an hour circle; let the dotted line FG represent the fixed wire of the micrometer, HI the moveable wire, and KL the perpendicular or horary wire. The difference of right ascension HV, and of declination CH, will be determined in the following manner: SC, or CD, the semidiameter of the sun is given, and CD — DH = CH, the difference of declination:



and SC and CH being given, SH may be found; and then the observed difference of right ascension SV being diminished in the ratio of radius to the sine of the polar distance of Venus, will give HV the difference of right ascension.

The sun's horizontal diameter, as measured by the micrometer, was 31′ 33″, and that of Venus by several observers 58″; the following observations were therefore deduced by assuming the semidiameter of the sun = 15′ 46″.5, and that of Venus = 29′.

Here are then given a number of observations of the kind above described, both those made at Shirburn castle and those at Greenwich; from each of which, calculated as above, the corresponding difference of right ascension and declination between the centres of the sun and Venus are obtained.

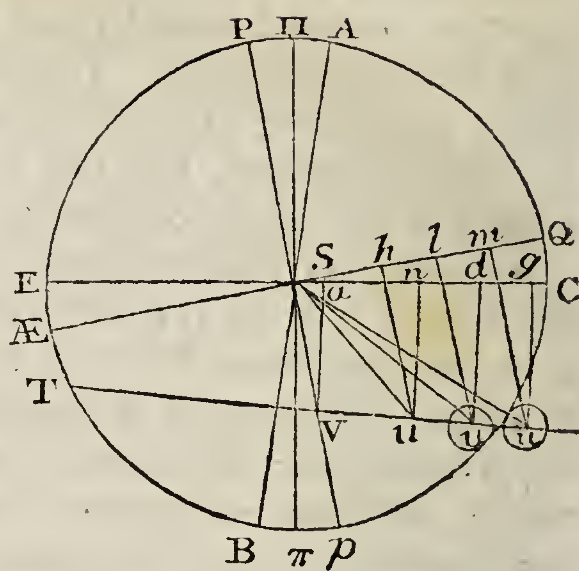
In order to determine more exactly the time of the ecliptic conjunction, with the latitude of Venus then, with the time of the middle of the transit, and the nearest approach of the centres, and thence the true place of her node, Mr. B. has carefully computed the following numbers from theory: because, as Dr. Halley has observed, in the Philos. Trans., N° 386, "there is always an unavoidable, though small uncertainty in what we observe, yet greater than there can be in the theory, especially now it is so very near the truth." The solar numbers were computed from new tables, not yet published, corrected by the small equations, occasioned by the influence of the moon and planet Jupiter, and also the nutation of the earth's axis. The sun's place was very well observed on the meridian, both at Greenwich and Shirburn, the day of the transit; which, allowing for the difference of longitude of those places, agreed to a surprizing

exactness, within 2 seconds; and did not differ more than 5 seconds in excess from the computed place. The place of Venus was computed from Dr. Halley's tables, only adding 31″ to the mean motion, and 1′ 45″ to the place of the node; by which corrections, they had been found to agree better with observations made near the inferior conjunction in 1753.

According to these numbers, the ecliptic conjunction of the sun and Venus was June 5, 1761, N. S. at 17^h 51^m 20^s, mean time, at Greenwich; and the place of the sun and Venus 2^s 15° 36′ 33″; and the geocentric latitude of Venus south 9′ 44″.9. The places of the sun and Venus being computed for 3 hours before, and 3 hours after the ecliptic conjunction, the horary motion of the sun is 2′ 23″.45, of Venus retrograde 1′ 33″.68; the horary motion of Venus from the sun therefore 3′ 57″.13, retrograde. The horary motion of Venus in latitude is south 35″.46. The angle of the visible way with the ecliptic 8° 30′ 10″; the horary motion in that way 3′ 59″.77. The right ascension of the sun, supposing the apparent obliquity of the ecliptic 23° 28′ 18″, was then 74° 22′ 19″.2; and the horary motion of the sun in right ascension was 2′ 34″.55. The declination of the sun was then 22° 41′ 35″.9; the horary motion in declination was 15″.33 northwards. The angle formed by the axis of the ecliptic, and the axis of the equator, was 6° 9′ 34″, decreasing hourly one minute.

The right ascension of Venus at the ecliptic conjunction was 74° 23′ 27″.2: and the horary motion of Venus in right ascension 1′ 36″.75 retrograde. The horary motion of Venus from the sun in right ascension was therefore 4′ 11″.3 retrograde. The declination of Venus was then 22° 31′ 54″.2: and the horary motion in declination was 45″.29 southwards: the horary motion of Venus from the sun in declination was therefore 1′ 0″.62 southwards.

The logarithm of the earth from the sun was then 5.006642; the logarithm of Venus from the sun was 4.861192; and the logarithm of Venus from the earth was 4.460874. If we suppose the horizontal parallax of the sun to be $10\frac{1}{3}$″, then the horizontal parallax of Venus, as seen from the earth, will be 36″.31; which, diminished by that of the sun, is 25″.97. If the parallax in longitude and latitude be computed from these data, the visible horary motion of Venus from the sun in longitude will be 3′ 58″.35 retrograde, and in latitude 33″.75 south. The longitude and latitude of the centre of Venus from the sun's centre, answering to the several right ascensions and declinations observed, may be determined in the following manner.



Let the circle ΠEπC represent the sun's disk: in which let EC be a portion of the ecliptic, Ππ its axis, ÆQ a parallel to the equator, Pp its axis, TVV the visible path of Venus on the sun, and AB the perpendicular to that path. The angle QSC = PSΠ = the inclination of the axis of the equator to the axis of the ecliptic, is given by calculation; then at the internal contact, the side SV, being the semidiameter of the sun, lessened by the semidiameter of Venus, is given, and also vl, the observed difference of declination; whence may be found by plain trigonometry the angle vsl; from which, if the angle QSC be subtracted, there will remain the angle vsd; whence, with sv, may be found sd, the difference of longitude, and vd, the difference of latitude from the sun's centre. In any other position, as at u, there will be given sh, the difference of right ascension, and uh, the difference of declination; whence may be found the angle ush, and the side su: if from the angle ush the angle QSC be subtracted, there will remain the angle usn: which, with the side su, before found, will give sn, the difference of longitude, and un, the difference of latitude from the sun's centre. At the conjunction in right ascension SV is the observed difference of declination, and the complement of the angle QSC is = the angle VSa: whence will be found the difference of longitude sa, and the difference of latitude Va, from the sun's centre.

1. If a mean be taken of the 4th, 5th, 6th, and 7th of Mr. Hornsby's observations, and also of the times at $17^{h}\ 56^{m}\ 46^{s}$, the right ascension of the centre of Venus will be $1''.2$ before the sun's centre, and the declination of it $10'\ 10''.6$; whence the visible conjunction in right ascension was at $17^{h}\ 56^{m}\ 31^{s}$, and the visible declination south of the sun's centre $10'\ 10''.4$; the visible longitude was therefore $1'\ 5''.5$ before the sun's centre, and the visible latitude south of it $10'\ 6''.9$. From the computed visible motion in longitude and latitude, by making the proper proportion, the visible ecliptic conjunction will be found at $17^{h}\ 40^{m}\ 3^{s}$ apparent time, at Shirburn, or at $17^{h}\ 44^{m}\ 4^{s}$ apparent time, at Greenwich, when the visible latitude was $9'\ 57''.6$ south of the sun's centre. At $17^{h}\ 56^{m}\ 46^{s}$, the parallax in longitude (supposing as above the horizontal parallax of the sun to be $10\frac{1}{3}''$) will be $14''$, to be added to the visible longitude of Venus, to give her true longitude before the sun's centre, and $20''.5$ to be subtracted from the visible latitude, to give the true latitude, as seen from the centre of the earth. The true ecliptic conjunction therefore was at $17^{h}\ 36^{m}\ 25^{s}$ apparent time

at Shirburn, or at $17^h\ 40^m\ 26^s$ apparent time at Greenwich, by making a proper proportion from the computed true motion of Venus from the sun, and the true latitude was then $9'\ 34.5''$ south.

2. From the mean of 10th, 11th, 12th, and 13th observations, at $19^h\ 24^m\ 9^s$ apparent time at Shirburn, the observed right ascension was $5'\ 40''$, and the observed declination was $11'\ 33''.6$; whence the visible longitude was $6'\ 52''.2$, and the visible latitude $10'\ 53''.3$, from the sun's centre; and the visible ecliptic conjunction was at $17^h\ 40^m\ 23^s$ at Shirburn, or at $17^h\ 44^m\ 24^s$ apparent time, at Greenwich, with $9'\ 54''.9$ of visible latitude south. The parallax of longitude was $13.2''$, to be added to the visible longitude; and the parallax of latitude $18.1''$, to be subtracted from the visible latitude, to give the true latitude. The true ecliptic conjunction was therefore at $17^h\ 36^m\ 31^s$ at Shirburn, or at $17^h\ 40^m\ 32^s$ apparent time at Greenwich, the true latitude being then $9'\ 31''.6$ south.

3. From the mean of the 14th and 15th observations at $19^h\ 47^m\ 29^s$, the observed right ascension was $7'\ 6''.3$, and the observed declination $11'\ 57''.6$; whence the visible longitude was $8'\ 20''.5$, and the visible latitude was $11'\ 8''$, from the sun's centre; and the visible ecliptic conjunction was at $17^h\ 41^m\ 30^s$, or at $17^h\ 45^m\ 31^s$ apparent time at Greenwich, with visible latitude $9'\ 57''.2$ south. The parallax of longitude was $12''.5$ to be added, and the parallax of latitude $17''.4$ to be subtracted, to give the true longitude and latitude. The true ecliptic conjunction was therefore at $17^h\ 37^m\ 42^s$ at Shirburn, or at $17^h\ 41^m\ 43^s$ apparent time at Greenwich, the true latitude being then $9'\ 33''.9$ south.

4. At the internal contact at Shirburn at $20^h\ 15^m\ 10^s$, if the motion in declination, answering to 3^m of time, be added to the declination observed at the 16th observation, the declination of the centre of Venus from the sun's centre will be $12'\ 24''.4$; whence the visible longitude was $10'\ 12''.6$, and the visible latitude $11'\ 23''$, from the sun's centre; and the visible ecliptic conjunction was at $17^h\ 40^m\ 57^s$ at Shirburn, or at $17^h\ 44^m\ 58^s$ apparent time at Greenwich, with $9'\ 56''.2$ of visible latitude south. The parallax of longitude to be added was $11''.6$, and the parallax of latitude $16''.5$ to be subtracted, to give the true longitude and latitude. The true ecliptic conjunction was therefore at $17^h\ 37^m\ 13^s$ at Shirburn, or at $17^h\ 41^m\ 14^s$ apparent time at Greenwich, the true latitude being $9'\ 33''.1$ south.

5. The 2d observation made at Greenwich being dubious, if the mean of the 1st, 3d, 4th, and 5th, be taken at $19^h\ 48^m\ 39^s$ apparent time at Greenwich, the observed right ascension was $6'\ 52''.4$, and the observed declination $11'\ 52''.4$; whence the visible longitude was $8'\ 6''.1$, and the visible latitude $11'\ 4''.3$, from the sun's centre. The visible ecliptic conjunction was therefore $17^h\ 46^m\ 17^s$ apparent time at Greenwich, with $9'\ 55''.5$ of visible south latitude. The parallax of longitude to be added was $12''.6$, and the parallax of latitude to be subtracted

17″.3, to give the true longitude and latitude from the sun's centre. The true ecliptic conjunction therefore was at $17^h\ 42^m\ 28^s$ apparent time at Greenwich, when the true latitude was 9′ 32″.4.

Mr. B. has omitted the computation of the longitude, latitude, and of the visible and true conjunction from the internal contact at Greenwich, and the difference of declination, as given in his last letter; because there must have been some mistake in reading the numbers of the micrometer, or in setting them or the times down; for they differ too much from all the above, which correspond so well with each other, though made at different places, and with different instruments, and give the true latitude, at the ecliptic conjunction, about 8″ less, that we cannot safely depend on them.

If therefore we suppose the visible ecliptic conjunction to have happened at $17^h\ 45^m\ 3^s$ apparent time at Greenwich, being the mean of the five foregoing deductions, where the greatest difference is no more than $2^m\ 13^s$ of time, or 8″ of visible longitude, with 9′ 56″.3 of visible south latitude, from the sun's centre: where the greatest difference is no more than 2″.7 in latitude, we cannot much err from the truth: and also from the mean of the same deductions, the true ecliptic conjunction, as seen from the earth's centre, will be at $17^h\ 41^m\ 17^s$, with 9′ 33″.1 of south latitude. The middle of the transit was therefore at $17^h\ 20^m\ 5^s$, and the nearest approach of the centres 9′ 26″.8. The latitude then was 9′ 20″.6 south; but the longitude of Venus being augmented by the aberration of light 3″.7, equivalent to 56s of time, by which the true ecliptic conjunction was accelerated, the true equated conjunction was at $17^h\ 42^m\ 13^s$. The error in latitude, caused by the aberration of light, was 1″.4, by which it was diminished; the equated latitude therefore was 9′ 34″.5.

The equation of time was then 1′ 52″, to be subtracted from the apparent time, to give the mean; consequently, the true equated ecliptic conjunction, as seen from the earth's centre, was at $17^h\ 40^m\ 21^s$ mean time at Greenwich. The true place of the sun, corrected by observation, was at that time $2^s\ 15°\ 36′\ 12″$; and consequently the heliocentric place of Venus was $8^s\ 15°\ 36′\ 12″$ with the geocentric latitude 9′ 34″.5. Now in this case the geocentric latitude is to the heliocentric latitude, as the distance of Venus from the sun is to the distance of Venus from the earth; and therefore the planet's latitude, as seen from the sun, was 3′ 48″.5. If we suppose the inclination of the orbit of Venus to be 3° 23′ 20″, as determined by Dr. Halley and M. Cassini, the distance of Venus from the node will be 1° 4′ 20″; consequently its true place $2^s\ 14°\ 31′\ 52″$ on the day of the transit. The effect of refraction is not taken into these calculations, because, at the first observations, when its effects would have been greatest, it amounted only to a very small part of a second.

XLVI. Observations of the same Transit made at Madrid. By Father Ant. Ximenes. From the Latin. p. 251.

By the latest observations, the latitude of Madrid is $40^{\circ}\ 25'$, and difference of time from Paris $24^{m}\ 18^{s}$. He made his observations with a telescope of $2\frac{1}{2}$ feet, made by Mr. Geo. Adams, and a clock made by Ellicot; with which he made a great number of observations on the appulses of Venus and the sun, to the vertical and horizontal wires of the telescope. At the exit he also observed the interior contact at $8^{h}\ 1^{m}\ 14$, and the exterior contact at $8^{h}\ 19^{m}\ 23^{s}$. He had doubts of the exterior contact to 3 or 4 seconds, but of the interior none.

XLVII. The same Transit observed at Tobolsk in Siberia. By M. Chappe. p. 254.

	Apparent time.		
End of the eclipse of the sun, June 2, 1761, at.	18^{h}	11^{m}	4
Internal contact of Venus with the sun's limb at ingress, 5th June	19	0	28
Internal contact at the egress, 6th June	0	49	$20\frac{1}{2}$
External contact at the egress	1	7	$39\frac{1}{2}$

These observations were taken with a refracting telescope of 19 Paris feet focal length, with an eye-glass of 3 inches focus. The least distance of the southern limb of Venus from the nearest limb of the sun, was measured by a micrometer fitted to a 10-foot telescope, and found to be $= 6'\ 2''$, and the sun's diameter was $= 31'\ 37''$.

XLVIII. Observation of the same Transit, made at Leyden. By John Lulofs, Professor of Mathematics and Philosophy in that University. From the Latin. p. 255.

Being furnished with proper instruments, Mr. L. commenced at $3^{h}\ 30^{m}$ to watch the approaching transit, but was prevented by clouds from seeing it, till by an opening between them, about $4^{h}\ 10^{m}$, he saw Venus on the sun's disc as a black patch. He had 4 or 5 other prospects of the transit through thin clouds; and he sometimes thought he observed a kind of lucid corona around Venus, of a breadth equal to $\frac{1}{6}$ or $\frac{1}{7}$ of her diameter. At $8^{h}\ 26^{m}\ 50^{s}$ true time, he observed the interior contact: but clouds prevented his seeing the exterior. He wondered that at the time of that contact, though they were then at an altitude pretty free from refraction, the border of Venus appeared serrated: which may render the time of it doubtful by 3 or 4 seconds.

XLIX. The Case of a Patient, who voided a large Stone through the Perinæum from the Urethra. By Dr. Frewen, of Rye in Sussex. p. 258.

Henry Taught of Hastings in Sussex, aged 76, a strong hale man, and natu-

rally of a good constitution, was never subject to any nephritic or gravelly complaints for almost 70 years, but enjoyed for the most part a good share of health, till about 6 or 7 years before, when he had some gravelly complaints, and uneasiness in making water: which increased on him progressively; and for the last 2 years he had so much pain in sitting, that he was obliged to use a perforated chair, made for that purpose. But for some months past, his increased pain would not permit him to sit at all, even at his meals, which he used to take either standing or lying. When he first came to be in this painful situation, there appeared a prominence on the right side of the perinæum, towards the hinder part of the scrotum; which, increasing by degrees, felt hard and superficial for some time, and the parts all about it became so extremely sore and tender, that at length on the 24th of September, 1761, on getting out of bed a laceration happened, and the stone was voided, falling down on the floor.

Five days after this happened, Dr. F. went to see the patient, to get a perfect knowledge of the circumstances of the fact; the particulars of which he then communicated to Mr. Warner, surgeon of Guy's hospital in London, who returned him a satisfactory account, from his own observations, of the manner by which a stone is contained in the urethra, &c.

Ever since the stone came away, this patient had discharged no urine but by the wound; which when Dr. F. first saw him, was so much contracted, as to be no larger than to admit into it a small finger, and the parts were become callous about it. Dr. F. would have recommended him to proper care on that occasion; but he would by no means hearken to him; seeming to be very happy in being freed from the cruel burden of the stone; and not regarding, he supposes, at his time of life, whether he could be helped in the discharge of his urine any other way.

In a further account Mr. Warner adds that when this surprizingly large calculus was first voided, which was on the 24th of September 1761, it weighed 6 oz. and 2 drs.; that on the 29th of the same month it weighed 6 oz., wanting 1 dr. 15 grs. On the 11th of October following, it weighed 6 oz., wanting 3 drs. 1 scr. On the 17th of the same month, it weighed 6 oz., wanting $3\frac{1}{2}$ drs.

Mr. W. further adds, that in March 1761 he produced 2 very remarkable calculi to the R.S. for their inspection; when they did him the honour to desire a written account of the case of the person, in whose urethra they were lodged. The whole of what he thought worth troubling the Society with, on that occasion, was, that they had been for many years lodged in the urethra of one Robert Bolley, a young man, aged about 22, and that they had produced no great inconvenience or pain till of late, when the integuments began to inflame; which inflammation commenced not long before he was put under his care. The consequence of this change in the parts was extreme torture, a severe symptomatic

3

fever, great wasting away of the whole body, and almost a continual and involuntary discharge of small quantities of urine.

The man was conveyed to London in a waggon. The parts were then arrived to so great a degree of distention, inflammation, and tenderness, that on the journey they burst, and there was discharged through an opening made in the perinæum (that is, the space between the anus and scrotum) one of these stones; the other stone remained firmly fixed in the urethra, which Mr. W. easily removed, having first cut away as much of the diseased integuments of the acceleratores urinæ muscles, and distended urethra, as he judged necessary to be removed for this purpose. After the removal of these parts, he brought together the lips of the wound, and kept them so, by means of what surgeons call the twisted suture, till the parts were united, which was effected in about a fortnight. Before the suture was applied, he introduced a ductile instrument, of a convenient size, through the penis into the bladder, by which means the passage was kept equally distended.

This operation so effectually answered his expectation, as totally to remove the incontinence of urine, as well as every other symptom that had attended the complaint; and the patient was in a short time restored to his usual healthy state and corpulency.

N. B. In the 2 instances here related, as well as in the case of Thomas Bingham, whose history Mr. W. communicated to the R.S. on the 13th of Dec. 1759, (vide Phil. Trans. for 1760) he observes that these patients according to the best information he could get, were never attacked with a suppression of urine, or a regular fit of the stone; for which reasons, he concludes, that the formation of these calculi originally commenced in the urethra itself, and that the stream of urine in its course from the bladder through the penis, had gradually formed those groves or channels, so apparent on the surfaces of these compact and hard bodies, over which they occasionally were voided; by this means a passage for the urine always remained open and unobstructed.

Plate 14, fig. 3, represents the size, shape, and appearance of the stone, with the grooves on its superior surface, that was voided through a laceration of the perinæum, described in the case of Henry Taught, of Hastings in Sussex. Fig. 4 represents the 2 stones that were lodged in the perinæum of Robert Bolley, a young man of 22 years of age, as above mentioned, with the polished surfaces. A, B, where they came in contact with each other. Fig. 5 are these 2 stones joined together, with their several eminences and depressions, as they lay in contact with each other in the perinæum.

L. The Case of a Boy, who had the Malleus of each Ear, and one of the Incuses dropped out. Communicated by the Rev. Philip Mordant, M.A. Rector of St. Mary's in Colchester. p. 264.

A young lad, at Manningtree in Essex, after about 3 or 4 weeks of a putrid, malignant, inflammatory fever, attended with a violent scarlet eruption on the skin, and swelling and soreness, and stuffage of the nose, had the malleus of each ear, and one of the incuses dropped out. Whether any of the rest came away unobserved, his friend could not tell; but these were all he saw. Nor could he say, whether the membrane was destroyed, and discharged with the bones, or only so relaxed, as to give room for the bones to come without it; not having seen the bones till after they were cleaned. But the consequence was, that he almost absolutely lost his hearing; almost, because though quite deaf as to all common voices and sounds, yet some violent and sudden noises seemed to affect him. But the organ of both ears seemed to be so much destroyed, as to make it highly improbable that he should ever recover his hearing again. In all other respects, he was very well, and then in good health. The coming away of those bones seemed the effect of an abscess, which affected the contents of the tympanum.

Another friend observed, that his disorder had been a malignant or ulcerous sore throat, as he judged from the scarlet eruption; and the passage from the back of the fauces into the ear having lain open exposed to its malign influence, an abscess had been formed in the tympanum, which had been destroyed; otherwise the bones could not come out at the other ear.

He had learned to read before this unhappy accident, and the people about him wrote down what they wanted to make him understand.

LI. Observations concerning the Body of his late Majesty, October 26, 1760. By Frank Nicholls, M.D., F.R.S. Physician to his late Majesty.* p. 265.

The circumstances† attending the death of the late King being such, as are not to be met with in any of the records of medical cases, and such as, from the nature of the parts concerned, are not easily to be accounted for; I presume (says Dr. Nicholls) it will be agreeable to the Royal Society and to the learned world in general, if I lay before them a minute detail of what occurred on that remarkable and melancholy occasion; with such explanations as arise from the circumstances of the case.

According to the report of the pages then in waiting, about 7 in the morning,

* George II.

† This account was laid before his present Majesty George III. for his inspection; and his Majesty's answer was that he saw no reason why it should not be made public.—Orig.

Saturday, October 25th, a noise was somewhere heard, as if a large billet had tumbled down; and, on enquiry, his Majesty was found fallen on the ground, speechless and motionless, with a slight contused wound on his right temple. He appeared to have just come from his necessary-stool, and as if going to open his escritoir. Mr. Andrews (at that time surgeon to the household) attempted to take away some blood; but in vain, as no signs of sense or motion were observed from the time of his fall.

The next day, (Sunday, October the 26th) by order of the Lord Chamberlain, I attended, with the 2 serjeant-surgeons, who were directed to open and embalm the Royal Body.

On opening the abdomen, all the parts therein contained were found in a natural and healthy state, except that some hydatides (or watery bladders) were found between the substance of each kidney, and its internal coat. These hydatides might, in time, have proved fatal, either by compressing and destroying the kidnies, so as to bring on an incurable suppression of urine; or, by discharging a lymph into the cavity of the abdomen, might have formed a dropsy, not to be removed by any medicines: but, in the present case, these hydatides were of no consequence, as none of them exceeded the bulk of a common walnut.

On opening the head, the brain was found in a healthy state, no-ways loaded with blood, either in its proper vessels, or in the contiguous sinuses of the dura mater.

On opening the chest, the lungs were in a natural state, free from every appearance of inflammation, or tubercle: but on examining the heart, its pericardium was found distended, with a quantity of coagulated blood, nearly sufficient to fill a pint cup; and, on removing this blood, a round orifice appeared in the middle of the upper side of the right ventricle of the heart, large enough to admit the extremity of the little finger. Through this orifice, all the blood brought to the right ventricle had been discharged into the cavity of the pericardium; and, by that extravasated blood, confined between the heart and pericardium, the whole heart was very soon necessarily so compressed, as to prevent any blood contained in the veins from being forced into the auricles; which therefore, with the ventricles, were found absolutely void of blood, either in a fluid or coagulated state.

As therefore no blood could be transmitted through the heart, from the instant that the extravasation was completed, so the heart could deliver none to the brain, and in consequence all the animal and vital motions, as they depend on the circulation of the blood through the brain, must necessarily have been stopped, from the same instant; and his Majesty must therefore have dropped down, and died instantaneously: And as the heart is insensible of acute and circumscribed pain, his death must have been attended with as little of that distress, which

usually accompanies the separation of the soul and body, as was possible, under any circumstances whatever.

The above-mentioned appearances (as they showed the immediate cause of his Majesty's death) were thought sufficient to form the report to his present Majesty, and his Council. But as the very eminent and amiable character of his late Majesty must make the nature of his death the object of every one's attention and inquiry: and as the case was exceedingly singular and extraordinary in itself; and as the heart must have been merely passive, and consequently there must have been some other concurrent circumstances necessary to produce such an effect; I judged, at the time when the report was drawn, that a more minute and exact detail would not only be expected by the world, but would be highly proper, as our inquiry furnished sufficient matter.

Two questions naturally arise on the face of our report; viz. by what means the right side of the heart became so charged with blood, as to be under a necessity of bursting? and how it could happen, that, as the ventricle (when under great distentions) generally makes one continued cavity with the auricle, and is much thicker and stronger than the auricle, the blood should nevertheless force its way, by bursting the ventricle, rather than the auricle, seemingly in contradiction to the known property of fluids, to force their way where the resistance is least.

On examining the parts, we found the two great arteries, (the aorta and pulmonary artery, as far as they are contained within the pericardium) and the right ventricle of the heart, stretched beyond their natural state; and, in the trunk of the aorta, we found a transverse fissure on its inner side, 1½ inch long, through which some blood had recently passed, under its external coat, and formed an elevated ecchymosis. This appearance showed the true state of an incipient aneurism of the aorta; and confirmed the doctrine, which I had the honour to illustrate, by an experiment, to the satisfaction of the R. S., in the year 1728; (See Phil. Trans. N° 402) viz. that the external coat of the artery may (and does) often controul an impetus of the blood, capable of bursting the internal or ligamentous coat; although this last is by much the thickest, and seemingly the strongest.

In regard to this distention of the aorta; as his Majesty had, for some years, complained of frequent distresses and sinkings about the region of the heart; and as his pulse was of late years observed to fall very much on bleeding; it is not doubted, but that this distention of the aorta had been of long standing, at least to some degree; and as the pulmonary artery was thereby necessarily compressed, and a resistance, greater than natural, thereby opposed to the blood's discharge out of the right ventricle, it is reasonable to conclude, that a distention and consequent weakness of the pulmonary artery and right ventricle, to some degree, were nearly coeval with that of the aorta. But that the aorta had suffered a more

extraordinary and violent distention, immediately antecedent to the bursting of the ventricle, is evident, from the recent fissure of the aorta, and the consequent extravasation of blood between its coats. Now as this increased and violent distention of the aorta must have been attended with a proportionate pressure on the pulmonary artery, and consequently an increased opposition to the passage of the blood out of the right ventricle; so that distention of the aorta must be considered as the immediate cause of the right ventricle's being surcharged with blood, and consequently of its bursting.

The immediate cause of this distention of the aorta, as also of its being determined to that particular time, are naturally explicable, from his Majesty's having been at the necessary-stool; as the office then required cannot be executed but by such a pressure on all the contents of the lower belly, and consequently on the great descending artery, as must of necessity subject the trunk of the aorta, and all its upper branehes, to a surcharge with blood continually increasing, in proportion as the pressure may happen to be continued longer, or exerted with greater violence, in consequence of a costive habit, or any other resistance.

As to the 2d question; viz. how it could happen that the blood should force its way rather through the side of the ventricle than of the auricle? since it is well known that when the ventricle is fully distended with fluids, they will easily pass back into the auricle; so that under such a distention as the ventricle must have suffered before it burst, it should seem to have made one continued cavity with the auricle; of which cavity, the auricle being by much the weakest part, must have been the most liable to a rupture. This certainly is the circumstance in which the very great singularity of the case before us consists; and many difficulties offer against any obvious explanation.

Two circumstances however seem to throw some light on this obscure and difficult question. The first consists in the texture, connexions, and capacity of the pericardium; the 2d in the order in which the several surcharges must have arisen.

The pericardium is a strong tendinous membrane, inelastic in every direction, containing the 2 auricles, the 2 ventricles, and the 2 great arteries, as in a purse; it is fixed to its contents at the back of the 2 auricles, where, by its connexion, it surrounds the 2 venæ cavæ: hence, passing along the arch formed by the aorta, it descends to the pulmonary artery, and continues round the orifices of the pulmonary veins, firmly attached to these several parts in its passage. By these connexions, these parts are all fixed in their several stations, incapable of separating from each other, or shifting their situations, however they may happen to be compressed. The pericardium is generally said to serve as a defence to the heart; but that defence seems to consist chiefly, in preventing the right auricle from being stretched by the depressions (or complanations) of the diaphragm in

hunger and inspiration, and, by its bearing firmly against the sides of the auricles, to support and strengthen them against too great distentions: for the cavity of the pericardium seems to be but little more than commensurate to the bulk of its contents, when one half of them are filled and the other half empty. This will appear on endeavouring to fill the heart, with its auricles, and its 2 great arteries, with wax, at the same time while it is inclosed in the pericardium; in which experiment, one or other of these cavities will be found to have been so compressed by the pericardium, as to have refused a free admittance to the wax, and will therefore be found proportionably empty.

The elastic texture, connexions, and capacity of the pericardium, being thus stated, let us now consider the order in which the several distentions must have arisen in the two great arteies and cavities of the heart, with the necessary effects of those distentions on the pericardium and the parts which it contains.

The first distention (and this a great and violent one) must have arisen in the aorta; and the consequent pressure on the pulmonary artery, by the aorta so distended, must have been sufficient (either by degrees or at once) to stop the blood's discharge out of the right ventricle and pulmonary artery, and to distend both those cavities beyond their natural state of repletion. So that under these circumstances the 2 great arteries and the right ventricle, must have been under an extraordinary and continual distention, and consequently an increase of bulk at the same time; whereas, in the natural state of the body, these 3 cavities are alternately dilated and contracted, and the right ventricle is always proportionally diminished in bulk, as the pulmonary artery is increased, and vice versa. So that with respect to these three great cavities, (supposing that their several distentions had been no greater than natural) the pericardium must have been obliged to contain one third more in proportion than its capacity was formed to receive. During this time, the blood being stopped in its passage through the lungs, and its afflux to the left auricle and ventricle being thereby suspended, the left auricle and ventricle must have remained in a contracted state; in consequence of which the right ventricle had ample space in the pericardium, to admit that degree of distention, which was previously requisite for its bursting. But the right auricle (being fixed to its station by its connections with the left auricle and the pericardium, and being firmly compressed against the pericardium, by the aorta, the pulmonary artery, and the right ventricle, all which appear to have been, at this time, greatly distended beyond their natural bulk) must have been thereby deprived of the space in the pericardium necessary to admit of its being distended; and the whole surcharge and distention must, by the pressure of the pericardium on the auricle, necessarily have been confined to the right ventricle till it burst.

Had these surcharges arisen in any other order, their effects must have been

greatly different: as for instance, if the surcharge in the right ventricle had arisen from any other pressure than from such a distention of the aorta, the extraordinary bulk of the aorta, and its pressure against the pulmonary artery, would not have existed, and the right auricle, not being then compressed against the pericardium, would have been at liberty to distend, till the blood had made its way through its sides.

In confirmation of this power here attributed to the pericardium, of strengthening and supporting its contained parts, let it be observed that, in the case under consideration, the place of the fissure in the aorta is precisely where the pressure of the pericardium is kept off from the aorta, to a considerable degree, by the situation of the right auricle and the pulmonary artery.

LII. Of the Irregularities in the Planetary Motions, caused by the Mutual Attraction of the Planets. By Charles Walmesley, F.R.S., and Member of the Royal Academy of Sciences at Berlin, and of the Institute, at Bologna. p. 275.

Finding that the influence which the primary planets have on one another, to disturb mutually their motions, had been but little considered, Mr. W. thought it a subject worthy of examination. The force of the sun to disturb the moon's motion, flows from the general principle of gravitation, and has been fully ascertained, both by theory and observation; and it follows, from the same principle, that all the planets must act on one another, proportionally to the quantities of matter contained in their bulk, and the inverse ratio of the squares of their mutual distances; but as the quantity of matter contained in each of them, is but small when compared to that of the sun, so their action on one another is not so sensible as that of the sun on the moon. Astronomers generally contented themselves with solely considering those inequalities of the planetary motions, that arise from the eliptical figure of their orbits; but as they have been enabled of late years, by the perfection of their instruments, to make observations with much more accuracy than before, they have discovered other variations, which they have not indeed been able yet to settle, but which seem to be owing to no other cause but the mutual attraction of those bodies. In order therefore to assist the astronomers in distinguishing and fixing these variations, Mr. W. endeavours to calculate their quantity, from the general law of gravitation, and reduce the result into tables, that may be consulted whenever observations are made.

He offers at present the first part of such a theory, in which he has chiefly considered the effects produced by the actions of the earth and Venus on each other. But the same propositions will likewise give, by proper substitutes, the effects of the other planets on these two, or of these two on the others. To ob-

viate in part, the difficulty of such intricate calculations, he has supposed the orbits of the earth and Venus to be originally circular, and to suffer no other alteration but what is occasioned by their mutual attraction, and the attraction of the other planets. Where the forces of two planets are considerable with respect to each other, as in the case of Jupiter and Saturn, it may be necessary in such computations to have regard to the excentricity of their orbits; and this may be reserved for a subject of future scrutiny. But the supposing the orbits of the earth and Venus to be circular, may, in the present case be admitted without difficulty, as the forces of these two planets are so small, and the excentricity of their orbits not considerable. On these grounds therefore he has computed the variations which are the effects of the earth's action: 1st, the variation of Venus's distance from the sun; 2dly, that of its place in the ecliptic; 3dly, the retrograde motion of Venus's nodes; and 4thly, the variation of inclination of its orbit to the plane of the ecliptic.

The similar irregularities in the motion of the earth, occasioned by its gravitation to Venus, are here likewise computed: but it is to be observed, that the absolute quantity of these irregularities is not here given, it being impossible, at present, to do it; because the absolute force of Venus is not known to us. He has therefore stated that planet's force by supposition, and has accordingly computed the effects it must produce: with the view that the astronomers may compare their observations with the motions so calculated, and from thence discover how much the real force differs from that which has been supposed. But the exact determination of the force of Venus must be obtained by observations made on the sun's place, at such times when the effect of the other planets is either null or known.

The influence of Venus on the earth being thus computed, that of the other planets on the same may likewise hereafter be considered: by which means, the different equations, that are to enter into the settling of the sun's apparent place, will be determined; the change of the position of the plane of the earth's orbit will also be known; and consequently the alteration that thence arises in the obliquity of the ecliptic, and in the longitude and latitude of the fixed stars. These matters of speculation are reserved for another occasion, in case what is here offered should deserve approbation. But the later more accurate calculations and astronomical observations have rendered a further account of this elaborate paper unnecessary.

LIII. An Account of a Treatise in French, presented to the Royal Society, intitled, Lettres sur l'Electricité, by the Abbé Nollet, Member of the Royal Academy of Sciences, &c. &c. By William Watson, M.D., F.R.S. p. 336.

About 8 years since, the learned and ingenious author of the work before us

published a treatise, of which the present work may be considered as a continuation. That consisted of 9 letters on the subject of electricity, which were addressed to persons who had distinguished themselves by their endeavours to illustrate this part of natural philosophy. In like manner, the present performance consists of 8 letters, and is addresed, as the former, to his friends and correspondents.

As an account of the former treatise was communicated by myself to the Royal Society, and printed by direction of the council in the Phil. Trans.,* the author requests, at the end of the 16th letter, which is addressed to me, that I would give myself the additional trouble to lay before you an account of the present work. This request I most readily comply with, not only in obedience to the order of the society, but likewise as a testimony of the esteem and regard which I have long entertained, and shall continue to do, for the excellent author of it.

This treatise, like the former, is printed in 12mo. and contains 284 pages exclusive of the preface and 4 tables exhibiting 14 figures. The principal design of the work is to support and further confirm the hypothesis of the author, and of several other persons who have considered these matters, that the effects of electricity depend on the simultaneous affluence and effluence of the electric matter.

In defending his opinions in regard to the effects of electricity, the Abbé Nollet has given a variety of new experiments, which cannot but be agreeable to those who are conversant in these matters. He has also occasionally mentioned those of other persons, which are come to his knowledge, and which he apprehends not to be sufficiently known. He has traced the origin of several happy inventions, and has exhibited the real authors of them. He has given, as he imagines, additional value to several experiments which appear to have been too much neglected; and brought others which have been over-rated to their proper standard.

As this work is of a controversial kind, the author has had particular attention to such points as have been the occasion of contest; to weigh the reasons of his opponents, and to add new explanations to such of his opinions as seem to want them; more particularly to such as have appeared to him to have been misunderstood.

The first of these letters is addressed to M. Necker, professor of experimental philosophy at Geneva.. In this letter, our author endeavours to establish his opinion published long since, in regard to the existence of the simultaneous affluence and effluence, and consequently the double current of the electric matter in opposite directions. And herein our author by a series of experiments, obviates some doubts which had occurred to M. Necker in respect to the validity of this hypothesis.

The 2d letter is addressed, as the former was, to M. Necker of Geneva. In

* Vide Vol. XLVIII. p. 202.

this letter the hypothesis of M. Jallabert of Geneva, a very worthy member of this Society, in regard to the electrical phenomena, is examined; and such part of it as does not coincide with the ideas of our author, he endeavours to confute by an ingenious series of deductions. The 3d, 4th, and 5th letters are addressed to M. Du Tour, of Riom in Auvergne, who has been a diligent inquirer into the nature and properties of electricity. In the first of these is a careful examination of the validity of the doctrine of plus and minus in bodies electrified. So early as in February 1745, I communicated to the Royal Society an experiment and some deductions from it, which laid the foundation of this doctrine. This experiment, and the deductions in consequence of it, were afterwards printed in the Philos. Trans.* These I explained more at large, both by experiments and observations, in another paper read to the Society in February 1745-6,† and were the experiments which so early caused me to conceive that there was something in the phenomenon of electricity not to be resolved but on statical principles; and enabled me first to assert that the phenomena in bodies electrified, however similar they might appear, did really arise from the electricity being either greater or less than their natural quantity. This doctrine has since that time been the cause of a variety of experiments, both here and abroad, by which great light has been thrown on this part of natural philosophy. How far our author has been able to overturn this doctrine must be left to other judges to determine.

In the 4th letter the doctrine of resinous and vitreous electricity is examined. In this letter, as well as in the 5th, a great number both of experiments and deductions are produced, not only to weaken the doctrine of plus and minus, but to establish the principle of simultaneous affluence and effluence of electric matter; as if this principle be allowed the doctrine of resinous and vitreous electricity may be reduced to it: as our author is of opinion that there is only one and the same kind of electricity, whether it is natural or artificial; and that however appearances may make it seem to vary, the electricity is one and the same.

The 6th letter is an answer to one of Father Beccaria, professor of experimental philosophy in the university of Turin, published in Italian, in the year 1753, and addressed to the Abbe Nollet. This letter of Pere Beccaria was translated into French, and published at Paris in 1754, by M. Delor, with many additions and annotations. It contains a very great number of curious experiments and observations, both on artificial and natural electricity; many of which are brought to prove the validity of the doctrine of our worthy member Dr. Franklin, in opposition to that of the Abbe Nollet. More particularly he endeavours to confute the Abbe's opinion, in regard to the affluence of the electric matter, which the Abbe has by experiments and observations ingeniously endeavoured to confirm. Pere

* Vide Vol. XLIV. p. 739.

† See Phil. Trans. Vol. XLV. p. 93—101.

Beccaria's observations on natural electricity, and on meteors, on which he has made a prodigious number of experiments, many of them of a delicate nature, do him a great deal of honour.

The 7th letter, the ingenious author does me the honour to address to me. In this letter he justly laments the calamities of war; more particularly as it in a great degree prevents that correspondence between men of letters which contributes so much to their mutual satisfaction, and on which the improvement of science so much depends. The more particular purport of this letter, is to answer some objections, which Mr. David Colden, of North America, published against the former letters of our author. These relate more particularly to the impermeability of glass to the electric fluid, and to the explanation of the phenomena of the experiment of Leyden. Besides these, he gives us his idea of non-electrized bodies electrized plus, as he does not approve of the idea generally received of the accumulation of electricity. He mentions, that he has read Mr. Canton's memoir relating to electricity, with his observations on stormy clouds. He finds many curious facts in that work; but thinks them not sufficient to make the deductions Mr. Canton has done, in favour of the doctrine of plus and minus. M. du Tour of Riom has sent the Abbé Nollet a memoir, which he has likewise been so kind as to send me, containing a review of these experiments, from which he thinks it very easy to resolve all these phenomena on the doctrine of simultaneous affluence and effluence of the electric matter.

The 8th letter is addressed to M. de Romas, assessor to the presidial of Nerac, and contains remarks on electrical kites; on Father Ammersin's manner of preparing and using wood to insulate bodies, in making electrical experiments; and some observations concerning the doctrine of simultaneous affluence and effluence of the electric matter. M. de Romas, in flying his electrical kite, was the first who used a cord composed of hemp and wire. This compounded cord conducted the electricity of the clouds far more perfectly than a hempen cord would do, even though it was wetted; and this cord being terminated by one of dry silk, enabled the observer, by a proper management of the apparatus, to make what experiments he thought proper, without danger to himself. The Abbé Nollet however desires M. de Romas to be very cautious in making these experiments, and not too much to confide in his silk lines; as the vastness of the electrical matter in thunder-storms may overcome the property of the silk, and even make it a conductor of electricity, and hazard the life of the observer. The quantity of electricity brought by M. de Romas's kite from the clouds has been so great, that on the 26th of August 1756, 'the streams of fire were an inch thick, and 10 feet long, which were conducted by the cord of the kite to the non-electric bodies near it, and the report of which was equal to that of a pistol.' If a stroke of this kind had gone through the body of M. de Romas,

probably the late unfortunate Professor Richmann had no longer been the only martyr to electricity.

Father Ammersin's method of preparing wood, so as to make it serve the purpose of glass, wax, &c. in electrical experiments, was published at Lucerne in the year 1754, and our author has given us an extract of it at the end of his work. This father found that the frying of wood, after its being well dried in an oven, or otherwise, in either the oil of walnuts or that of linseed, made it fit to insulate those bodies, which you chose to electrize, by preventing the dissipation of the electricity: not only so, but what makes it still more valuable to those who are engaged in these pursuits, you may excite electricity with it, as the Abbé Nollet says he has done, to his great convenience. He says further that the end of a board mounted upon 4 pegs, a pair of wooden shoes, some truncheons of beech, walnut, or lime, &c. fried in oil, cost him but little, and answered his purpose better than cakes of wax, pitch, rosin, and all the supports of glass or silk, which he had employed before; and in case of necessity a cylinder of this prepared wood, or a globe turned out of it, will excite an electricity so strong, that you need not be at the trouble of exciting it with other bodies. Father Ammersin himself employs common wooden measures, such as are usually found in granaries, first boiled in oil, and afterwards mounted so as to be turned by his wheel.

The Abbé Nollet, being desirous of supporting the validity of some opinions of his, in regard to the nature and properties of electricity, desired of the Royal Academy, that a committee should be appointed to examine the truth of some experiments, which the Abbé considered as proofs of what he had established. A committee was accordingly appointed, which consisted of Messrs. Deparcieux, Fougeroux, Bezout, Tillet, and Brisson, who all attested to the academy, that the results of these experiments, at the making of which they were present, were such as the Abbé had foretold, in a memoir, which had been read to the academy; an attestation of which is given in this work, signed by M. de Fouchy, secretary to the academy, and is dated 10th April 1760.

These experiments are 60 in number, some of which are subdivided to more subordinate ones, and are most of them exceedingly well chosen. They tend to prove the simultaneous affluence and effluence of the electric matter, a doctrine long since espoused, and very well supported by our author; but vehemently, and with much asperity, controverted by some gentlemen at Paris. For a detail of these experiments, I must refer you to the work itself; and as they without doubt are very fairly stated, every person conversant in these inquiries will carefully consider them, and at the same time reflect how far the hypothesis is deducible from the phenomena.

LIV. The Case of a Man, whose Heart was found Enlarged to a very Uncommon Size. By Mr. Richard Pulteney. p. 344.

Thomas C. aged about 32 or 33 years, had the rickets in his infancy, and continued very weakly for several years after. In the winter of the year 1759, on taking cold, he was afflicted with peripneumonic and pleuritic symptoms; which had scarcely left him, when he was seized in the summer of the year 1760, after great exercise in walking, with a fever, and very violent rheumatism: this, after affecting most of his joints, remained the longest and most troublesome in his knees. When he was somewhat better of his rheumatism, but before the pain and stiffness of his joints had left him, he was advised to go into the cold bath: he did so; but on coming out again, instantly felt such an increased load, fainting, and anxiety about the præcordia, that he thought he should scarcely have recovered the shock it gave him; yet he ventured in again a day or 2 after; but experienced the former symptoms in an aggravated degree: and from this time he dated the disorder which terminated his life. A palpitation of the heart, to which he had been subject for some years before, became now much stronger, and gradually increased with his other complaints, to a very great degree. His rheumatism continued to affect his breast, and all his joints, particularly his knees; especially on taking cold, or any irregularity in the non-naturals, he became weaker, breathed shorter, especially on walking a little, or talking rather more or higher than usual, any of which exertions put him out of breath presently.

When he first applied to Mr. P., in the beginning of March 1761, he found him labouring under the above-mentioned complaints; and on examining his pulse, found it soft, and extremely quick: it commonly went at the rate of 110 in the morning, and in the evening 120 pulsations in a minute, as he repeatedly observed. The palpitation of the heart struck Mr. P. instantly, as it shook his whole body at every stroke. He could never observe any inequality of the intermittent kind in the pulse, under any the most accelerated motion of it, or in whatever situation the body was placed. At this time the chylopoietic organs were all tolerably good. Stimulating food, or fermented liquors, had for some time always increased his anxiety and load on his breast, and this experience had induced him to refrain from them. He had slept very ill for several months, sometimes not more than an hour or two during the whole course of the night. He could not sleep on the left side at all, and was always easiest in an erect posture. He was commonly awaked with a sense of suffocation from the vast load and oppression on his breast, and from the strength of the palpitation.

From his first application to Mr. P. he had no hopes of doing him any real service, as he thought it evident from his complaints, and particularly from the

great and uninterrupted palpitation, and the feel of the pulse, that there was something very extraordinarily disordered in the heart itself, or in some of the large vessels near it. The regularity of the pulse inclined him to suppose an aneurism rather than polypose affections. All this time however no outward appearance strengthened this supposition. No remedies alleviated his complaints in any degree, except bleeding, which afforded a relief; but very temporary, and weakened him too much to be repeated more than once. All that it seemed to do for him, was the procuring him rather more sleep the night after than he usually had, and easing a little tickling cough which had remained with him ever since the year 1759 at times; and particularly since his rheumatism, but which was never very troublesome.

Soon after Mr. P. first saw him his legs became œdematous, and by the beginning of April his thighs were much enlarged, and at length his belly in some degree. At this time he began to cough more, from having taken cold, inadvertently as he thought, but he soon expectorated freely. By the middle of April he was too weak to sit up, nor could he speak or stir without being ready to expire for want of breath. On the night of the 20th of April, as he was coughing, an hæmoptoe suffocated him instantly.

About 2 quarts of a thin coffee-coloured liquor were found in the cavity of the abdomen. The omentum was very small, perhaps it would not weigh more than 2 oz. The stomach and intestines were greatly inflated. In all other respects, the viscera of this cavity, as far as a hasty examination would permit them to observe, were in a sound state.

In the thorax they found the lungs very sound, but extremely turgid with blood: they adhered very firmly to the pleura on both sides, and particularly on the left, where the adhesion was almost total. The heart, as might be expected, appeared to be the organ principally affected. The pericardium adhered almost every where so close, as to form as it were the external coat of it. The heart itself was of an enormous size, and of a very pale colour, and loose and flaccid in its texture, to a very remarkable degree. Mr. P. could not find that either of the auricles or ventricles bore an extraordinary proportion to the other. The whole heart might be said to be entirely aneurismatical. The parietes were every where thin, in proportion to the size of the whole. There was no particular enlargement of the aorta, as far as he traced it, which he did to some distance; but its texture, as that of the heart, was very lax and flabby. He could not find the least polypose concretions in any part whatever. When the heart was cut short from the great vessels, emptied of the coagula, and washed as clean as possible, it weighed upwards of 28 oz. avoirdupois.

Observations.—The size of the human heart, in a natural state, is found to differ greatly in different subjects. And it is usually supposed, that the capacity

of the blood-vessels bears a general proportion to the size and capacity of the heart itself. Very few anatomists, in describing this organ, have estimated its size by its weight. Dr. Haller, where he treats so amply and professedly on the heart, does not, from his own knowledge, mention its weight. From Tabor, he says, it is estimated at 10 oz; but this is supposed to be when freed from the auricles, as well as the extremities of the larger vessels. Its mean weight by some other anatomists is reckoned at 13 oz.

Aneurisms of the heart, both with and without polypose concretions, are not unfrequent; many instances occur in the writers of observations. Dr. Douglas saw a young man, who died of a palpitation of the heart, the left ventricle of which was found 3 times larger than the right. This case bears considerable analogy to the instance before us; and is quoted, among several others, by the Baron Van Swieten, in treating on aneurisms of the heart. The baron also relates a case from Lancisi, in which the left ventricle was twice as large as the right, and the whole heart weighed $2\frac{1}{2}$ lb. Hoffman, in his Systema, when treating on the palpitation of the heart, gives us a case, where the heart was greatly distended; but he does not ascertain to what degree by any method whatever; he only says, cor miræ fuit magnitudinis.

De Haen, in his Ratio Medendi, tells us he was present at the opening of a man, whose heart was 3 times larger at least than in its natural state. The dilatation was in its left ventricle, which was so thin as to resemble a whitish membrane only; and the heart was broader at its apex than at its base.

De Haen likewise, in his Ratio Medendi, informs us that the heart of a woman, who died of a fever, with extreme debility, weighed 24 oz., even after it was washed, and wiped very dry. This increased weight and magnitude arose more particularly from the left ventricle. The extension of the ventricles was so great, that they both together contained more than a quart. Though this woman was no more than 37 years of age, the aorta at its base was degenerated into bone, and was 4 inches in circumference. Besides the whole portion of the aorta at its base being ossified, there were interspersed in several parts of its length, what our author calls insulæ osseæ. In one who lived so long as the excellent Wepfer, such appearances are not extraordinary; but in one so little advanced as the woman in question, these ossifications are very unusual.

It would be endless to quote instances of the preternatural dilatation of this organ: to name no more, we have a very recent and striking one of this kind, in the body of our late most gracious sovereign, whose sudden death was owing to the rupture of the right ventricle of the heart; a circumstance which cannot be conceived to have taken place without a previous gradual dilatation of the same, and that probably to a very considerable degree.

In cases of this kind, commonly one of the ventricles is found distended to a

monstrous size, while the rest of the heart remains nearly in its natural state. It is but rare perhaps that the heart is seen so equally and universally enlarged, as in the case under consideration. This man, Mr. P. observed, had the rickets when a child: in this disorder the whole system was found to be in a very lax debilitated state; and the heart was said to be so in particular. The constitutions of rickety children frequently amend as they grow up, and particularly about the age of puberty. But in this case Mr. P. thinks we may safely conclude, that this man's heart never recovered its due tone after he grew up. It is scarcely to be supposed that the heart could suffer so great an enlargement during the last year or two of his life only: the more so, as he remembered to have heard him say, that for many years before his death, a very little exercise put him out of breath. Doubtless it was increased greatly during the latter years of his life, by his business, which obliged him to exercise much, particularly in walking; so that before he got his rheumatism, he came home so weak, and so much fatigued with his usual day's exercise, that he has been almost unable to stir for a day or two. He adds to this the increased force that the heart sustained during the time he laboured under his inflammatory disorders, both before and after his rheumatism seized him.

The great increase of his disorder, on going into the cold bath is not surprizing. The shock of the cold water, and the resistance necessarily given by that means to the circulation, must occasion a vast surcharge of blood in the auricles and ventricles of the heart, already too weak to perform its office with sufficient power. Besides the impropriety of such a step, while there was reason to think that the inflammatory spissitude of the blood was by no means overcome, the preternatural distension was doubtless increased by this means.

Hence however may be deduced a useful hint in practice; namely, where from the state of the pulse, from a palpitation of the heart, a faint weak voice, an aptitude to fall into lipothymies from slight causes, or from the concurrence of any other symptoms, we have reason to suspect that the heart is too weak; in such cases, not to direct cold bathing, till the patient has been prepared for it, by going into water between the degrees of tepid and quite cold water; nay probably it might be better to wait, before cold bathing be prescribed at all, til the effect of medicines seems previously to have invigorated in some degree the cardiac system.

The considering the heart as a muscle capable, like all others, of great alteration respecting its tone; and at the same time that such alteration must essentially affect the whole animal economy, from the very great importance of the organ itself, is evidently of great use in medicine. It must assist us in accounting for several phenomena that occur in various disorders, which are utterly inexplicable by other means; and of consequence must lead to a more successful

practice. In nervous disorders, and in fevers of the putrid malignant kind for instance, we find the heart so extraordinarily weakened, that it is in many instances dangerous to subject the patient to an erect posture, even though it be but for a very little time. Syncopes, and even fatal deliquia and comatose affections, have been the consequence. In scurvies too, where the whole system is become very lax and tender, and has lost much of its tonic and vital elasticity, the same phenomena have occurred. In these cases the necessity of the horizontal, or at least the recumbent posture, is manifest; as it is obvious how much more force is requisite to throw the blood up into the head in an erect than in a horizontal position.

It is probable that the extreme weakness and slow recovery of some women, particularly such as are of a delicate constitution, after a hard labour, depends often on the weakness of the heart, occasioned by the force it sustained during the throws of labour. In these cases, though rest is among the first methods of recovery, yet Mr. P. thinks he had observed the use of the quinquina to be attended with good success.

To conclude, it is probable that cases of this kind occur much oftener than we are aware of; as doubtless the dissection of morbid bodies, were that but more frequently allowed of, would teach us. There is room to think that this is the case, though not in the degree of the instance before us, in almost all diseases arising from a weak and lax fibre. Cheselden tells us, in his Anatomy, that in persons "that died of a dropsy, he always observed the heart large, its fibres lax, and the vessels about it immoderately distended."

Aristotle expressly says, that timid people, and those of cold constitutions, have large hearts; on the contrary, that the bold, and those of a warm temperament, have small ones. Nor does this opinion of that excellent philosopher seem ill founded, as women, children, and weakly men, from whom much courage is not looked for, are lax fibred, and consequently more liable to an enlargement of this organ, than those who are robust and tense fibred, from whom a manly exertion of courage is more to be expected.

LV. On several Experiments in Electricity. By Edw. Delaval, Esq., F. R. S. p. 353.

It appears by the experiments mentioned in my letter, published in the 51st volume of the Philos. Trans. that stones, and other earthy substances, are convertible by several methods, and particularly by different degrees of heat, from non-electrics into electrics. Since that time it has been the opinion of some persons, that this change does not immediately depend on the heat, but only consequentially, by evaporating the moisture, which they suppose returns again on the bodies cooling.

That we may judge the better of this, Mr. D. mentions the circumstances of one of those experiments particularly. When a common tobacco-pipe, or any other slender body of the like kind, is heated red-hot, it conducts the electric fluid as perfectly as when cold: on cooling, it gradually arrives at its most perfect electric state in 2 minutes; and in less than 2 minutes more it entirely loses its electric property again, though at that time it is not cold; it cannot therefore in that interval have imbibed a moisture sufficient to have destroyed its electricity. Nor are any of the substances, employed in the experiment, of that kind of bodies, which are apt suddenly to draw moisture from the air.

In confirmation of particular bodies requiring particular degrees of heat, to render them electric or non-electric, independent of moisture, Mr. D. mentions a substance, which is affected by heat in an opposite manner to the former instances; for the degree of heat necessary to render the other substances electric, makes this non-electric. This substance is island crystal, well known for its singular property of a double refraction, on a piece of which he made the following observations. 1st. After this piece of crystal has been rubbed, when the heat of the air is moderate, it shows signs of electricity, though not very strong ones. 2d Obs. If the heat is increased, so as to be a little greater than that of the hand, it destroys its electric power entirely. 3d Obs. By cooling the stone again the electric power is restored. He immersed this piece of crystal into a vessel filled with quicksilver, and surrounded by ice, where it remained near 2 hours, when the weather was very cold; on taking it out with a pair of tongs, that it might not be altered by the heat of his hands, and rubbing it again, it was more strongly electric than he had at any other time experienced; but on placing it for a few minutes on the hearth, at some distance from the fire, its electric property was again destroyed, for rubbing would not occasion any signs of it. Thus we see two different kinds of fixed bodies, the one of which acquires an electric property, with the same heat with which another loses it; while a 3d set of substances, as glass, &c. retain their electricity through both the degrees of heat necessary to the other two.

Some pieces of island crystal, which he had procured from different places, had not the property of losing their electricity by a moderate heat. He had in particular a piece of that crystal, one part of which, when gently heated, becomes non-electric, while the other part, with the same heat, or even with a much greater one, remains perfectly electric. There are several other earthy substances he found, whose electricity is destroyed by very different degrees of heat.

LVI. Of an Encrinus, or Star-fish, with a jointed Stem, taken on the Coast of Barbadoes, which explains to what kind of Animal those Fossils belong, called

Starstones, Asteriæ, and Astropodia, which have been found in many Parts of this Kingdom. By John Ellis, Esq., F. R.S.* p. 357.

The writers on natural history have been much at a loss to discover to what kind of animals those petrified bodies have properly belonged, which are known to us by the name of trochites, entrochi, carpophylloides, encrini, asteriæ, &c. and therefore it is with the greater pleasure Mr. E. lays before the Royal Society a recent animal of the rarest of this class.

Mr. Mason of Barbadoes, in the month of May 1760, brought Mr. E. this rare lythophyton, as he called it. Dr. Bruce mentions, that they are the inhabitants of those seas, and that he is in hopes of sending over a more perfect specimen.

Mr. Guettard, that able and curious naturalist, has given, in the Memoirs of the Academy of Sciences at Paris, published in 1761, for the year 1755, a most minute description and dissection of an animal of this kind, from the curious cabinet of Madam Bois Jourdain at Paris; it was sent from Martinico by the name of palma marina: the head of it, being more perfect than this, has some resemblance to the branches of a palm tree.

As it comes nearest to the fossils called encrini, or lilii lapidei, Mr. E. still keeps that name, and calls it, encrinus, capite stellato ramoso-dichotomo, stipite pentagono equisetiformi.

The stem and head of this animal, in its present state, measures about 14 inches. The stem is about 13 inches in height, and about the third of an inch in diameter, lessening a little towards the top; it is formed of pentagonous joints, or vertebræ, placed regularly over each other, which are of a testaceous substance, and united by very thin cartilages, as appears by examining minutely the base of the lowest vertebra, where it is fastened to the starry indentures of the joint; this makes the vertebræ capable of bending at the will of the animal, in any direction.

If we examine the 5 furrows or channels along the stem, we shall discover a small hole between every vertebra, and in the centre of the base of the lowest, we shall find a small hole there, which probably communicates through the middle of all the vertebræ to the cavity in the centre of the head. Along this stem, at different distances, from an inch and quarter to a quarter of an inch in length, we observe many series of 5 cylindrical jointed arms, each series of equal length, and placed in a wheel or whirl-shaped form like the equisetum or horsetail plant. Each arm is inserted in one of the 5 cavities of a vertebra, and each joint into each other; that the upper end of one joint inclines over the lower end of the next to it, which it appears at the same time to inclose with a small margin.

* This curious zoophyte is the *Isis Asteria* of Linneus.

These joints are generally about $\frac{1}{14}$ of an inch in length, and the same in diameter, except a few near their insertion in the stem, which are shorter and thicker the nearer they are to it. We may plainly trace a small hole here through the midst of the joints, which communicates through the centre of the starry vertebræ in the main stem, to the hooked joint at the extremities of these arms.

On the under or inner side of those joints, that are near the end of the arms, we may discover 4 minute tubercles in every joint, 2 at each end; these are of the same testaceous substance with the rest of the joint. By means of this uneven surface, together with the hook, which the last joint forms, bending downwards, the animal can take a more secure hold of whatever it seizes. But as the stem of this animal appears evidently to be broken off short at the bottom, we must remain in doubt, whether it moves about in the sea, or is fixed to rocks and shells by a base, like corals, sponges, and keratophytons, till some future discovery shall clear up this matter more to our satisfaction.

In examining the main stem or column, we may observe some single joints or vertebræ projecting a little farther than the rest. There are generally 3 or 4 of these in each division, between the whirls of arms; the angular parts of these joints end in small round knobs; but the knobs at the corners of the vertebra, immediately under the head of the animal, are remarkably larger than the rest. The joints or vertebræ of the stem vary in thickness, as well as in diameter; the common thickness is about $\frac{1}{10}$ of an inch, but in the last 4 divisions approaching towards the head, they gradually diminish, till they become extremely thin.

We now come to what is called the head, perhaps the body of the animal: for in the centre of this dry specimen, there still remains a cup of a crustaceous substance, and of an oval form, about an inch in length, three quarters of an inch over, and a quarter of an inch deep; in the centre of this, as was observed before, is a small hole, which apparently communicates with the internal part of the vertebræ of the stem: in this cup or cavity it is probable were the intestines and stomach of the animal, as in the asteria, called caput medusæ. This cup is supported by the bases of 6 dichotomous testaceous arms, or branches, perhaps 5 is the natural number, for one seems irregularly placed. These lower parts, or bases of the branching arms, consist of 3 joints each, and surround the cup, to which they seem united: each of these divide into 2 other jointed branches, that are round or convex on their under side, but flattish on the upper, with a deep groove running along the middle, which is furnished with 2 rows of suckers, as in the sepiæ and asteriæ. From the upper edges of each alternate joint of these branches, arise 2 rows of small jointed claws, like fingers; these 2 opposite rows bend in towards each other: each small branch, or finger,

is about half an inch long, and $\frac{1}{20}$ of an inch broad: the size of these joints diminishes a little till you come to the last joint, which ends in a point. Each of these joints is pointed at top, and being concave, embraces the lower convex part of the next above it: these are likewise furnished on their concave side with 2 rows of suckers, clasping together; they secure their prey with these opposite claws, or fingers.

Views of this curious animal are found in the French engraving of their encrinus before-mentioned.

LVII. Remarks on a Passage of the Editor of the Connoissance des Mouvements Célestes, pour l'Année 1762. *By Matthew Raper, Esq., F. R. S.* p. 366.

Sir Isaac Newton, in the 2d and 3d editions of his Principia, lib. iii, prop. 19, has mentioned Norwood's measure of a degree on the meridian, as taken about the year 1635. The editor of the Connoissance des Mouvements Célestes pour l'Année 1762, p. 196, has the following passage.

"On pretend aussi en Angleterre, que des l'année 1636, Norwood avoit trouvé le degré par des mesures prises entre Londres et Yorck de 57300, ou de 57400 toises; résultat, qui se trouveroit d'une exactitude bien singuliére pour ce tems la: mais un fait plus authentique c'est que Newton en 1666 jettant les premiers fondemens de son admirable système de la gravitation, n'avoit jamais oüi parler des mesures de Norwood, et supposoit, avec tous les pilotes de son tems, le degré de 60 milles Anglois, qui font 49200 toises."

Here this writer asserts, that Sir Isaac Newton had never heard of Norwood's measure in 1666, of which he can bring no proof, and would thence insinuate that probably there never was such a one, or at least not so early as is pretended. In either case, Sir Isaac, in the proposition above-mentioned, must have positively asserted what he did not know to be true, or knowingly have published a falsehood.

Norwood's book is intitled, The Seaman's Practice, containing a fundamental Problem in Navigation, experimentally verified, namely, touching the Compass of the Earth and Sea, and the Quantity of a Degree in our English Measures, &c. By Richard Norwood, Reader in the Mathematics. He tells us, that having observed the latitude of London in the year 1633, and that of York in 1635, he measured the distance of the two cities, in his return from York to London; and the account he gives of his measurement is so clear and ingenuous, that the reader will find no cause to doubt either his abilities or his fidelity.

The book was first published in the year 1636, and has since gone through many editions, the 8th being printed in 1668. The title above-mentioned is

likewise found verbatim in London's catalogue of the most vendible books in England, published in the year 1658, 12 years before Picard measured a degree in France, so that the authenticity of the fact, that Norwood's measure preceded Picard's, cannot be doubted.

The editor of the Connoissance, p. 195-196, has given a list of different measures of a degree, according to different authors, who had either actually attempted to measure one themselves, or had adopted the measure in this list for a true one. Among these he has most disingenuously put Sir Isaac Newton's name to a measure of 60 English statute miles; which must imply, that Sir Isaac believed this to be nearest the truth, till he knew of Picard's measure in 1670. Whereas it does not appear, nor is it at all probable, that he ever preferred that rude conjectural measure to the measures of Snellius, and others well known to the learned world before the year 1666; but being at that time retired from Cambridge, on account of the plague, and absent from his books, having occasion to use the diameter of the earth in a calculation, he took the common account in use among seamen, as Dr. Pemberton has related, in the preface to his View of Sir Isaac Newton's Philosophy. And this anecdote seems to be all the authority the French writer had for ascribing that measure to Sir Isaac Newton, and for asserting, that he had never heard of Norwood's measure in the year 1666.

If his view was to do honour to his own country, by depriving others of their due praise, the wiser part of his countrymen will not think themselves much obliged to him, well knowing that the reputation of a great kingdom, which has so long distinguished itself in Europe by men eminent in arts and arms, does not stand in need of the varnish of such ungenerous practices.

LVIII. *An Extract of a Letter of M. De la Lande, of the Royal Academy of Sciences at Paris, to Dr. Bevis, dated there March 26, 1762. Translated from the French.* p. 369.

I have received, with a great deal of gratitude, the Seaman's Practice, which you were so good as to send to me. I return you my most humble thanks for it. I had never heard that Norwood's measure had been printed so early as the year 1636; and I did not think, that before Newton, that is, before 1666, it was at all known. I assure you, that I will publish in our Memoires an extract of this book, in order to do homage to the labours of that celebrated Englishman, who had preceded us with regard to the figure of the earth. I am sorry that I have seemed to have been in doubt when I spoke of it, and that my book is already dispersed; but I shall find an opportunity to repair this another time. In the mean time, do me the justice to observe, that I did not say, that Norwood's measure did not exist, but only that Newton had no knowledge of it, as seems to result from the testimony of Dr. Pemberton, who relates, that Newton hav-

ing had the notion of the attraction of the earth on the moon, was diverted from pursuing it by observing, that the earth was too large not to produce a greater attraction. If any member of your illustrious Royal Society is offended with my reflections, I desire you to make my excuses to him, and to assure the Royal Society of my most humble respects.

LIX. Observation of the Transit of Venus over the Sun, June 6, 1761, at the Island of Rodrigues. By Mr. Pingré, of the Royal Academy of Sciences at Paris. Translated from the French by Matt. Maty, M. D., F. R. S. p. 371.

June 5, at about 18^h 30^m, the sun rose amidst very thick clouds. At 18^h 43^m 51^s, Venus was entirely on the sun's disk; the exterior limb of the planet being at the distance of at most 15″ from that of the sun. The intervening clouds did not permit him to measure the distance more exactly. He made use of an 18-foot refracting telescope. These are, a number of the distances of the nearest limbs of the sun and Venus. The diameter of Venus measured several times $54\frac{1}{2}''$.

0^h 34^m	47^s	The contact of the occidental limbs.
52	5	Venus, almost got off, is covered with a cloud.
52	23	It is still seen, but little; another cloud.
53	18	The sun's disk seems still a little altered: but this perception is faint, and a new cloud prevents making a better observation.
54	21	The transit is certainly ended.

The latitude of the observatory was 19° 40′ 40″ s. The variation of the magnetic needle was 10° 42′ N. W.

LX. Observations made at the Cape of Good Hope. By Mr. Charles Mason, and Mr. Dixon; reduced to Apparent Time by Mr. Mason. p. 378.

This is a kind of journal and register of all their observations, which are very numerous and miscellaneous, relating to their clocks, their instruments, and various proceedings, before and till after the transit. The instruments used were two reflecting telescopes, each 2 feet focal length, and magnifying 120 times, made by Short. A quadrant of 1 foot radius. And an astronomical clock made by Ellicott.

The sun's diameter measured..........	31′	33″.3.		
And that of Venus measured..........		59″.6.		
The time of internal contact.........	21^h	39^m	52^s	apparent time.
Ditto of the external...............	21	57	23	
The former by Mr. Dixon............	21	39	48	
The latter by the same	21	57	21	apparent time.
Latitude of the place	33°	55′	42″	south

LXI. Latitude of the Observatory at the Cape of Good Hope reduced from the Observations of different Stars. By Mr. Charles Mason. p. 395.

The latitude is here deduced from observations of 10 remarkable stars, the mean of all being 33° 55′ 42″ south.

LXII. An Observation of the same Transit of Venus over the Sun, June 6, 1761, at Madras. By the Rev. William Hirst, Chaplain of one of his Majesty's Ships in the East Indies. Dated Fort St. George, July 1, 1761. p. 396.

Mr. Hirst began to make observations for regulating his clock near 3 weeks before the day of the transit, by taking equal altitudes first, and then by meridional passages of Spica virginis, and of the sun; so that there can be no doubt as to the accuracy of his time. The place of his observation was fort St. George, on the top of the governor's house, the latitude, as determined by many observations made not long before, with an excellent quadrant, Mr. Hirst says is 13° 8′ N. and and he makes it $3^m\ 4^s$ of time eastward of Pondicherry. The telescope was a reflecter 2 feet long, made by Mr. Adams, of Fleet-street, London.

Sometime before 5 in the morning of the 6th of June, Mr. Hirst and the rest of the gentlemen, met on the terrace of the fort-house, and were at their glasses at the time the sun rose, lest Venus might enter the disk before the time calculated by the astronomers. The Jesuits had calculated the beginning for Pondicherry, at $6^h\ 57^m$. The London calculations, reduced to the meridian of fort St. George, gave it at $7^h\ 26^m\ 35^s$, apparent time.

The morning proved favourable to the utmost of their wishes, which the more increased their impatience. At length as Mr. Hirst was stedfastly looking at the under limb of the sun towards the south, where he expected the planet would enter, he plainly perceived a kind of penumbra, or dusky shade: on which he cried out, 'tis a-coming, and begged Mr. Call to take notice of it. Two or 3 seconds after this, namely, at $7^h\ 31^m\ 10^s$ apparent time, happened the first exterior contact of Venus with the sun, which all the 3 observers pronounced at the same instant as with one voice. Mr. Hirst is apprehensive that to be able to discern an atmosphere about a planet at so great a distance as Venus, may be regarded as chimerical; yet he affirms that such nebulosity was seen by them without presuming to assign the cause. They lost sight of this phenomenon as the planet entered the disk, nor could Mr. Hirst perceive it after the egress. The total ingress, or first internal contact, was determined with a precision equal to that of the first external contact, at $7^h\ 47^m\ 55^s$ apparent time.

Mr. Hirst thinks it necessary to take notice of another odd phenomenon. At the total immersion, the planet, instead of appearing truly circular, resembled more

the form of a bergamot pear, or as Governor Pigot then expressed it, looked like a ninepin; yet the preceding limb of Venus was extremely well defined. Mr. Hirst suspected this appearance might be owing to their telescopes not being nicely enough set to their focal lengths; accordingly he took care to try this several times during the transit, but found it not to be the case; for though the planet was as black as ink, and the whole body truly circular, just before the beginning of the egress, yet it was no sooner in contact with the sun's preceding limb, than it assumed the same figure as before, at the sun's subsequent limb: the subsequent limb of Venus keeping well defined and truly circular.

The beginning of the egress, or 2d interior contact, was observed only by Mr. Hirst and Mr. Call, Mr. Pigot having retired. This phasis came on at $1^h\ 39^m\ 38^s$ P.M. and the total egress by Mr. Hirst alone at $1^h\ 55^m\ 44^s$, apparent time, Mr. Cal unfortunately losing the solar image out of the field of his telescope.

LXIII. *An Account of a printed Memoir, in Latin, presented to the Royal Society intitled, De Veneris ac Solis congressu observatio, habita in astronomica specula Bononiensis Scientiarum Instituti, die,* 5 *Junii* 1761. *Auctore Eustachio Zanotto, ejusdem Instituti Astronomo, ac Regiæ utriusque Londinensis et Berolinensis Academiæ Socio. By Nathaniel Bliss, Savilian Professor of Geometry, and F.R.S.* p. 399.

The planet Venus has been so seldom observed in those circumstances, which are of the greatest use in determining some of the most essential elements of its motion, that all such observations, made by an accurate astronomer, cannot but be very acceptable to the public.

At Bologna on the night preceding the day of the transit, the weather was very unfavourable; but early in the morning the clouds which covered the whole hemisphere began to break, and were driven off towards the horizon by a gentle wind: so that the observations were retarded only during the space of about half an hour. Father Frisi, professor of mathematics at Pisa, and Signors Mathenci and Marini, assisted in making the observations; the two latter observing in the upper room of the observatory, together with Mr. Professor Zanotti; and Father Frisi, accompanied by the two professors of mathematics Signors Casali and Canterzani, in a lower chamber.

S. Zanotti, in order to determine the place of Venus on the sun, made use of a quadrant of $2\frac{1}{2}$ feet radius, in the telescope of which were placed two wires, the one in a horizontal, the other in a vertical direction: by observing the appulses of the limbs of the sun and Venus to these wires successively, no error from refraction can take place.

When the planet drew near to the edge of the sun's disk, the observers pre-

pared to determine the time of the two contacts, Professor Zanotti, with the telescope of the quadrant of 2½ feet focus, Professor Mathenci with the telescope of 22 feet, and Signor Marini with that of 10 feet.

The internal contact was observed

At 21^h	4^m	34^s	with the telescope of	2½ feet.
21	4	58	....	10
21	4	58	....	22

The external contact was observed

At 21^h	22^m	30^s	with the telescope of	2½ feet.
21	23	0	....	10
21	23	7	....	22

During the intervals of the observations made with the quadrant, the planet was always observed to be perfectly round, without any ring or nebulosity. The author then proceeds to determine, by calculations, (the method of which he has at large explained) the difference of longitude between the centres of the sun and Venus, and also the planet's latitude.

The following are the elements deduced from those observations, which were made at the distance of at least an hour and a half.

The horary motion of ♀ in longitude..............................	0°	3′	55″⅘
The horary motion in latitude..............................	0	0	36⅗
The true time of the conjunction of ⊙ and ♀..............................	18^h	26^m	0^s
The latitude of ♀ at the conjunction..............................	0°	9′	27⅚″

From these numbers the author deduced the following elements, by trigonometrical calculations.

The angle of the path of the ecliptic..............................	8°	49′	23″
The horary motion in the path..............................	0	3	58⅘
The part of the path between the middle of the transit and conjunction	0	1	27
The distance of the path from ⊙'s centre southwards..............................	0	9	21
The length of the path within the ⊙'s disk..............................	0	25	29½
The difference of longitude of ⊙ and ♀ at the ingress..............................	0	14	2
The difference of longitude at the egress..............................	0	11	9½
The latitude of ♀ at the ingress south..............................	0	7	17
The latitude of ♀ at the egress south..............................	0	11	12
The time of the middle of the transit..............................	18^h	4^m	6^s
The ingress of the centre of ♀ on the ⊙'s disk..............................	14	51	49
The egress of the centre of ♀..............................	21	16	23

It appears also by his calculation, that the time of the internal contact was accelerated 30″, and the last contact 18″, by parallax. The internal contact there-

fore, as seen from the centre of the earth, was at $21^h\ 5^m\ 28^s$, and the external contact was at $21^h\ 23^m\ 25^s$, and the egress of the planet's centre at $21^h\ 14_m\ 33^s$.

From the time of the planet's passage over the edge of the sun's disk, as seen from the earth's centre, the author very accurately determines the planet's diameter to be $57''\frac{2}{3}$.

The egress of the centre of Venus, as deduced from the position of its path, and from the other elements as related above, differs near 2 minutes from the observed time, when corrected by parallax, and reduced to the earth's centre. This difference is entirely to be attributed to an error in the motion of Venus in longitude, which perhaps could not be deduced with sufficient accuracy from these observations, and from a small error in some of the other elements; all which the author might have taken with the utmost accuracy from the tables either of Dr. Halley or M. Cassini. Perhaps also some part of this difference might arise from our ignorance of the true quantity of the sun's parallax.

Hitherto our author has given us those elements which might immediately be determined from his observations: the following are deduced from the tables. From the motion of Venus in latitude, it may readily be collected, that the planet was in its node on June 5, at $14^h\ 55^m\ 9^s$. The place of the sun at that time, according to the tables of the Abbé De la Caille, was in ♊ 14° 59′ $5''\frac{1}{2}$; and the planet's elongation from the sun at the same time was 1° 0′ 58″. Therefore the longitude of Venus, and also of the node, was in ♊ 13° 58′ $7''\frac{1}{2}$. The angle at the sun or the difference of the longitude of the planet and the earth, as seen from the sun, was 0° 24′ 15″ Therefore the longitude of the descending node of Venus as seen from the sun, was in ♐ 14° 34′ 50″.

The latitude of Venus as seen from the earth, at the time of the conjunction, was 0° 9′ $27''\frac{5}{6}$; by solving a triangle of which the computed distances of the earth and Venus from the sun constitute 2 sides, the angle at the sun, or the planet's heliocentric latitude, viz. 0° 3′ 46″, will be determined. With this heliocentric latitude, and the calculated place of the sun at the time of the conjunction, and the longitude of the node, as before laid down, from two sides of a spheric right-angled triangle, an angle may be computed which will express the inclination of the planet's orbit with the ecliptic. The place of the sun at the time of the conjunction, was in ♊ 15° 36′ 10″. The difference of the heliocentric longitude of the earth and the node was 1° 1′ 20″. Therefore the angle of the inclination of the orbit of Venus with the ecliptic is 3° 30′ 49″.

N. B. The several numbers contained in this paper, are taken from the correct numbers written in the margin of the printed memoir, with the author's own hand, and which seem to be the result of his latest calculations. And though his observations were made with great care, and faithfully calculated, yet the re-

sults will not be found so accurate as could be wished; since the latitude of Venus, deduced from these observations is in all probability 10″ or 12″ too little; a quantity, which must have a very sensible influence, both on the place of the node, and the inclination of the planet's orbit with the ecliptic; the latter of which ought to be deduced from observations made on the planet, when in its greatest latitudes.

In the lower chamber of the observatory, the observers made use of two telescopes, one of 6, the other of 8 feet, furnished with wires at half-right angles, in order to determine the place of Venus on the sun, by causing the sun's southern limb to run down one of the threads. The internal contact was observed there by 3 different telescopes, viz.

At $21^h\ 4^m\ 54^s$ with a telescope of 6 feet.
21 5 0 8
21 4 56 11

And the external contact was observed

At $21^h\ 22^m\ 53^s$ with a telescope of 6 feet
21 22 50 8
21 22 59 11

Professor Canterzani examined the observations by projection, and found them to agree very nearly with those made in the upper chamber by Signor Zanotti.

LXIV. Of a Burning Rock and Flaming Well, in the East Indies. By Mr. John Wood, from Calcutta. p. 415.

This account is from Mr. Plaisted, who went as surveyor, to take possession of the province of Chetagou, lately ceded to the Company. He writes, that the party entered the province of Chetagou, Jan. 1, 1761; and after travelling 54 miles, came to a city called Islamabad; beyond which, about a mile and half, they met with a burning rock, that continually emitted a weak flame from several parts, that might be extinguished for a time, and which he did in some places, for experiment sake, and found flame then breaking out from other parts of it a-new, in a very little time, after putting some out, which he kindled again also with straw; and the natives assured him, any extinguished part would kindle of itself, and flame out again in time. In passing again from Chetagou to Luckipore, he visited the rock again, which he adds is of a hard nature, seems to have no unctuous matter in it; nor would a piece of it, broken off near a flaming part, when heated red-hot, sweat, or discover any sulphureous matter, or even throw off any smoke. Six inches from the burning places, no heat was perceivable; and close to the rock was a small stream of water, that forms a

large cascade over part of it, during the rains. A small pagoda is lately built over this rock.

The other natural curiosity is a flaming well among the hills, about 4 miles to the southward of the rock, that blazes on the surface of the water, which the people of the country have inclosed with brick-work, in the form of a funnel or chimney, that draws the flame to a point, and makes it burn fiercer. The flame issues also with the water through some holes left in the brick inclosure, for conveying it to an adjacent cistern, like fire confined, and wanting vent. The water thus let out bubbles like a boiling pot, but close to the flame is only lukewarm. A pagoda, built also over the well, is in a constant misty fume, much like what rises from the waters at Bath in England, the taste of which, he says, is like this, having drank of both.

LXV. On the Extraordinary Agitation of the Waters in Mount's Bay, and other Places, March 31, 1761. *By the Rev. W. Borlase, M. A., F. R. S.* p. 418.

March 31, 1761, about 5 in the afternoon, there was an uncommon motion of the tide in Mount's-bay, Cornwall. It was full sea that day about half an hour after 12. After the tide had ebbed about 4 hours and half, instead of continuing to retreat gradually, as usual, till it had completed the 6 hours ebb, on a sudden it advanced as it is usually at the time of the moon, at an hour and half before high water. It then retreated near to the point of low water, then it advanced again, and retreated, making 5 advances, and as many recesses, in the space of one hour; viz. from about 5 to 6 o'clock; which was the whole time that these uncommon stretches of the tide continued. But the first motion was most considerable, the sea advancing the first time to a quarter ebb; whereas the 2d advance was but as far as the sea reaches at half ebb. At the first surge the waters rose at this place 6 feet perpendicular. At the pier of St. Michael's mount, 3 miles to the east of Penzance, the tide was observed, at the same time, to rise and fall about 4 feet. At Newlyn, a mile west of Penzance, the tide rose to the same height nearly as at Penzance. At Moushole pier, 3 miles s.w. of Penzance, it was only observed that the sea was in great agitation, and the fishing-boats in danger. At the islands of Scilly, the sea was judged to rise about 4 feet; but the agitation continued longer than in Mount's bay, viz. more than 2 hours.

On the coast of Scotland, from Fort Augustus on Lochness, we had accounts that on the same 31st of March, about 2 in the afternoon, Lochness rose on a sudden about 2 feet perpendicularly, and continued alternately rising and falling for the space of $\frac{3}{4}$ of an hour. The king's galley broke from her moorings, and

drove into the loch; several boats were cast very far upon dry land; in the middle of the loch the water swelled up like a mountain, extremely muddy, and the motion was attended with a very uncommon hollow sound. By a subsequent account from the same place, the water is said to have risen near 30 inches, between the hours of 12 and 1 P.M. and continued for near half an hour. It is added, an instance similar to this happened here at the time of the Lisbon earthquake in 1755.

On the coast of Ireland, from Cork, there was advice that on the same 31st of March, a quarter after noon, a shock of an earthquake was felt in that city, and between the gates only, allowed to be more violent than that of November 1, 1755. It did not continue above one minute, undulating from east to west, and vice versâ. At Kinsale, about 6 o'clock P.M. near dead low water, the tide rose suddenly on the strand, about 2 feet higher than it was, and went out again in the space of 4 minutes, with great force, which repeated several times; but the first was the greatest. At Amsterdam the branches in the synagogue were observed to vibrate between 1 and 2 o'clock. In the great church at Maesland-Slys, the branches moved about a foot from the perpendicular, and the vessels in the harbour were agitated.

But this earthquake was felt more violently on the ocean, between the coasts of Spain and the British channels. Ships on their passage from that part of the continent, many leagues to the westward of Cape Finisterre, felt an unusual agitation of the sea, as if they had struck on sunken rocks; the time agreeing with that of Cork and Fort Augustus. Captain Woodward, of the Expedition packet-boat, sailed from Lisbon March 29. On the 31st, soon after he had passed the rocks of Lisbon, in the morning, and almost calm, the sea swelled to a great degree, with a rumbling noise. The vessel was tossed about as if in a storm. The agitation continued 4 minutes.

The Gosport man of war, off the rock of Lisbon, at ¾ past 11 in the forenoon, felt two violent shocks of an earthquake; the first continued near a minute and half, the second not so long. Under the convoy of the Gosport, were several ships, all affected in the same manner. One off Lisbon felt the shock, attended with a noise, as if empty casks had been tossed about in the hold. The ship immediately made a good many inches of water, which proceeded from the seams opening; the vessel shaking in so dreadful a manner, that under the apprehensions that the ship was sinking, the people threw out their boat, and got all things ready for leaving her, then about 50 leagues from shore. In the latitude 43, not many leagues off shore, in her passage from Lisbon, the Amey of Bristol, Capt. Condon, felt a most violent shock. The concussion was so great, that it shook the needle off the spindle of the compass; and immediately after arose such a storm of wind and rain as he never before met with. The shock was

felt 10 minutes A.M. viz. half an hour before it was felt at Cork, and 5 hours before the waters rose at Kinsale, and in Mount's bay on the same day.

At the same time there was a violent earthquake at Lisbon, thought by some as severe as that of 1755, but the agitation more equable; consequently the damages not so deplorable, no lives lost, a few old houses shattered and thrown down, and some new ones cracked; the shock lasted between 3 and 5 minutes. But more particular is the account following, from an eye witness, in an English vessel then off Lisbon, i. e. lying before the city. 'On March 31st, at mid-day a severe shock, not so strong as that of 1755, but of longer duration. I saw the ruins of the last earthquake falling heap upon heap, and turning round beheld the rocks on the opposite side falling from the mountains, followed by a continual cry of the people; the buildings erected since 1755 damaged to the amount of 20,000 moidores at least. It lasted about 5 minutes, some say 7; the water in continual agitation all the afternoon, ebbing and flowing 3 or 4 feet in a very short time. At 12 at night, another shock, of short continuance; that night 3 more; did no damage. St. Ubes, 10 leagues distance to the south, has suffered much; and the villages to the north, as also a large convent. During the confusion in the city, 300 persons in the several jails gained their liberty; but all except 14 were secured again. The shock felt at Oporto was very strong; but did no damage. At a village about 20 miles distant, 3 or 4 houses were thrown down, by which several people were killed.'

It is said that the government of Portugal interposed; and to prevent the consequences of terror and fancy, (oftentimes as pernicious as realities) prohibited particular accounts, and even public thanksgivings.

At Madrid the violent shock lasted $2\frac{1}{2}$ minutes, shaking the houses, and throwing down the furniture; the inhabitants left their houses for fear of being buried in the ruins. It was remarked here as something extraordinary, that at the moment it was felt the atmosphere was quite calm, and a gentle shower fell.

At Madeira the shock was felt very violent at 10 o'clock A.M. It did no damage in the town; some rocks were split, and fell into the sea, and some of the roads of the island suffered. The greatest damage there sustained was the loss of one church, and 4 people killed, 2 of which were in a boat fishing near the shore, when the rocks fell.

At Barbadoes, about half past 4 P.M. near low water, the sea suddenly retired from the shore, and in about 3 minutes returned again, to the height of near 4 feet. This flux and reflux abated about 8 o'clock, so as scarcely to be perceived; but about 10 it increased again for some short time, then decreased till 6 the next morning. The flux and reflux was not always regular after the first 3 hours; it was sometimes performed in 3 minutes, and at other times in not less

than 6. On November 1, 1755, the day the earthquake happened at Lisbon, there was then here, in this same manner, an agitation of the water.'

At Fyal island, one of the Azores, and Terceira, the sea rose to a great height, and fell again so low that the quays were left dry; all the lighters and fishing-boats that were hauled up in Portorico, were carried down into the bay, and broken to pieces on the rocks. About a fortnight after, several earthquakes (successively more and more violent) ended not, till on the 20th 3 volcanos threw out as many rivers of lava, of near a mile in breadth, and 4 yards high, which threatened desolation to the whole country, and continued over-running every tree and house till the 24th.

From these accounts the violence of this earthquake was greatest at, or rather near Lisbon; perhaps at sea, in latitude between 43 and 44, and longitude about 11° 19′, where no tremors of land could be observed, and consequently the effects not so terrifying, nor perceived by so many, nor so destructive as if it had happened on the land, and contiguous to Lisbon, as that of 1755. The weather various in the different places, but mostly calm.

There was a great conformity between the effects of the earthquake of Nov. 1, 1755, and of this of March 31st, 1761; viz. in the extent; in the rise of the waters; in the calmness of the weather in most parts; and in the succession of time, beginning sooner at Lisbon than on the northern shores both times.

LXVI. Observations on a Clock of Mr. John Shelton, made at St. Helena. By the Rev. Nevil Maskelyne, M. A., F. R. S. p. 434.

The ancients were well acquainted with the rotundity of the earth, and were satisfied, that heavy bodies, in every place, had a tendency to its centre: but they had never any suspicion that the force of their tendency to the centre was greater in one country than another, or that when dropped from any height they fell faster in one latitude than another.

The great Huygens, who first set the doctrine of centrifugal forces in a clear light, saw plainly that the weight of bodies must naturally be less at the equator than at the poles; their great velocity there round the earth's axis taking off part of the weight which they acquire by their gravitation towards the earth's centre. And though he was not quite exact in settling the true proportion of the force of gravity in different latitudes; yet we owe this obligation to him, of having made the first discovery of a thing, which has since been the ground of so many theories and experiments. Mr. Richer, when he went to the island of Cayenne, made the first experimental proof of the decrease of gravity, in approaching the equator, though he was not led to it by Huygen's theory, which was then but lately published, and not so generally known; but from finding his clock, which

he had brought with him, go considerably slower than it had gone in France. But Sir Isaac Newton first of all showed, how the variation of gravity in different latitudes depended, not only on the centrifugal force, but also on the figure of the earth, which he likewise determined, as well as the proportion of the force of gravity in different latitudes, as far as theory alone could limit them.

Mr. Colin Campbell made a very curious experiment, of the diminution of gravity from London to Jamaica, by means of an excellent clock, made by Mr. Graham, an account of which is given by Dr. Bradley in N° 432 of the Philos. Trans., to which he has added a table of his own, expressing the proportion of the force of gravity in different latitudes, and has subjoined the proportion of the equatorial to the polar diameter of the earth, which should follow from the experiment, according to Sir Isaac Newton's principles. A like experiment was made by the learned French astronomers, who went to the polar circle, to measure the length of a degree of the meridian, by a clock made by the same excellent artist, expressly for the same purpose. We are likewise obliged to the gentlemen of the R.A.S. at Paris, for several experiments made by them, in order to determine the force of gravity in different places, by measuring the length of the second pendulum.

Mr. M. could not fail of being desirous of improving the opportunity, which his voyage to St. Helena afforded, of examining this curious point, among other experiments: and the R.S. were pleased to furnish him with an excellent clock, with a gridiron pendulum adapted to it, executed by that diligent and ingenious artist Mr. John Shelton, for that purpose. Dr. Bradley set the clock up at the Royal Observatory at Greenwich, and there examined its going, where he found that it lost 11 seconds per day on sidereal time, the thermometer of Fahrenheit's construction, which was placed withinside of the clock-case, standing about 50 degrees, at a medium, during the time in which the experiment was made.

Soon after his arrival at St. Helena he set up the clock in the valley near James's Fort, in a place elevated 85 feet above the level of the sea; and by a proper meridian mark, and a transit instrument, he observed the transits in the annexed table; where the first column expresses the day of the month of the observation; the 2d the time shown by the clock, at the instant of the transit of the sun's centre, across the middle vertical wire of the instrument on that day; the 3d column is made from the 2d, by applying the equation of time to it. Therefore the differences between the numbers in the 2d column show the rate at which the clock gains on the sun; and the differences between the numbers in the 3d column show the rate at which the clock gains on the mean time, the thermometer being at 71°, being on a medium, at the rate of $1^m\ 59^s.2$ per day.

1761.	Time by the clock at the sun's transit.	Time by the clock, with the equation of time applied.
April 25	$0^h\ 11^m\ 30^s$	$0^h\ 13^m\ 47^s$
27	15 10½	17 48
29	18 49	21 45.1
30	20 40½	23 44.8
May 3	26 6¼	29 42.1
6	31 50¾	35 39¼
8	35 45½	39 36.1

Allowing therefore the clock to get $1^m\ 59^s$ in a mean solar day, on mean solar time, in $23^h\ 56^m\ 4^s$ of mean solar time, or in a sidereal day; it will get only $1^m\ 58^s\frac{2}{3}$ on mean solar time: but a clock adjusted to mean solar time loses $3^m\ 56^s$ by the stars in a sidereal day, therefore the clock loses $1^m\ 57^s\frac{1}{3}$ of sidereal time, in one revolution of the stars; which agrees exactly with what he found by the transits ε, υ, α, and ζ Leonis.

The same clock at Greenwich, with the pendulum adjusted to the same length, lost 11^s on the stars in a sidereal day. Therefore the force of gravity at Greenwich is to the force of gravity at St. Helena as the square of $23^h\ 59^m\ 49^s$ to the square of $23^h\ 58^m\ 2^s\frac{2}{3}$ :: 10000000 : 9975405. The extent of the vibrations of the pendulum here, as well as in England, is exactly $1°\ 45'$ on each side of the perpendicular, according to the divided arch, which is at the bottom of the pendulum.

Mr. M. excuses himself from attempting to deduce any consequences at present from the above observations, either with respect to the law which the force of gravity observes in its changes in different latitudes, or with respect to the figure of the earth, which it has been supposed might be determined from experiments of this kind alone, independently of any others, the great Sir Isaac Newton having himself set us the example. If the body of the earth were homogeneous throughout, not only the figure of the earth, but also the law of the variations of gravity in different latitudes, would be given, and would be the same as Sir Isaac Newton has described them. But if the earth be not homogeneous, and there seems great reason, from late experiments, to doubt if it be so, we can form no certain conclusions concerning the figure of the earth, from knowing the force of gravity in different latitudes; as this force must depend not only on the external figure, but also on the internal constitution and density of the earth. Many more experiments, not only of the kind, but also of other different kinds, may be necessary before we shall be able to infer any thing with certainty concerning the internal constitution of the earth, or even to determine its external figure. But every experiment is useful which tends to throw a light over this intricate subject, and to show the perfect agreement of the laws of nature with the actual constitution of things.

LXVII. Observations on some Gems Similar to the Tourmalin. By Mr. Benjamin Wilson. F.R.S. p. 443.

Mr. W. here mentions that he had met with several gems of different sizes and colours, that resemble the tourmalin in regard to electrical experiments. The most beautiful of them were something like the ruby, others were more pale, and there was one inclining to the orange colour. In point of hardness and lustre they were nearly the same with the topaz.

Six of these gems were cut brilliant fashion, and 3 retained their natural

shape. Of the 6, when heated properly, and while they were cooling, 3 were electrified plus, and 2 minus at the table surface, and at the collet or opposite surface the 3 were electrified minus and the 2 plus. The 6th, which was the largest, and of an oblong shape, appeared to be electrified plus near one end, and at the other end, in the opposite part, minus. These instances he thinks are further proofs of the law observed in the tourmalin, viz. that the electric fluid flows to or fro, in one invariable line, according to the circumstances attending the experiment. See the letter to Dr. Heberden, Phil. Trans., vol. 51.

From the contrary appearances happening with gems of the same shape, it is now abundantly evident that the direction of the fluid does not depend on the external figure of the gem, but on some particular internal make or constitution of it. And that there is some such natural disposition in all gems affording these appearances, may be collected from another curious specimen of the tourmalin kind; which is green, and formed in long slender crystals with several sides, many of which are found sticking together, and are brought from South America.

Mr. E. M. da Costa, member of the R. S., furnished Mr. W. with a parcel of these uncommon crystals the 12th of November 1761, and desired he would try whether they afforded the electric effects of the tourmalin. They were examined the same evening, when he was agreeably surprized to find them, not only like tourmalins in regard to electric appearances, but that the direction of the electric fluid moving in them, is always along the grain or shootings of the crystals; one end being electrified plus, and the other end minus. And that the fluid is more disposed to pass in that direction than in any other, may be further collected from what has been observed on the grain of the loadstone by Dr. Knight; for though the magnetic poles of a natural loadstone may be varied in any direction, yet the same loadstone admits of being made much more magnetical along the grain than across it.

LXVIII. Observations on the Tides in the Straits of Gibraltar. By Henry More, Esq. p. 447.

That a very strong tide, at the rate of some knots, sets from the strait into and out of Gibraltar bay, from Cabrita and Europa points, is notorious. That this stream out and in by Europa is a mile or better wide, he had frequently observed. At the same time that the tide has been pouring into the bay, round Europa, he had remarked, both from the high shore and in boats, another stream in the offing, going the contrary way.

As a further confirmation of his idea, relating to the mid-stream: being on guard at Europa, in the forenoon, there came a Spanish xebeck from the west, with little wind, and in time was becalmed right off a rock he sat on; where he

continued almost the whole day to observe her, driving back again with a mid-stream, to appearance half-channel over, and edging outwards towards the Barbary side; when about 7 in the evening, with little or no wind as before, she returned at a great rate, and so continued till night and distance hid her in the Mediterranean.

It seems to be Mr. M.'s opinion that there are different streams or currents setting opposite ways in the straits at the same time.

LXIX. The Case of a Young Man Stupified by the Smoke of Sea-coal. By Dr. Frewen of Sussex. p. 454.

Wm. Colebrook, 17 years of age, was left alone to take care of his master's vessel in Rye harbour, the 4th of June 1761; and shutting up all close, at 9 o'clock in the evening, he laid himself down to sleep in a small cabin, where there had been a sea coal fire, which was not properly extinguished, and the chimney place being stopped, it soon became full of smoke; the effect of which, when the people came on board next morning, proved to have been so powerful as to render him totally deprived of all the sensible motions of the body, excepting those of the heart and lungs. The cause of this stupor being presently suspected, he was brought out upon the deck, in hopes the fresh air would prove of service; but neither that, nor bleeding, blistering, or any other applications they made use of, assisted him in the least under this torpid situation. Being brought home to his master's house about noon, Dr. F. visited him, and found him in the same soporous, apoplectic state, with a feeble pulse, respiration laboured and difficult, a rattling in his throat, and utterly void of all sensation. He appeared much like one he had seen who had taken an over-dose of opium, and died of it.

Dr. F. strongly recommended plunging this patient into a cold bath, which being complied with, and done as expeditiously as it could be, was attended with a success even beyond his expectations. Immediately on the immersion he opened his eyes and mouth, and shut them again. He was then instantly put to bed naked between the blankets; and in a very few minutes time a very great and universal sweat came on him, which continued for many hours. In the evening he was first perceived to move a little, seemingly as if disturbed by the roughness of the blankets stimulating his skin: a while after he opened his eyes, and looked a little about him in a confused manner. Some time after, he became more sensible and spoke; but could only give the short answers, yes and no. His respiration was still difficult and very laborious; but his pulse was stronger and fuller; on which account Dr. F. ordered a little blood to be taken away; and he took frequently a very little at a time, of some sweet oil. For his ordinary drink, Dr. F. directed boiling water poured upon bread, with a little white wine, lemon juice, and sugar,

of which at first he took but very little at a time, and afterwards more in quantity as he could get it down.

The next day Dr. F. found him much better, when he sat up, talked, and drank some tea, His breathing was easier, but he complained of a short troublesome cough and hoarseness; for which he ordered him a smooth pectoral linctus: and a lenient purging draught was also given him, which had the desired effect. He continued getting better for a day or two, when Dr. F. called on him again, and finding his cough and hoarseness still remain, with a little shortness of breath, he directed him pills of millepedes and gum ammoniac made up with bal. sulph. to be taken twice a day, drinking warm milk after them; by which means he got perfectly well and went to sea in 12 days.

LXX. A Letter from Benjamin Franklin, LL. D., and F.R.S. Dated Craven-Street, Feb. 4, 1762. p. 456.

Mr. Canton did me the favour to show me the ingenious experiments he has described in the inclosed letter. They succeeded perfectly as he has related them; and I imagine the communication of them must be agreeable to the curious in this branch of natural knowledge.

LXXI. A Letter from John Canton, M.A., and F.R.S., to Benjamin Franklin, LL.D. and F.R.S. containing some Remarks on Mr. Delaval's Electrical Experiments. p. 457.

Mr. Delaval, in his curious electrical experiments, found that Portland stone, common tobacco-pipe, &c. would readily conduct the electrical fluid, when very hot, or when quite cold: but were non-conductors in an intermediate state. As no one has yet attempted to account for this, Mr. D. submits the following solution to Mr. F.'s judgment

The stone tobacco-pipe, wood, &c. I apprehend, says Mr. D. conduct when cold, by the moisture they contain in that state; when their moisture is evaporated by heat, they become non-conductors; and when they are made very hot, the hot air at, or near their surfaces, will conduct and the bodies appear to be conductors again.

To prove that hot air will conduct the electrical fluid, let the end of a poker, when red-hot, be brought but for a moment within 3 or 4 inches of a small electrified body, and its electrical power will be almost if not entirely destroyed. And if excited amber, &c. be held within an inch of the flame of a candle, it will lose its electricity before it has acquired a sensible degree of heat.*

* I have observed also, that the tourmalin, Brazil topaz, and Brazil emerald, will give much stronger signs of electricity while cooling, after they have been held about a minute within 2 inches of an almost surrounding fire, where the air is a conductor, than they ever will after heating them in boiling water. And if both sides of either of those stones be equally heated, but in a less degree than

That glass is a conductor in damp weather, on account of the moisture on its surface, is well known; as also that warming it a little will render it a non-conductor; and that a great degree of heat will make it seem to be a conductor again. Now tobacco-pipe, wood, &c. will not only attract the moisture of the air to their surfaces, but will also absorb it; whence they are conductors in dry weather; and require more heat than glass, as well as a longer continuance in it, to render them non-conductors. It is remarkable that tobacco-pipe after it begins to cool, will become a conductor again sooner than most other substances and much sooner than wood. The cause of this appears to me to be the tobacco-pipe's absorbing the moisture of the air faster than most other substances, and much faster than wood; for the surfaces of tobacco-pipe and wood being wetted, the surface of the wood will continue wet much longer than the surface of the tobacco-pipe.

That tobacco-pipe does not become a non-conductor by a particular degree of heat without evaporating its moisture is evident, from the following experiments. If 3 or 4 inches of one end of a tobacco-pipe, of more than a foot in length, be made red-hot, without sensibly heating the other end, this pipe will prove a ready conductor, through the hot air surrounding one part of it, and the moisture contained in the other; though some part of it must have the degree of heat of a non-conductor. But if the whole pipe be made red-hot, and suffered to cool till it has only superficial moisture enough to make it a good conductor, and then 3 or 4 inches of one end be again made red-hot, it will become a non-conductor. And if a nail be placed at or near each end of a longish solid piece of any of the absorbent bodies above-mentioned, so that the point of each nail may be about half the thickness of the body within its surface; this body by heat may be made a non-conductor externally or superficially, while it remains a good conductor internally: for the electric fluid will pass readily from one nail to the other, through the middle of the body, when it will not pass on its surface; and even when the internal parts of the body are in an equal degree of heat with the external; as they must soon be after it begins to cool. But if the same body be exposed for a short time, to a greater degree of heat than before; or if it be kept longer in the same heat, it will become a non-conductor entirely.

P. S. Having formerly observed that the friction between mercury and glass in vacuo, would not only produce the light of electricity, as in the luminous baro-

will make the surrounding air a conductor, the electricity of each side, whether plus or minus, will continue so, all the time the stone is both heating and cooling, but will increase while it is heating, and decrease while it is cooling. Whereas, if the heat be sufficient to make the surrounding air conduct the electric fluid from the positive side of the stone to the negative side of it, while heating; the electricity of each side will increase, while the stone is cooling, and be contrary to what it was, while the stone was heating. See the Phil. Trans. vol. LI. p. 403 and 404.—Orig.

meter, or within an evacuated glass ball, but would also electrify the glass on the outside; I immerged a piece of dry glass in a basin of mercury, and found that by taking it out the mercury was electrified minus, and the glass electrified plus to a considerable degree. I found also that amber, sealing-wax, and island crystal, when taken out of mercury, were all electrified positively.* How does it then appear that the electricty which was observed on rubbing the last mentioned substance after it was taken out of mercury surrounded by ice, was owing to cold, and not to the friction between it and the mercury in taking it out? Island crystal when warm is a non-conductor, and all non-conductors may be excited with proper rubbers.

LXXII. *An Attempt to assign the Cause, why the Sun and Moon appear to the naked Eye larger when they are near the Horizon. By Mr. Samuel Dunn.* p. 462.

The sun and the moon when they are in or near the horizon, appear to the naked eye, so very large in comparison with their apparent magnitude, when they are in the zenith, or somewhat elevated, that several learned men have been led to inquire into the cause of this phenomenon, and after endeavouring to find certain reasons founded on the principles of physics, they have at last pronounced this phenomenon as a mere optical illusion.

The principal dissertations conducive to give any information on this subject, or helping to throw any light on the same, have been those printed in the Transactions of the Royal Society, the Academy of Sciences at Paris, the German Acts, and Dr. Smith's Optics; but as all these accounts had not given Mr. D. satisfaction, curiosity induced him to inquire after the cause of this singular phenomenon in a manner somewhat different from what others had done before, and by such experiments and observations as have appeared pertinent.

From the common appearance of the sun near the horizon, and other like circumstances, Mr. D. first began to suspect that a sudden dip of the sun into the horizontal vapours might some how be the cause of a sudden apparent change of magnitude, though the horizontal vapours had been disallowed to be able to produce any other than a refraction in a vertical direction; and reducing things to calculation, he found that, from the time when the sun is within a diameter or two of the horizon, to the time when he is a semi-diameter below the horizon, the sun's rays become passable through such a length of medium, reckoning in the direction of the rays, that the total quantity of medium, (reckoning both depth

* A small quantity of an amalgama, or mixture of mercury and tin, with a very little chalk or whiting, being rubbed on the cushion of a globe, or on the oiled silk-rubber of a tube, will excite the globe or tube to a great degree, with very little friction; especially if the rubbers be made more damp, or dry, as occasion may require.—Orig.

and density) through which the rays then pass, being compared with the like total depth and density through which they pass at several elevations, it was proportionable to the difference of apparent magnitude, as appearing to the naked eye. This circumstance of sudden increase and decrease of apparent magnitude, and as sudden decrease and increase of light, (for they both go together) seemed no improbable cause of the phenomenon, though he could not then perceive how such vapours might contribute toward enlarging the diameter of the sun apparently in a horizontal direction.

He therefore examined the sun's disk again and again, by the naked eye, and by telescopes, at different altitudes, and, among several circumstances, found the solar maculæ appear larger and plainer to the naked eye, and through a telescope, the sun being near the horizon, than they had appeared the same days when the sun was on the meridian, and to appearance more strongly defined, yet obscured.

Before sun-rising, when the sun has been near the tropic, and the sky at the utmost extent of the horizon has appeared very clear, and when certain fogs have appeared in strata placed alternately between the hills, and over intervening rivers, valleys, &c. so as to admit a sight of the rising sun over those fogs, Mr. D. had often observed, with admiration, the most distant trees and bushments, which at other times have appeared small to the naked eye, but while the sun has been passing along a little beneath the horizon, obliquely under them, just before sun-rising, when the sun has been thus approaching towards and beneath any trees and bushments, they have grown apparently very large to the naked eye, and also through the telescope; and they have lost that apparent largeness as the sun has been passed by them. Thus a few trees standing together on rising ground, at the distance of a few miles, have appeared to grow up into an apparent mountain. Such apparent mountains formed from trees, put on all forms and shapes, as sloping, perpendicular, overleaning; but soon recover their natural appearance, when the sun is passed by them, or got above the horizon.

Mountains themselves at a distance sometimes appear larger than at other times. Beasts and cattle in the midst of, and being surrounded with water, appear nearer to us than when no water surrounds them. Cattle, houses, trees, all objects on the summit of a hill, when seen though a fog, and at a proper distance, appear enlarged. All bodies admit of larger apparent magnitudes, when seen through some mediums, than others. But more particularly,

He took a cylindrical glass vessel about 2 feet high, and having graduated its sides to inches, he placed it upright on a table, with a piece of paper under the bottom of the glass, on which paper were drawn parallel right lines, at a proper distance from each other; and having placed a shilling at the bottom of the ves-

sel, it was nearly as low as the paper. Pouring water into the vessel, and viewing the shilling through the medium of water, with one eye, while he beheld with the other eye, where the edges of the shilling were projected on the paper, and its parallels, he found the shilling appear larger, at every additional inch depth of the water; and this was the case if either eye was used; and the same when the eye was removed far from the surface, or near to it, or in any position to it.

He took large vessels, filled them with water, and placed different bodies at the bottom of those vessels. It always followed that the greater depth of the water he looked through, in the direction from his eye, to the objects in the water, the nearer those objects appeared to him. Thus light bodies appeared more mellow and faint, and dark bodies rather better defined, than out of the water, when they were not deeply immersed. And thus they appeared, under whatever directions or positions he viewed the bodies.

He placed different bodies in proper vessels of fair water, and immersed his face in the water; viewing the bodies in and through the water, they all appeared plain, when not too far from the eye, and though a little hazy at the edges. they appeared much enlarged, and always larger through a greater depth of water. Thus a shilling appeared nearly as large as a half crown, within a red glowing arch on that side opposite to the sun, when the sun shone on the water. From this experiment he concluded, that divers see light objects not only larger, but very distinctly in the water.

These and several other circumstances being considered, they left him with but little doubt, whether the atmosphere refracts horizontally or not, as some protuberances observed in the sun's limb must have been wholly owing to such a cause, and the nearly allied strata in the atmosphere. That the apparently formed mountains of trees and bushments at sun-rising, so easily comparable with other trees and bushments of equal magnitude at other times, but in their affected state as much larger, must also be owing to the same cause. He therefore concluded that these were proofs that objects seen through a medium of greater depth or density, do appear more large; and that therefore not only the sun and moon, but that all other objects seen at great distances under a horizontal direction, do appear larger to the naked eye, than objects of equal magnitude and distance appear when seen under a vertical direction.

Though the quantity of medium, with its density, be here mentioned, as if it was the efficient cause of this effect, possibly it may be some other cause in the horizontal vapours, water, and other mediums which produce effects nearly proportionate to the difference arising from a comparison of the quantity of medium or density. Whether this effect arises from density or rarity, reflection, refraction, or inflection, acceleration, retardation, or absorbency of the rays, seems to deserve a proper inquiry. What others may find to be the cause of this phe-

nomena, he cannot determine; to him it has seemed most natural that the rays under the foregoing circumstances, first become obstructed, and many of them wholly absorbed, the rest proceeding with a retarded motion, are thereby first more reflected, and then less refracted through the humours of the eye; and lastly, the image on the retina becomes hereby enlarged. In other words, certain accidents making the rays more divergent than they otherwise would be, at their entrance into the eye, seem to Mr. D. to be the cause of these and other like appearances.

LXXIII. Extract of a Letter from Mr. John Bartram, of Philadelphia, to Benjamin Franklin, LL. D., F. R. S., relating to a Remarkable Aurora Borealis. Dated Philadelphia, Nov. 12, 1757. p. 474.

Here is a visible aurora borealis; at 7 o'clock it was about 2 hours high, to the northward pretty bright. Soon after daylight disappeared it was much more east, where it was redder, with some faint streamers, whose points reached near 45 degrees elevation, which soon disappeared, and the light descended by degrees under the pole, and by 10 o'clock was nearly extinct.

Extract of the Answer to the above Letter. Dated London, Jan. 11, 1758.

I thank you for your account of the Aurora. A very considerable one appeared here the same evening, being Saturday, Nov. 12. I did not see it, but have heard of it from several. If it was the same that you saw, it must have been very high, or very extensive, as the two places are 1000 leagues asunder.

LXXIV. Observations on Noxious Animals in England. By the Rev. Richard Forster, M.A., Rector of Shefford in Bucks. p. 475.

This paper proves what is now perfectly well known, viz, that the slow-worm is an innoxious animal, and its bite attended with no ill consequences.

The author relates 2 cases, both of which happened under his own inspection, and which were productive of no other mischief than that of momentary alarm.

LXXV. On the Extraordinary Agitation of the Sea at Barbadoes, March* 31, 1761; *and an Epidemical Disorder in that Island. By Mr. A. Mason.* p. 477.

There was here a very extraordinary motion of the sea, March 31, not unlike that remarked here on the dreadful day of calamity which happened at Lisbon; with this difference, the last was not so sudden as the former in the flux and reflux: which sufficiently shows, that the shock must have been greater that occasioned it, as most likely they proceeded from the same cause, viz. that of an

* See another account of this agitation at p. 601, of this volume.

earthquake, at some place under the surface of the sea. The tide ebbed and flowed, in about 8 minutes, between 18 inches and 2 feet, and continued so for 3 hours, regularly decreasing till night, when it was no more observable.

It is very remarkable, that since that time the island had been in a very deplorable condition, having suffered under the severest colds that had been ever known. The distress had been so general, that $\frac{19}{20}$ of the inhabitants of the island had felt the effects of the contagion; and to some it had been repeated several times. Few however had died of it. The leeward islands had not escaped it, having raged there more violently and more fatal. His majesty's ships had severely felt the effects of it, some of them not being capable of keeping the seas, for want of men fit for service. This happening at a season of the year remarkably the healthiest, made it the more surprizing. Bleeding had been found of great efficacy, and used, both by way of prevention and cure, with great success.

LXXVI. Observations on Auroræ Boreales in Sweden. By Mr. Torbern Bergman, of Upsal. p. 479.

These are observations on the ordinary appearances of auroræ boreales, and not of importance to be reprinted.

LXXVII. Of the Double Refractions in Crystals. By Father John Beccaria, Professor of Experimental Philosophy at Turin. p. 486.

This is now a very common and well known property in certain crystals.

LXXVIII. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1761, *pursuant to the Direction of Sir Hans Sloane, Baronet. By John Wilmer, M. D.* p. 491.

This is the 40th presentation of this kind, completing to the number of 2000 different plants.

LXXIX. An Account of a Work, intitled, Jacobi Christiani Schaeffer Icones et Descriptio Fungorum quorundam singularium et memorabilium; simul Fungorum Bavariæ Icones nativis coloribus expressæ editioni, jam paratæ, propediem evulgandæ, denuntiantur. By Mr. William Hudson, F. R. S.* p. 495.

This treatise consists of 16 pages, and is divided into 2 parts: the first con-

* Mr. Hudson was by profession an apothecary in the metropolis, and on the institution of the British Museum was made one of the assistant librarians in the year 1756. He resigned this office however in 1758 in order to pursue his profession. In 1762 he published the first edition of his well known work the Flora Anglica, in which the indigenous plants of England were arranged according

tains the figures and descriptions of some singular and remarkable fungi; the 2d contains proposals for publishing the figures of all the fungi growing in Bavaria, coloured after nature. The titlepage farther sets forth, that the materials for this work were already provided, and that it was carried on under the direction, and at the expence, of the Electoral Academy at Munich in Bavaria.

The author begins, by thinking it would be acceptable to the public, if he prefixed something of a new and singular nature to the proposals; to which purpose he thought nothing could be better adapted than coloured figures, and descriptions of some remarkable fungi, which had lately fallen in his way. These were selected from a number of others, and according to him, serve to illustrate and confirm an hypothesis he had adopted, and by which the existence of seeds, and consequently the propagation of the fungi, are rendered doubtful.

The fungi which he has given figures of, and which he uses to confirm his opinion, are certainly very irregular, and perhaps not easy to be accounted for. In the 3 first of them, more than one fungus grows from one common base, or stem; and what seems extraordinary to the author is, that part of the base of one of the fungi does not touch the ground, but rests on the pileus, or cap of the other, and consequently cannot receive from the ground what is necessary for its rise, evolution, and nourishment. Mr. H. remarks that he does not see the force of this argument; for why may not one common stem, the bottom of which is fixed in the ground, give nourishment to each of the fungi, though part of the stem of one rests on the pileus of the other? Does not common experience prove, that nourishment can be conveyed through imperfect branches, even when such parts as would be judged at first sight to be necessary to convey the nutritive pieces are wanting? This is the case of trees, when all the bark is stripped off quite round a branch, and yet that branch shall bear flowers and fruit in great quantity. The 4th figure exhibits 2 fungi, out of the pileus of which grows another complete fungus, with stem, pileus, and lamellæ, of the same kind with the lower one in every respect. However extraordinary this may be, yet it seems by no means to prove his hypothesis, or even suggest any reason for a new one, about the propagation of the fungi; for why may not the cap of the fungus afford as proper a bed for a seed that happens to be lodged there, as the earth itself? We know that a birch tree has often been found growing out of the head of an oak, yet no one suspects from thence, that the birch is propagated without seed. This author indeed seems to be sensible himself of the insufficiency of his arguments, hitherto made use of, to prove his new hypothesis, and suspects, he says, had he nothing more to produce in its favour, people

to the Linnæan system. This work met with much approbation, and a second improved edition was published in 1778.

might be apt to make objections; but luckily he has 2 other fungi, that will serve his purpose better, which are represented by figure the 5th and last. In these the edges of the pileus of one fungus adheres only to the upper surface of the pileus of another, and receives thence all its nourishment. Now, though it may be difficult to account for this strange position, yet Mr. H. thinks the consequence this author draws from it does not follow, viz. that fungi differ from other plants as to propagation, and in some other respects. As to propagation, the same answer will serve that was made use of above; and what further particulars, in this instance, deserve notice, will be considered, when Mr. H. comes to his observations on the corollaries he draws from all the fungi represented in his book.

The corollaries are as follow:

Corol. 1. That the mode of rise, evolution, increment, and propagation, of fungi, must be of a peculiar kind, and totally different from that which prevails in other kinds of plants. Corol. 2. That what from the analogy of the other plants is called the seed, of the fungi cannot properly be called seed. Corol. 3. That there must be such a similarity in all the parts of the fungi, that it is indifferent whether any part be placed above or below, whether it communicates or receives nourishment. Lastly, That every fungus, according to his hypothesis, is contained in an entire and perfect state from the beginning in every egg, or as it is called, its seed, and wants nothing but evolution to imbibe the necessary juices.

These are the corollaries the author draws; and for a further confirmation of his doctrine, refers to a treatise expressly written on this subject, in which he says, he has made use of such strong arguments, that it would be quite needless to add any thing more in this place. On this Mr. H. remarks, that had he had an opportunity of perusing this treatise, it is possible he might find reason to agree with him, but his arguments must be quite of another nature than what appears here, to make him think these plants exempt from the common laws of vegetables. For as to the supposed difference in regard to rise, evolution, increment, and propagation, which is his first corollary, it has been considered already in part, and will be more fully considered, when he comes to his 3d corollary.

As to the 2d, viz. that what from analogy is called the seed of the fungi cannot properly be called seed. Mr. H. remarks that he cannot see the least foundation for any distinction between the seeds and eggs of plants, the latter of which terms, he thinks, ought to be used when we speak of the fungi; for those terms are perfectly analogous, by the confession of all the nicest observers of nature; and what is called the eggs in animals goes under the name of seed in vegetables. Thus Linneus says, Philosophia Botanica, p. 88, every living thing comes from an egg; consequently all vegetables, whose seeds appear to be

eggs from their final cause, which is to produce an offspring. He then cites a passage from our Harvey to the same purpose. The author seems to have been led into this confusion of ideas by the improper use of the word semen, or seed, which is applied to the impregnating juice in animals, and to the parts which contain the embryo in vegetables, which are by no means analogous; for the impregnating dust in vegetables, answers to the impregnating juice in animals, as it has a similar use; and the eggs in animals answer to what we call seeds in plants.

When the author asserts, in the 3d corollary, that there must be such a similarity in all the parts of the fungi, that it is indifferent whether any part be placed above or below, whether it communicates or receives nourishment, Mr. H. supposes he draws this corollary from the 5th and 6th fungus; for in those the upper fungus adheres only to the edge of the cap, or pileus, of the upper surface of the cap of the lower fungus. The answer to this case, which was before omitted, shall now be made; nor is it at all difficult, for it appears by Dr. Hales's Vegetable Statics, experiment the 41st, fig. 24, that a tree inarched between 2 other trees, though its root be cut off, or dug out of the ground, will continue to grow; and that many trees will grow in an inverted state. In what respect then do fungi differ from all other plants as to the similarity in all the parts, &c.?

Lastly, when the author asserts, that every fungus is contained in an entire and perfect state from the beginning in the egg, or as it is called, the seed, and wants nothing but evolution, in order to imbibe the necessary juices; when the author asserts this, he asserts nothing but what will be readily granted by every one, who has read the observations made by modern philosophers, on this part of nature; and the only difference between him and others is, that he confines himself to one order of plants, what they imagine, from good reason, to be the case of all; and this seems likewise to be the case throughout the animal creation, with this difference only, that in some animals, some parts fall off entirely, after a certain time, and a new form ensues; yet even here, all the forms preceding the last may, and perhaps ought, as has been observed by Linneus, to be looked on as embryo states.

On the whole, Mr. H. thinks the author seems inclined to invent a new hypothesis, from a few insufficient data, rather than to be forced into it by any leading phenomena, which ought to be very strong and convincing, to make us give up an analogy, that is confined within such moderate bounds. However, this ought not to prejudice us in regard to the latter part of his work; on the contrary, it ought to give us a favourable opinion of him, as it shows his zeal and application to this part of natural history. A man may be an excellent and useful observer, and yet be a very indifferent natural philosopher.

Mr. H. comes now to the 2d part, which contains the proposals, and with the greater pleasure, as, at the same time that it affords no opportunity for criticism, it gives us hopes of seeing a very useful work, on one of the most obscure parts of botany.

The author observes, that the Electoral Academy at Munich, from its first institution, determined to turn their thoughts particularly to this part of natural history, which before had been but indifferently cultivated; and in consequence of this determination, the present method proposed to them was approved of. He adds, that the year before last, viz. 1760, though the drought was very great, they not only had an accession of above 100 fungi, but that plates of them were entirely finished, both as to engraving and colouring, and fit to be published. The circumstances and method of proceeding in this work are as follow, and will, as the author assures the public, be strictly and religiously observed.

1. They will begin by publishing the different species of fungi, engraved and coloured according to the specimen in these proposals, with which the public will have reason to be satisfied. While these are publishing, they will be looking out after other fungi that may grow in Bavaria. 2. These plates, when completed, will make a separate volume, intitled, Jacobi Christiani Schaeffer Fungorum Bavariæ, potissimum qui circa Ratisbonam nascuntur, Icones, auspiciis et impensis Academiæ Electoralis Bavariæ Monacensis accurate delineatæ, et publici juris factæ. This volume may be useful without the other volumes, of which hereafter. 3. There will be no plates of the fungi, but such as have been examined from the first growth, and if possible from the egg, or seed, as it is called. 4. There will be at least the following primary figures of every kind of fungi, viz. 1. The fungus before it is unfolded. 2. When it is half perfect, or half grown. 3. Its characteristic appearance. 4. Its dissection. Lastly, whenever it may be necessary, in its decaying state. 5. Besides these primary figures, there will be added as many secondary ones, as there are changes which may render a fungus dubious; and the author foresees, that sometimes one species may require 2, 3, or 4 plates. 6. The plates will be all engraved by able artists, and well coloured. 7. All the parts serving for propagation will be represented, both according to their natural appearances and also magnified. 8. Whenever it may be necessary, figures representing particular parts dissected will be made use of, both of the natural size, and as seen through the glass. 9. In all the names and division of the genera, Linneus alone will be followed. 10. To give a greater variety to the work, in its progress, sometimes the species of one genus will be published, sometimes of another; but yet in such a manner, that order will not be totally neglect_d. 11. No order will be observed either in the species or in the individuals. No specific names, or synonyms, will be given; instead of which, they

will be numbered according to each genus, and a short explanation be put at the bottom of each plate, as in those annexed to this treatise. 12. To the end that beginners in botany, the illiterate, and even the country people, may be able to know the genera of the fungi, as far as is necessary for them, the author hopes that before next Easter, or certainly not long after, copper-plates, engraved and coloured, will be published, representing all the genera in such a manner that they may be easily known. These plates, along with descriptions, will make another separate volume, which will be intitled, Isagoge in Fungorum Bavariæ Historiam, &c. 13. When all the fungi of Bavaria are finished, it is proposed to go on with such foreign ones as can be procured. 14. As soon as the collection of the fungi of Bavaria shall be completed, a new and last volume will follow, containing accurate descriptions, explications, synonyms of authors, and whatever else may be thought necessary. This volume will be intitled, Fungorum Bavariæ Historia.

Thus far the author, in regard to his proposals, which, as far as can be judged by the plan and specimen, seem to deserve the encouragement of the curious. Every one who is conversant in botany, knows how obscure and imperfect this part of it still remains, after all that has been done on the subject. Micheli and Dillenius were the first botanists that examined this order of plants with any degree of accuracy; and though their observations are very considerable, and have been of great service to succeeding botanists, yet Linneus, who mentions them with applause, says, Philosophia Botanica, p. 241, that the order of fungi, to the reproach of botany, still remains a chaos, as we are ignorant what is a species and what a variety. Since the forementioned authors, Gleditschius, who wrote a tract on the same subject, which is commended, expresses himself also to the same purpose: for, speaking of Dillenius, in the preface, p. 5, he says, that his genera are not only not well determined, but are even construeted in such a manner as to contradict the natural characters; and as to Micheli, that though he first determined the seeds of the fungi, yet his genera are too artificial; and therefore he himself follows the method of Linneus. Gleditsehius seems to have taken great pains about this subject; but candidly owns that, after all he had been able to do, in order to distinguish species from varieties, by collecting all the fungi he could find, disposing and describing them according to the degree of similitude, yet he found himself often at a loss, and unable absolutely to determine which side to take.

Gleditschius's book was published in the year 1753, and is, Mr. H. believes, the latest writer who has done much on this subject, except perhaps Scopoli, in his Flora Carneolica, published anno 1760, who differs from all the preeeding botanists in regard to species and varieties; and though his work has great merit,

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both in respect to this and other orders of plants, especially the cryptogamiæ, yet there still remains much to be done, as he believes every skilful reader will easily allow.

From all that has been observed, he would draw a few conclusions in favour of the proposals in question. First, then, the uncertainty of authors on this branch of botany shows, that it requires a still further examination than has hitherto been made.* 2dly, That though in general, since the writings of Linneus, figures have been in a great measure laid aside, yet in this, and some other obscure parts of botany, they may be employed to good purpose. 3dly, That the curious in botany ought to consider themselves as particularly obliged to those who are inclined to labour and make researches in the obscure, and, according to the vulgar opinion, contemptible parts of nature, especially this order of plants, some of which are used for the table, and not a few of them are of a poisonous nature, so that a mistake may, and has perhaps sometimes proved of fatal consequence.

LXXX. Of a Remarkable Agitation of the Sea, July 28, 1761; *and of two Thunder Storms in Cornwall. By the Rev. Wm. Borlase, M. A., F. R. S.* p. 507.

On Tuesday, July 28, 1761, the day quite calm, the sky lowering and cloudy, thunder at times all the day, the tide in Mount's bay was considerably agitated. Between Penzance and Marazion, there is a level of sands, on which there is good travelling when the tide is out: but when the tide is full, the sands are covered. At 10 A. M. the driver of a plough, belonging to William Tregennin, laden with tin, for Penzance coinage, driving as usual on the then bare sands, found himself and the plough on a sudden surrounded by the sea. The horses were frightened and plunged, the oxen stood still, the driver and his boy could neither recollect how they should help the cattle or secure themselves: several people saw them at a distance, but dared not to approach; and in a few minutes when all was given up for lost, the sea retired and left them, safely to pursue their journey. Mr. B. came to Chandour, a small village at the western extremity of these sands, about 11, and found several persons standing on the shore, intent on the several extraordinary fluxes and refluxes of the tide at that time, and was informed, that at the first agitation, when the plough was surprised by the sea, the water must have risen about 6 feet perpendicular. During his stay he observed the sea flowing and retreating several times, and by his watch it was 7 minutes flowing, the water rising about a foot and half, or somewhat more,

* Since this was written by Mr. Hudson, much new light has been thrown on the structure and economy of the fungi by the celebrated Hedwig of Germany; and their different species have been accurately described and figured by Mons. Bulliard in France, and by Mr. Bolton in England.

and the like time nearly in retiring. About half past 11 he was obliged to move homewards, and as he passed by the brim of the water, observed that the sea advanced and retired, and was not settled; but the alterations were then small, and scarcely perceptible. In the more western parts of this bay, the agitations were very apparent; and, by the papers, the like agitations were felt in the harbours of Falmouth, Fawy, and Plymouth.

On the same day, about 8 o'clock P. M. the wind at east, Fahrenheit's thermometer at 64, the atmosphere continuing in the same calm, sultry, and grumbling temperature, the fiercest lightning, accompanied in the same moment with a thunderclap, broke over Ludgvan church; it came from the northwest, and fell on the southern pinnacle of the east side of the church tower. The lightning threw down great part of the pinnacle and stones of the tower, then descended by the belfry and steeple, doing much damage in its course, and passed through the body of the church, damaging the pews, &c. but particularly the pulpit and altar.

More furious still was the thunder storm on the 11th of January last, which fell on the church and tower of Breâg, about 7 miles east of this place. About a quarter past 4 P. M. the barometer as low as 28, the wind blowing hard at south west, on a sudden it grew very dark, and a shower of hail, not remarkably large, followed, accompanied with the fiercest flash of lightning and the most violent explosion of thunder; the lightning and thunder being almost instantaneous. The havock made on the church is past description or conception. The western side of the tower was rent from almost the top to the bottom, the crack not in a straight line but irregular, and from 1 to 5 inches wide; the south east pinnacle split into a thousand pieces, and scattered all over the spacious church-yard and church town; two of the battlements on the western, and four on the eastern and southern sides of the tower struck off, and every one of the windows of the church, excepting one in the jet out north aisle, shattered to pieces, presented a most dismal prospect.

It is difficult to say in what direction the force proceeded, it is apprehended it must have penetrated the tower, through the middle of the arch over the belfry door, which though locked and strongly bolted, was burst open; the centre of the arch was divided, and the top stone of that remarkably fine one over the window cracked athwart; the lightning must therefore have passed directly up the tower, through the midst of the wall, the outside of which has the exact appearance of being battered by cannon-ball, and is quite bulged out between the first and second ring. Had not this been the case, how could such a large quantity of entire stones, and fragments of others of a prodigious size, be forced out of their places, as well on the inside as the outside of the wall?

The stones of the pinnacles and battlements were scattered in all directions;

one, of at least 150lb. weight, fell on the top of a house, about 60 yards to the south, another was cast full 400 yards to the north, one very large one to the south-east of the church; a long stone, which served for a bench, adjoining to the south stile, was cracked cross ways, and one end turned quite upside down. The lightning in passing through the church did every where very great damage. It is remarkable that about the middle of the south aisle, over one of the arches, a round hole, of about 2 inches diameter, was pierced through the carved oak, directly under the plaster, and a piece of the main soil, of more than a foot in length struck off, and part of it burnt to a charcoal. The eastern part of the tower is likewise somewhat damaged, and a small crack appearing on the inside of the wall. Two of the standing pinnacles are much damaged, and part of the cross of the north-western one is struck off; the corners of the tower are very firm, so are the buttresses, excepting the southernmost one of the west end, some stones of which are moved out of their places. Thus the beauty of this admired tower is quite destroyed, never more to be retrieved, as the top of it, as far down as the leads, must be entirely taken off, and the western side is condemned from top to bottom.

LXXXI. On two Remarkable Cases in Surgery. By John Huxham, M.D., F.R.S. p. 515.

June 12th, 1747, Mr. T. Adams, surgeon at Liskard in Cornwall, was sent for to assist John S——r, of the parish of St. Clear. The messenger informed him he had cut his throat from ear to ear. When he came to him he found a very large wound, near 7 inches long, 3 parts round his neck; the trachea cut almost through; but the knife had luckily escaped wounding the jugular arteries. No considerable hæmorrhage ensued, and that was entirely stopped. Mr. A. endeavoured a reunion of the parts by suture; which he performed in the following manner. He first made 2 stitches through the external parts and wind-pipe, which he conveniently performed, as the wound admitted of introducing his fore finger and thumb into the trachea, and left them untied, till he had brought the 2 ends of the wound into contact by suture; then tying the 2 stitches, it had a fair aspect for reunion; which by superficial dressing and bandage was completed in a month's time. As soon as he had dressed him he was able to speak, and informed him, as well as his neighbours, that his wife had made that desperate attempt on his life in a wood, coming from her father's house to the place where she was a servant, by first blindfolding him with handkerchiefs, and then, under pretence of taking measure to make a new shirt for him, took off his stock, unbuttoned his collar, cut his throat, and then ran from him. After he had been about a fortnight employed about his business, as a carpenter, he complained of a troublesome tickling cough, and loss of appetite. His complaints grew worse,

and he was fearful of an ulcer being formed internally, as he had every appearance of a consumption. But coming one day to him, he complained of a soreness externally. On examining, he found a little matter formed, and on opening it, extracted a little silk, about the length of a small pin, which relieved his complaints entirely. He lived 2 years in perfect health, and died of the small-pox.

June 28th, 1756, Mr. A. was desired by the parish of Duloe, to attend Charles R——s, who, 2 days before had been struck by lightning. On his examination, he found it had pierced through his coat, waistcoat, and shirt, a little above the middle of the deltoid muscle of the right arm. It had burnt to tinder almost all the sleeve of the shirt, waistcoat, and inside of the coat sleeve, but the outside appeared untouched, except where the lightning pierced. The flesh of his arm, from the shoulder to the elbow, was burnt, especially where the lightning pierced, a full inch deep, and onwards to the wrist and fingers less and less deep, till it did but just destroy the scarf skin; it pierced again near the umbilical region in a different direction, but not so deep: his thighs were burnt in various directions, but not so deep; from the right knee downwards on the outside, it first burnt the hair, then the scarf skin, and continued on deeper, especially about the ancle and instep of the foot. The left leg much in the same manner on the inside, but not so deeply burnt. His waistcoat, breeches, and stockings burnt on the inside as his coat sleeve, and the outside appeared untouched: his buckles melted in his shoes in various directions. In this deplorable condition, his arm and the other parts appearing greatly inflamed, Mr. A. bled, and gave him a purging draught to empty his bowels, and the next day put him on the use of the bark: the applications were a warm spirituous bath, and the common digesters. By these means there was a separation begun; in 2 days the edges of the burnt parts beginning to separate, when he thought to assist nature by deep scarification; but to his very great surprize, he could no more thrust his knife through the burnt parts than through hide leather, or a thong; by which means the separation was rather slow, and the stench intolerable. By the end of July he was able to walk abroad; and about the middle of August perfectly healed.

The lightning came through the upper part of the window; a pair of sheep-shears lay in the window, behind his back, which Mr. A. imagines collected, and threw it in such various directions about his body. Another man sat by him, slightly struck about his neck and left shoulder. It is remarkable, while the man of the house went to his cellar to draw a jug of cyder, on his return he found his wife and children along the floor, and the 2 men fallen forward, with their faces on the table, all insensible. The man so much hurt recovered his senses first.

LXXXII. On the Success of Mons. Daviel's Method of Extracting Cataracts. By Andrew Cantwell, M. D. p. 519.

The extraction of the crystalline from the posterior chamber, by an incision made in the cornea, with a design to cure the cataract, seems to have been first attempted by Mr. Daviel. It is true, surgeon Petit, and the oculist St. Ives, extracted it out of the anterior chamber in 1708, and the following years; but that operation was designed only to rid this chamber of an accidental burden fallen into it, in couching the cataract; and it is very reasonable to believe it was only the examples of these 2 operators that led Daviel into this new method, which has wonderfully facilitated the cure of that disorder, and cleared up the many difficulties that appeared in it.

The Greeks and Arabians considered the glaucoma as an incurable cataract; and the moderns pretended that the incurability proceeded from the nature of some other distemper complicated with the cataract. It was hard to tell why, the cataract once couched, the patient should remain blind, or why it should rise again into its place.

This new operation shows that not only the crystalline, but even sometimes its capsula, and sometimes only the anterior membrane of this bag, are opaque, sometimes adherent to, sometimes separated from, the body of the crystalline. Sometimes the anterior membrane of this bag has been found opaque, and the crystalline transparent, and in all these cases the patients have recovered their sight.

With this account Dr. C. sent a small box, in which were 3 packets. N° 1 contained a portion of the anterior membrane of the capsula crystallini. This humour, being still transparent, was left untouched. The patient saw perfectly well after the operation. N° 2 contained an opaque crystalline, and a portion of the anterior side of its bag, quite opaque. The patient recovered his sight. N° 3 contained the whole bag and crystalline, extracted the 14th of this month. It adhered to the posterior and superior side of the iris; and was quite whole and plump when drawn out. In this last case it commonly happens that some portion of the vitreous humour follows; sometimes it mixes with the aqueous, and comes off with it; sometimes the eye appears quite sunk; and sometimes the vitreous humour filling as it were the posterior chamber, makes the iris bulge forwards, and appear prominent on it, the whole together resembles a kind of hernia. These two last cases require a nice and prudent hand: the prominent vitreous humour is to be cut off in 2 or 3 days after the operation, the eye then banded, but not compressed, and the patient laid in his bed, the head lower than usual, till the rest of the vitreous humour gets back into its cells, and remains there. This he had seen examples of; the vitreous humour regenerates, and though a

great portion of it be lost, it is supplied again, sooner or later, and the patient recovers his sight.

Dr. C. saw Daviel extract the crystalline quite opaque, with its whole bag adherent to it; and this bag he dissected from it after the operation was over. In the operation of the 14th instant, the crystalline and its whole bag made one entire spheroid, soft and plump; but it is already broken and dry.

Blue eyes are the most subject to the cataract, and black ones to the amaurosis. In these the ciliary nerves and fibres are always weak; in those strong and elastic. 1. A crystalline couched, if the capsula be opaque, leaves the patient blind. 2. A crystalline couched in appearance, if it adheres in any point to its capsula, must rise again. 3. If the posterior side of the bag be opaque, and remains, the patient must remain blind, whether the crystalline be couched or extracted. 4. If the posterior side of the bag is adherent to the crystalline, it must be extracted; and then there is great danger of the vitreous humour coming off. 5. The mistakes of Sennertus, Riverius, Heister, Antoine, Maitre Jean, Brisseau, and St. Ives, &c. about the glaucoma, are easily accounted for in this new theory, founded on facts and daily experience.

LXXXIII. On the Case of Mortification of Limbs in a Family at Wattisham, Suffolk. By Charlton Wollaston, M. D., F. R. S. p. 523.

John Downing a labouring man at Wattisham, in Jan. 1762, had a wife and 6 children; the eldest, a girl about 15 years of age, the youngest about 4 months. They were also at that time very healthy, as the man himself and his neighbours assured Dr. W.

On Sunday the 10th of January, the eldest girl complained in the morning of a pain in her left leg; particularly in the calf of the leg. Towards evening the pain grew exceedingly violent. The same evening another girl, about 10 years old, complained of the same violent pain in the leg. On the Monday the mother and another child, and on the Tuesday all the rest of the family, except the father, were affected in the same manner. The pain was exceedingly violent; insomuch that the whole neighbourhood was alarmed with the loudness of their shrieks. The left leg of most of them was only affected; but in some both legs. The little child was taken from its mother's breast as soon as she was taken ill, and lived a few weeks. The nurse told Dr. W. it seemed to be in violent pain, and that its legs were black before death.

Dr. W. was exact in his inquiries about each particular person. By what he could learn from them, in about 4, 5, or 6 days, the diseased leg began to grow less painful, and to turn black gradually; appearing at first covered with spots, as if it had been bruised. The other leg began to be affected at that time,

with the same excruciating pain, and in a few days that also began to mortify. In a very little time both legs were perfectly sphacelated. The mortified parts separated without assistance from the sound parts, and the surgeon had, in most of the cases, no other trouble than to cut through the bone, with little or no pain to the patient. The separation was in most of them about 2 inches below the knee; in some rather lower; and to one child the feet separated at the ancle, without any assistance from the surgeon. In some the separation was not quite so perfect. The eldest girl had one leg taken off, and the other was perfectly sphacelated; but the surgeon had not thought proper to cut it off yet, as the thigh was much swelled, and there was a large abscess under the ham. The mother had the right foot off at the ancle; the other leg was a mere bone, quite black, and exceedingly fetid, with some little remains of putrid, almost dry flesh, in some parts. One child only had one leg saved, with the loss of 2 toes of that leg. Three of the children had lost both legs, and the other child both feet. This was the state of their legs at that time; viz.

Mary the mother, ætat. 40. The right foot off at the ancle: left leg mortified, a mere bone; but not off. Mary, ætat. 15. One leg off below the knee: the other perfectly sphacelated; but not then off. Elizabeth, ætat. 13. Both legs off below the knees. Sarah, ætat. 10. One foot off at the ancle. Robert, ætat. 1. Both legs off below the knees. Edward, ætat. 4. Both feet off at the ancles. An infant, 4 months old, dead.

The father was attacked about a fortnight after the rest of the family, and in a slighter degree, the pain being confined to 2 fingers of his right hand, which turned blackish, and were withered for some time; but were then better, and he had in some degree recovered the use of them.

It is remarkable, that during all the time of this calamity, the whole family are said to have appeared in other respects well. They ate heartily, and slept well, when the pain began to abate. When Dr. W. saw them they all seemed free from fever, except the girl, who had an abscess in her thigh. The mother looked emaciated, and had very little use of her hands. The rest of the family seemed well. One poor boy in particular looked as healthy and florid as possible, and was sitting on the bed quite jolly; drumming with his stumps.

Dr. W. made what inquiry he could into the manner of their life and food, before this misfortune befel them: but he could not discover any thing to which he could attribute this very surprizing attack. They lived, as the country people do, on dried peas, pickled pork, bread and cheese, milk and small beer. The man was a day labourer, and the woman and children spun, and by their industry and sobriety maintained themselves very well. There was no reason to apprehend that these poor people had suffered by being exposed to severe cold, as the beginning of January was remarkably mild. It is not very uncommon for

one limb to be lost by a sphacelus, attended with the same symptoms as in these cases; but it is very extraordinary that a disorder of this kind should run through a whole family with such amazing violence and rapidity. A nurse, who had lived with them from the beginning of their illness, had not been affected. She did not live in the house with them before; but used to be with them frequently.

LXXXIV. Extract of a Letter from the Rev. James Bones, M.A., Minister of Wattisham, near Stowmarket in Suffolk, relating to the Case of Mortification of Limbs in a Family there. p. 526.

This is no more than a repetition of the case in the last article.

LXXXV. Extract of a Second Letter from the Rev. Mr. Bones to Dr. Baker. p. 529.

I have taken all the pains I can to inform myself of every circumstance which may be deemed a probable cause of the disease by which the poor family in my parish has been afflicted. But I fear I have discovered nothing that will be satisfactory to you. The following is an answer to your queries.

Water.—This they have taken out of a ditch, or pool of standing water, at their own door, as is common in this clay country. We have no spring or well in the parish.

Beer.—They have generally bought their beer at a public house. But in August last the poor man brewed 2 bushels of malt, in a large brass kettle, which is very commonly let out to the poor. It is an old one, but belongs to a cleanly housewife.

Bread.—We have no rye. This family have been used to buy two bushels of clog-wheat, or rivets, or bearded wheat (as it is variously called in this country), every fortnight. Of this they have made their household bread. This wheat they have bought of the farmer, whom I lodge with, who tells me that last year he had some wheat laid, which he gathered, and threshed separately, lest it should spoil his samples. Not that it was mildewed, or grown, but only discoloured, and smaller than the other. This damaged wheat he threshed last Christmas; and then this poor family used no bread but what was made of it, as likewise did the farmer's own family, and some others in the neighbourhood. We observed that it made bad bread, and worse puddings; but I do not find that it disagreed with any body. A labouring man of the parish, who had used this bread, was affected with a numbness in both his hands, for about 4 weeks from the 9th of January. His hands were continually cold, and his fingers ends peeled. One thumb he says still remains without any sensation,

Kitchen utensils.—They have 2 small iron pots, which have long been in use. In these they boiled their pork, peas, &c. They have likewise 2 brass skillets,

rather old, in which they boiled milk, &c. The man tells me they are in constant use, and never were cankered.

Pease.—They have now and then eaten pease and pease broth. These they have always bought, as others do, at the shop: and they have never disagreed with any of the family, except only on Sunday, January 10. Three of the children were then sick after eating them; but became easy after they had vomited.

Pork.—This they generally bought pickled of the farmer whom I lodge with. The farmer's family, and several others, have constantly eaten it. In this part of the country there is a great deal of old ewe-mutton killed, between the first of November and January, some of which is very poor and rotten, and is usually sold at 3 halfpence, or perhaps one penny a pound. In December last this family lived for 3 weeks at least on this mutton, of which they bought a quarter at a time, weighing 7 or 8 lb., for one shilling. The man is so prepossessed with notions of witchcraft, and is so obstinate in his opinion, that I cannot excite in him even a desire of attributing this disease to any other cause.

Since my last letter to you, Mary, aged 16, who sat for 14 weeks in a great chair, and for 7 days without any feet, or flesh on her leg-bones, has consented to have the bones taken off. She is now in bed; the abscess is healing, and she seems likely to do well. The father's fingers are almost healed: but he every day feels severe darting pains in many parts of his body. The mother lies in bed with her leg-bones bare, which she will not suffer to be taken off. Her hands are still benumbed, but not black. Her fingers are contracted. The rest of the family seem to be recovering perfect health."—In addition, Dr. B. says,

There is in L'Hist. de l'Acad. Royale des Sciences, for the year 1710, a paper, the title of which is, Sur le bled* cornu appellé Ergot. Here it is said, that M. Noël, surgeon of the Hotel-Dieu at Orleans, had sent an account to a member of the academy, that within about a year's time, he had received into the hospital more than 50 patients affected d'une gangrene seche, noire et livide, which began at the toes, and advanced more or less, being sometimes continued even to the thighs; and that he had only seen one patient who had been first seized with it in the hand. He adds, that he observed that this disease affected the men only; and that in general the females, except some very young girls, were quite free from it. In the same paper is mentioned, as a fact well known to the academy, the case of a peasant who lived near Blois. In this patient a gangrene, at its first attack, destroyed all the toes of one foot, then those of the other, afterwards the remaining parts of both feet; then the flesh of both his legs, and that of his thighs, rotted off successively, and left nothing but bare bones.

* Secale corniculatum nigrum, mentioned as a poison by Hoffman.—Orig.

The gentlemen of the academy were of opinion that the disease, of which M. Noel had sent an account, was produced by bad nourishment, particularly by bread, in which there was a great quantity of ergot.* This substance is described by M. Fagon, first physician to the king, and is said by him to be a kind of monster in vegetation, which a particular sort of rye, sown in March, is more apt to produce, than what is sown in the autumn, and which often abounds in moist cold countries, and in wet seasons. How far it is true, that this substance was really the cause of the French epidemical gangrene described, I cannot determine. On comparison, we find that the present disease at Wattisham, and that recorded by the French academy, do agree extremely in their effects. However, it is now certain that rye made no part of the nourishment of the poor family at Wattisham. Though we undoubtedly excel the ancients in the knowledge of poisons, yet a great deal of that subject still remains unknown to us. It will therefore be very difficult for us to discover to what cause, or to what combination of causes, so uncommon a malady is to be attributed.

LXXXVI. Observations for Proving the Going of Mr. Ellicott's Clock at St. Helena. By Mr. Charles Mason. p. 534.

In Mr. Mason's return from the Cape of Good Hope, the clock, used in the observations made there, was set a-going at St. James's fort, St. Helena, the pendulum remaining as at the Cape. Here he was at a great loss to get observations to prove its motion, the heavens being almost perpetually covered with clouds. At length, considering that the place being situated in such a narrow deep valley, if the times of the descent of the stars, over the western ridge of rocks, (the altitude of whose nearest summit was about 30°, and distant about a quarter of a mile at the observatory) were observed, it would give the time per clock, in a sidereal day; and the chances for such observations would be greater than by any other method, as they might be continued the whole night. Accordingly he began to observe, by fixing the eye to a point: but this was soon improved by the Rev. Mr. Maskelyne, by making the stars descend each night, in the same part of the telescope of the equal altitude instrument: and it was very beautiful to see how instantaneously they disappeared.

The difference of the effect of gravity at the two places, on the going of the clock, may be seen by comparing these with the observations made at the Cape. By these it appears that the clock at St. Helena lost from 58″ to 59 per day of sidereal time.

* This degenerated rye is called ergot, from its resemblance to a cock's spur.—Orig.

LXXXVII. An Account of Mr. Mason's Paper concerning the Going of Mr. Ellicott's Clock, at St. Helena. By James Short, M.A., F.R.S. p. 540.

In this paper Mr. Mason tells us that, in order to determine the regularity of the motion of Mr. Ellicott's clock, he resolved to make observations of the occultations of stars, by a ridge of rocks, the altitude of which was about 30° above the place of observation, and at about a quarter of a mile distance; but that this method was soon improved by the Rev. Mr. Maskelyne, who proposed to make use of the equal altitude instrument for that purpose, by observing the vanishing of the stars out of the field of the telescope. By observations of this sort, from the 31st of October to the 19th of November 1761, he found that the clock went very regularly, not varying so much as a second in that time; but from the 19th of November to the 3d of December, he found that the clock had gone slow, or lost 2 seconds of time; and this alteration he imputes to the wedges, behind the clock, having got loose, shrunk as he supposes by the dryness of the place; he therefore secured the wedges, and found that from the 3d of December to the 22d of December, the clock did not vary in its motion above one second of time. On the 5th of January the clock was stopped; and it appears that the clock did not vary so much as one second of time from the 9th of January to the 22d of January. The thermometer was hung by the side of the clock, and he never saw it higher than 74½ divisions, nor lower than 67, from the 12th of Jan. 1761 to the 18th of January 1762. He has given no description of the clock.

Remark. The method proposed by Mr. Mason, of making these observations by means of the occultations of stars behind the ridge of rocks, was certainly better than the other, by means of the equal altitude instrument; for it has been found by experience, that any instrument, however securely fixed, is liable to alterations in its direction, owing perhaps to the effects of heat and cold, moisture and dryness, in the parts to which the instrument is fastened; and an equal altitude instrument was the most improper for this purpose, because it could not be rectified by looking at a distant mark, to correct any alterations it might have suffered in its position or direction. Mr. Mason further says, that by comparing the observations of the going of the clock, made at St. Helena, with those made at the Cape of Good Hope, the difference of the effect of gravity at the two places may be found.

Remark. No observations of the difference in the going of a clock, made at different places, can with certainty determine the difference of the effect of gravity at these places; because it has been found by experience that the same clock, placed at different times on different walls, in the same room, will make a difference in the going of the clock, even though every part of the clock remains the same.

LXXXVIII. The Eclipse of the Moon, May 8, 1762, in the Morning, observed by Mr. Short, in Surry-street, London. p. 542.

Diameter of the moon, (in the direction of an angle of 45° with the horizon, the lower end of the diameter being to the west of the moon's centre, measured with an achromic object-glass micrometer of 40 feet focus, and found to be =

31′ 31″.7,—On the 7th May, at	11^h	35^m	0^s
α Libræ passed the meridian, at	11	38	25
Preceding limb of the moon on the meridian, at	11	49	58
Subsequent ditto, at	11	52	13½
Penumbra, sensible at	13	40	0
Diameter of the moon, 31′ 26″, measured again, at	13	50	0
Beginning of the eclipse, through an achromic opera-glass, at	14	12	30
Shadow very dense on the moon's limb, at	14	14	0
So that I conclude the eclipse began at	14	13	0

The times above are apparent times.

LXXXIX. Observations on the same Eclipse. By Dr. Bevis. p. 543.

Apparent time.

14^h 8^m 0^s A discernible penumbra on the moon's limb.

14 20 The beginning of the eclipse, by the bare eye; but doubtful to about a minute, from the dilute and uncertain termination of the true shadow.

XC. Of a Remarkable Monument found near Ashford in Derbyshire. By Mr. Evatt, of Ashford. p. 544.

In the year 1759, as some people were making a turnpike road through the village of Wardlow, near this place, they thought proper to take out of an adjoining field a heap of stones, that had lain there time immemorial, and without any tradition why it was thrown together in that place, though it was manifestly a work of art. Here, to their great surprize, on removing the stones, they found a monument, to the memory of 17 persons, or more, who had been there interred. The bodies appeared to have been laid on the surface of the ground, on long flat stones, and their heads and breasts protected from the incumbent weight of stone, by small walls made round them, with a flat stone over the top; excepting 2 principal ones, which were walled up, and covered from head to foot, in the form of a long chest, with a stone cover over each. On removing the rubbish, many bones, such as jaw-bones, teeth, and the like, were found undecayed; but none at all of the larger bones of the body. The heap of stones that covered them was 32 yards in diameter, and about 5 feet high; and the stones of which the coffins or tombs were composed, appear very plainly to have

been taken from a stone quarry, above a quarter of a mile distant. They were disposed in a circular form, but a part of the circle is vacant; though it is probable it was not so at first; as there were found several bones and teeth in that space; the cause might be, that as part lay next the road, it might have met with an accidental disturbance; or, what is yet more likely, the people that came to draw the stones away, beginning on that side of it, destroyed that part before they were aware that it was any ways remarkable or worthy note.

There is one circumstance that seems to denote the monument to have been rather modern, which is this: It appears from the best observations he could make, that a wall of the field cutting off a small segment of the circle, marked d, was erected before the monument was made, as it is hardly probable that the persons who built it, would be at the trouble to remove that part of the circle that was without, for the sake of building a field-wall entirely level; which is the case, for all that portion of the circle, from the inside of the wall, was as level as any other part of the field; and as walls are not probably of very ancient date here, (if the above be a fact) he concludes, that the monument must have been erected in some of the wars of the houses of York and Lancaster, or later. The several coffins were about 2 feet high each: the two complete ones about 7.6 long each; and the others had the flat stone nearly the same length; but the covering extended only as far as the breast.

XCI. Description of the Hiero Fountain at the Chemnic Metal Mines in Hungary, erected in the year 1756. *By Wolfe, M.D. From the Latin.* p. 547.

This machine, though it has nothing new, seems not unworthy to be known, both as it is thought the only one of its kind, applied to a large work, and because of the singular generation of snow and ice, observed in its operation.

In pl. 15, fig. 1, N is a wooden cistern at the middle of the mountain, at 143 feet above the horizontal plane, into which the waters from a higher mine are received. O is a similar cistern, at the top of the mountain, 260 feet above the plane, into which the rain waters are led for assisting the work. A is an air vessel, at the foot of the mountain, into which the water from the cistern N or O is emitted, by the tubes RT or GT, and the cock H, by the force of which the air is compressed, and propelled through the tube LMM into a lower vessel. B is a similar vessel in the bottom of the mine, 104 feet below the vessel A, which receives the water from the cistern D collected in the mine, which, being compressed by the force of the air coming from the upper vessel, is raised and discharged through the tube QSF. K is a tube with a cock for letting out the water from the vessel A, after the work is finished, for which purpose also the tube I sometimes serves. L transmits or stops the air. The cock E of the little tube ought to be open,

while the water flows from the cistern D through the tube QCP into the vessel B. At the orifice of this, snow and ice are generated. P and S are small valves, to prevent the return of the air from the vessel B, and the water from the tube FS.

The operation of the machine is performed thus; two men being ready at the vessels, and the cocks being all shut, and sufficient water being let into the tubes RT and GT, from the cistern N or O; first they open the cocks C and E of the lower vessel, and quite fill the lower vessel with water from the cistern D. Then the cocks C and E are shut, and H and L of the upper vessel opened, so that the water, flowing in through HX, may leisurely fill the vessel, and compressing the air, thus force it through LMM into the lower vessel; the water begins then to flow out at F, till the vessel A be hardly half full. The vessel B being thus nearly emptied, H is shut, and the pipes K and I opened, the greater part of the water is let out of the vessel A. Then K and I are shut again, L opened, and the air returns into the vessel A with a loud noise. Lastly L being closed, the operation is finished, till it be proper to repeat it.

When the work is performed with the water from the cistern N, which is saturated with nitrous and sulphureous particles, if in the foregoing operation, the air being returned by opening the cock L, and the tubes C and E opened, a kind of snow is always generated at the orifice of the tube E, and collected like a cap about it. But if, after the said operation, the cock L be not opened, and so the air remain in a state of compression, on opening the tubes C and E, the cap, instead of snow, is formed into a firm and thick cap of ice. But nothing of this kind happens when the operation is performed with the rain water from the cistern G.

The measures of the parts are as follow;—

Of the vessel A, the diameter $49\frac{1}{2}$ inches, height 54 inches, thickness $1\frac{1}{4}$ inch.

Of the vessel B, the diameter $32\frac{1}{2}$ inches, height 60 inches, thickness $1\frac{1}{4}$ inch.

The tubes RT, GT of iron, diameter $4\frac{1}{2}$ inches, thickness $1\frac{1}{4}$ inch.

Also the diameter of FSQ $3\frac{1}{2}$ inches. LMM tapering, has the upper diameter 2 inches, the lower 1 inch, and thickness $1\frac{1}{4}$ inch.

The Chemnic foot is to the Paris, as 1538 to 1440, the pound as 106 to 92. A cubic foot of the mine water weighs 72lb.

The vessel A contains $57\frac{1}{2}$ cubic feet, B $27\frac{1}{2}$ cubic feet, or 22 amphoras.

At each operation there is raised 25 cubic feet, sometimes $31\frac{1}{7}$; differing only in time, as the water flows in from N or from O; and there will be performed 20 or 21 repetitions of the operation in the one case, in the other only 17 or 18.

XCII. *Of a remarkable Marine Production.* By Alexander Russell, M.D., and F. R. S.* p. 554.

Extract from Dr. Nasmyth's Letter, who brought the animal from America.

"At my return from North America, in November 1759, I sent you 2 or 3 articles picked up in that country. One of these, from its singular appearance, and from its being a perfect stranger to every body that saw it, I must now recommend to your attention. The desire of keeping it entire, and as it was found, prevented any other investigation, than that of viewing it particularly, when I first got it, and at times afterwards, to be assured of its safety, as well as to observe the changes it might undergo while it continued in spirits.

In June 1759, the squadron destined against Quebec arrived in the river St. Lawrence, when being in the latitude 49° 50′ north, and about 10 leagues to the westward of Anticosti, (an island in the mouth of the river) we sounded, and struck ground in 42 fathoms; the soundings white sand and black specks. Having at the same time thrown overboard a fishing-line, the hook was found strongly attached at the bottom; and after some efforts, brought up a piece of rock, into the surface of which was inserted a strong tendinous substance of a light brown colour, in length about 7 inches; it was round, and nearly of the thickness of a common goose-quill; the other end formed a sack, or bag, of the size and shape of a pigeon's egg. The whole of this substance was elastic; and on pressing the bag, I plainly discovered a contained substance, and imagined that it was attended with motion."

[Thus far Dr. Nasmyth.]

On examination of it, by Drs. Solander and Russell, and Messrs. Collinson and Ellis, it appeared to come nearest to what has been by naturalists called Priapus; they therefore named it Priapus pedunculo filiformi corpore ovato. The body was oval, and in size between a pigeon and pullet's egg, smooth, membranous, and of a silver ash colour. What appeared to be the mouth, was situated a little below the apex, and was quadrivalvular, in the form of a (+) cross. The anus was on the same side, a little above the base, or insertion of the stalk, and also quadrivalvular. Towards the aperture of the mouth and anus, the body felt more callous. From this body issued a peduncle, or stalk, of 10 inches in length; the extreme end of which was fixed to a piece of rock. This stalk was of a light brown colour, about the thickness of a large hen's quill, round, hollow, rough, and of a membraneous, leather-like substance. When the body was opened, the internal coat appeared to be composed of reticular fibres. The interior orifice of

* This animal, which was referred by Linnæus, in the twelfth edition of the Systema Naturæ to the genus Vorticella, under the name of *Vorticella ovifera,* is a speices of pedunculated Ascidia; and is the *Ascidia pedunculata* of the Gmelinian edition.

the mouth was surrounded by a radiated substance, about the size of a silver penny, thicker and more callous than the coats of any other part. The internal aperture of the anus was composed of fibres interwoven with one another. From the apex to the base, on each side, descended obliquely, and winding, a smooth, solid body, in width about one fifth part of an inch, part of which separated in the examining, so that it is but imperfectly represented in the drawing. We cannot give a clearer idea of this body, than by saying, that it had greatly the appearance (except in size) of one of the small intestines, and was attached to the interior surface of the main body, much in the way as they are to the mesentery.

XCIII. Results of Observations of the Distance of the Moon from the Sun and fixed Stars, made in a voyage from England to the island of St. Helena, in order to determine the Longitude of the Ship, from Time to Time; with the whole Process of Computation used on this Occasion. By the Rev. Nevil Maskelyne, M.A., F.R.S. Dated St. Helena. Sept. 9, 1761. p. 558.

During the course of his voyage from England to St. Helena, Mr. M. made frequent observations of the distance of the moon from the sun and fixed stars, in order to determine the ship's longitude: and, as from their agreement with each other, he conceived it will be allowed, that the longitude may in general be ascertained by this method to sufficient exactness for nautical purposes, he thus communicated to the R. S. the results of his observations. He also delivers the whole process of computation, which he used in deducing the longitude from an observation, wherein he included several useful rules of his own investigation, which he apprehends render the calculation not only much shorter, but also much less intricate than it was before.

The time being determined by an altitude of the sun or a star, and the distance of a proper star from the moon's limb, or the distance of the sun and moon's nearest limbs in the first and last quarter, being carefully observed, the longitude may thence be found without any other observations; and this is the method proposed by the late Dr. Halley, which certainly deserves to be highly esteemed for its great simplicity, and the small number of observations which it requires. Yet Mr. M. owns himself of opinion with the Abbé De la Caille, that it will be more convenient at sea to require the aid of more observations, which is the method Mr. M. constantly practised during his voyage, having always two observers, who were ready, one to take the altitude of the star, and the other of the moon's upper or lower limb, at the instant he spoke when he had made the observation of the distance of the star from the moon.

Mr. M. can therefore answer from his own experience, both that the method

is practicable at sea, and also that so far from being less simple, it is more so than the other method; for the additional observations that it requires are very easily made, and even the error of a degree in the altitudes would seldom be of more consequence than an error of a minute, in taking the distance of the star from the moon; so that an error of 10′ or 15′ in the altitudes would be of no great prejudice: but with respect to the facility of the calculations, there is no comparison between the methods, the latter being much less intricate, and much more concise. The Abbe de la Caille requires the altitude of that part of the moon's limb from which the distance of the star is taken; but as at sea we can only take the altitude of the moon's upper or lower limb, an allowance might be made near enough, by estimation of the eye, for the difference of altitude between the moon's upper or lower limb, and that part of the limb from which the distance of the star is taken, he generally added the semidiameter of the moon to, or subtracted it from the observed altitude of the lower or upper limb, in order to have the apparent altitude of the centre, and he found the apparent distance of the star from the moon's centre, by adding or subtracting the moon's horizontal semidiameter, augmented according to her height, to or from the observed distance of the star from the moon's nearest or remotest limb.

This method will be exact enough, if the altitude of the moon or star be not less than 5°. Having thus got 3 sides of the spherical triangle formed by the moon, the star, and the zenith; namely, the apparent zenith distance of the moon, the apparent zenith distance of the star, and the distance of the star from the moon, he finds the effect of refraction and parallax, in altering the apparent distance of the star from the moon, by the two following rules.

Rule 1. To find the effect of refraction in contracting the apparent distance of two stars, or of the moon and a star.

Add together the logarithm-tangents of half the sum, and half the difference of the two zenith distances, the sum abating 10 from the index is the tangent of arc the first. To the logarithm-tangent just found, add the logarithm-cotangent of half the distance of the two stars, the sum abating 10 from the index is the tangent of arc the 2d. Then add together into one sum the logarithm-tangent of double the first arc, the co-secant of double the 2d arc, and the constant logarithm 2.0569; the sum abating 20 from the index is the logarithm of the number of seconds required; by which the distance of the stars, or of the moon and stars, is contracted by refraction: which therefore added to the observed distance, gives the true distance cleared from refraction.

This rule may be made universal, so as to serve with equal exactness almost down to the horizon, if the apparent zenith distances be diminished by 3 times the refraction belonging to them, found from any common table of refraction, and the computation be made with the zenith distances thus corrected. But if

the altitudes of the moon and star be not less than 10°, this correction will not be necessary. It will not be proper to make the observations, if the altitudes of the star and moon are either of them less than 4° or 5°, on account of the variableness of refraction near the horizon.

Rule 2. To find how much the distance of the moon and a star is increased or diminished, on account of the moon's parallax.

Add together into one sum the logarithm-tangents of half the sum, and half the difference of the zenith distances, and the cotangent of half the distance of the moon and star, all corrected for refraction; the sum, abating 20 from the index, is the tangent of arc the 3d, for which arc the 2d, found by the first rule, may be taken, without any sensible error.

Then if the zenith distance of the moon is greater than that of the star, take the sum of this arc and half the distance of the moon and star; but if the zenith distance of the moon is less than that of the star, take the difference of the said arcs; the tangent of the sum or difference, which may be called the parallactic arc, added to the cosine of the moon's zenith distance, and the logarithm of the moon's horizontal parallax in minutes abating 20 from the index, is the logarithm of the number of minutes required, by which the apparent distance of the moon from the star is always augmented by parallax, unless the zenith distance of the star be greater than that of the moon, and at the same time, arc the 3d be greater than half the distance of the moon and star, in which case the apparent distance of the moon and star is diminished by the parallax.

Therefore the number of minutes found by this rule is always to be subtracted from the observed distance of the moon and star, first corrected for refraction, in order to find the true distance, cleared from the effect of parallax likewise; except in the case specified, when the zenith distance of the star is greater than that of the moon, and arc the 3d is at the same time greater than half the distance of the moon and star, when the correction is to be added. In computing these corrections, 4 places of figures beside the index will be sufficient.

It remains to be found by calculation, at what hour under a known meridian, the distance of the moon from the star will be the same as results from the observation, cleared of refraction and parallax. For this purpose it is necessary to compute the moon's longitude and latitude, and horary motion both in longitude and latitude, from the most exact tables, for the time under the known meridian, which is judged to correspond nearly to the given time of observation under the unknown meridian. The mean motions of the sun and moon he took from very exact tables, which he received as a present from the ingenious Mr. Gael Morris. composed by himself, from the comparison of a great number of Dr. Bradley's observations; to which he applied the lunar equations, as they stand in Mr. Mayer's printed tables. After finding the mean longitude of the star at the pre-

sent time, he always allowed for its aberration in longitude, which will sometimes amount to 20″, without considering the aberration in latitude, which can be of no consequence in a zodiacal star, such as those are which are always to be used in these observations. The distance of the star from the moon he computed from their longitudes and latitudes, by the two following rules:

Rule 1. Add together the logarithmic cosine of the difference of the computed longitudes of the moon and star, and logarithmic cosine of the difference of their latitudes, if they are of the same denomination; or sum if they are of different denominations; the sum abating 10 from the index is the cosine of the approximate distance.—This gives the absolute distance of the moon from the sun, without any further calculation. But in case of a star, it is necessary to apply another rule also. Seven places of logarithms, besides the index, must be used in computing from this rule, and the calculation must be carried to seconds.

Rule 2. To the constant logarithm 3.5363, add the sines of the moon and star's latitudes, the versed-sine of the difference of longitude, and the co-secant of the approximate distance just found; the sum abating 40 from the index is the logarithm of a number of minutes, to be subtracted from the approximate distance, to find the true distance, if the latitudes of the moon and star are of the same denomination; but to be added if they are of contrary denominations. The 2d of these two rules, though only an approximation, is so exact, that if the latitude of the moon was 5°, and that of the star 15°, the error resulting would be only 10″ in the distance. Four places of figures will be sufficient in computing from this rule.

If the distance of the moon from the star thus computed, at the assumed time under a known meridian, suppose Greenwich, agrees with the distance observed, corrected for refraction and parallax, the time at Greenwich was assumed right; and the difference between this time and the time of observation under the unknown meridian, is the difference of longitude in time between the said meridian and Greenwich; which is turned into degrees and minutes of the equator, by allowing 15° for every hour, and 1° for every 4 minutes of time.

But if the distance computed differs from the distance inferred from the observation, it must be found by proportion from the moon's horary motion to or from the star, how long time she will take to run over that difference; whence the time will be found at Greenwich, when the true distance of the moon from the star was the same with that resulting from the observation; which, compared with the time of the observation by the meridian of the ship, gives the difference of longitude from Greenwich as before. If the distance of the moon from the star computed, agrees with that resulting from observation within 10′ or 12′, and the distance of the moon from the star be not less than 20° or 30°, the horary motion of the moon in the ecliptic may be taken for the horary motion of the moon to or from the star; but otherwise the moon's longitude and latitude must

be found at an hour's interval after the time assumed at Greenwich, by adding the horary motions to the longitude and latitude computed; and by the application of the rules, the distance of the star from the moon must be found again at the end of that hour; which gives the horary motion to or from the star as required.

It is to be observed that the longitude thus found is that of the ship, at the instant when the altitude of the sun or star was taken, by which the watch was regulated, and not at the time of the observation of the distance of the star from the moon; for the watch being supposed not to vary considerably during that interval of time, must continue to indicate the time according to the meridian by which it was corrected; and the observation of the distance of the moon from the star showing the time at Greenwich, the difference must show the difference of longitude between that meridian and Greenwich.

Perhaps the following method of deducing the longitude from the observations may be least liable to mistake;—Find what the longitude by account was, at the instant of taking the sun or star's altitude, for the regulation of the watch; which being turned into time, at the rate of one hour for every 15°, and 4 minutes for every degree, add to the correct time from noon, when the distance of the star from the moon was taken, if the ship is to the west of Greenwich, or subtract from it, if it be to the east; this gives the apparent time at Greenwich by account; and the mean time is found, by applying the equation of time; to which time, compute the moon's longitude and latitude from the tables, and the distance of the star from the moon by the rules, and find by proportion as before, what time the moon will take to run over the difference between the distance computed, and that resulting from the observation; this turned into degrees and minutes of the equator, will show the error of the ship's account; and the following rules will show whether the ship is to the east or west of its account.

If the distance of the moon observed east of a star (or the sun in the first quarter) is greater than that computed, the ship is west of the longitude by account; but if the distance observed is less than that computed, it is east of account. If the distance of the moon observed west of a star (or the sun in the last quarter) is greater than computed, the ship is east of account; but if the distance observed is less than computed, it is west of account.

The horary motion of the moon in the ecliptic, may be thus made out very expeditiously from Mayer's equations, by the help of the principal arguments used in the computations of the moon's place. Call A, B, C, and D, the differences of the equations of the centre, evection, and variation, and reduction to the ecliptic, for 1° addition to their arguments; where it must be noted, that they must have the same sign as the equation, if it is increasing; but a contrary sign, if it is decreasing. Compute the value of $32' 56'' + A \times \frac{11}{20} \times \frac{99}{100} + B \times \frac{1}{2} \times \frac{16}{17}$, which put $= H$, and the true horary motion of the moon in her orbit $= H + C$

$\times \frac{H - 2' 28''}{60'}$, which put $= K'$; and the horary motion of the moon in the ecliptic is $K' + \frac{D \times K'}{60'}$.

The horary motion of the moon in latitude, calling the difference answering to 1° increase of the argument of latitude E, is $\frac{E \times K'}{60'}$.

The most difficult part in the above computations, and in which a person is most liable to make mistakes, is the computation of the moon's place; but if this be done at land for every 12 hours at least, and the distance of a proper star, or of 2 stars, one to the east, and the other to the west, from the moon's enlightened limb, be computed for every 6 hours at least, according to Mons. De la Caille's proposál, the rest of the computation, which will remain to be done at sea, will be very plain and concise.

Here follows the series of his determinations of the longitude during his voyage, delivered in an extract of his sea journal. The first column contains the day of the month; the 2d, the latitude; the 3d, the longitude, which he deduced from his observations of the moon, reduced to the nearest noon; the 4th shows the longitude by account, kept in the usual manner; the 5th gives the difference between the 3d and 4th columns, and expresses how much the longitude deduced from the observation of the moon is west of the longitude by account; the last column shows whether the distance of the sun from the moon, or distance of what star from the moon, was observed.

1761. ♂ Jan. 20 | At noon took our departure from the Lizard, which bore full north, distance 21 miles, allowing its longitude from London to be 5° 14′ west, and latitude 49° 57′ north.

	Latitude.	Longitude w. by observation of moon.	Longitude by reckoning.	Longitude by moon w. of account.	The sun or stars whose distance from the moon's enlightened limb was taken.
♂ Feb. 10	16° 49′N.	30° 22′w.	27° 33′w.	2° 49′w.	Sun's E. limb from moon's w. limb.
11	14 3 N.	29 22 w.	26 47	2 35 w.	Sun's E. limb from moon's w. limb.
15	5 10 N.	23 39 w.	22 44	0 55 w.	Cor Leonis from moon's w. limb.
19	1 42 N.	23 35	22 44	0 51 w.	Pollux from moon's E. limb.
28	9 6 S.	29 44	26 2	3 42 w.	Sun's w. limb from moon's E. limb.
March 9	24 9 S.	30 7	25 55	4 12	Aldebaran from moon's w. limb.
10	25 51 S.	29 32	24 32	5 0	Aldebaran from moon's w. limb.
13	29 49	27 55	22 19	5 36	Sun's E. limb from moon's w. limb.
		27 44		5 25	Cor Leonis from moon's w. limb.
15	30 8	26 52	22 8	4 44	Cor Leonis from moon's w. limb.
17	30 39	24 50	18 58	5 52	Pollux from moon's w. limb.
		25 5		6 7	Spica Virginis from moon's w. limb.
18	31 1	24 0	18 47	5 13	Pollux from moon's w. limb.
		24 33		5 46	Spica Virginis from moon's w. limb.
19	31 37	21 41	17 7	4 34	Pollux from moon's w. limb.
20	32 4	20 19	15 27	4 52	Pollux from moon's E. limb.
		22 4		6 37	Antares from moon's E. limb.
25	31 23	12 4	5 57	6 7	Spica Virginis from moon's E. limb.
26	30 51	9 55	4 17	5 38	Spica Virginis from moon's E. limb.
29	30 32 S.	6 49 w.	1 13 w.	5 36	Sun's w. limb from moon's E. limb.

April 6 | 7½ A. M. came to an anchor in the harbour before James's fort at St. Helena; making the latitude per account 15° 55′ S. longitude by common reckoning 1° 28′ E. longitude corrected by observation of the moon 4° 16′ W.

By the comparison of the longitude determined by the moon, with the longitude by the common reckoning, they seemed to have been set by a current to the eastward about 20 miles per day, between February 10th and 15th, while they were passing from 17° to 5° north latitude, at the distance of about 11° westward of the coast of Guinea; and he was told that ships passing near the coast of Guinea always meet currents, which set them in on the land, which are so much the stronger the nearer they approach the coast; as on the contrary if they approach the opposite coast of the Brasils, they will be set by a current to the westward, which appeared to have been their case; for between February 19th and 28th, during which time they passed by the most eastern part of the coast of Brasils, leaving Cape St. Augustin only 6° to the west, they appeared to have been set by a current, at the rate of 20 miles per day to the westward, and from this time to their arrival at St. Helena, they seemed to have been continually set to the westward, though slower than before; which must have been owing to their approaching so much nearer, and continuing so much longer near the eastern coast of South America, than the western coast of Africa.

Though Mr. M. had no observation of the moon within less than 8 days of his arrival at St. Helena, he makes the longitude of the island, by his account, to be only $1\frac{1}{2}$° east of its true situation, which is $5\frac{3}{4}$° west of London; whereas the account kept in the common manner made the island $1\frac{1}{2}$° east of London, or $7\frac{1}{4}$° east of its true situation, and most of the accounts on board the ship made it 10° east of the true longitude. Having got 12 observations in the compass of 11 days, between March 9th and 20th, he had the curiosity to compare them together. Setting aside any errors in the common method of keeping a ship's reckoning, and supposing her not to be affected by currents during this time, the same difference ought to have been found between all the longitudes by account, and the longitudes deduced from the moon, or the same error of the common account ought to have resulted from all the observations. The mean error of account from all the 12 observations is 5° 20′, by which they were really more to the west than the account made them. And comparing each particular error of account with this quantity, the difference between them, in any of the 12 observations, scarcely exceeds a degree; whence we may suppose, that the longitude was deduced truly from every one of these 12 observations, within the compass of less than $1\frac{1}{2}$°.

Mr. M. has set down in the annexed table the error of the common account, and the difference between 5° 20′, the mean quantity of it, and each particular error of account, which, except in the first and last observations, does not exceed $\frac{3}{4}$°. The last observation, which differs most from the medium, was taken in some haste, on account of the position of the sails of the ship, which did not allow an uninterrupted view of the star; yet as he was tolerably satisfied with

the observation at the time, and as it does not materially differ from the others, he did not think proper to reject it.

1761.	Error of account.	Difference between 5° 20′, and each particular error of account.
March 9	4° 12′	— 1° 8′
10	5 0	— 0 20
13	5 36	+ 0 16
	5 25	+ 0 5
15	4 44	— 0 36
17	5 52	+ 0 32
	6 7	+ 0 47
18	5 13	— 0 7
	5 46	+ 0 26
19	4 34	— 0 46
20	4 52	— 0 28
	6 37	+ 1 17
	5 20	mean error of account.

After finding so great an agreement in the result of all the different observations, whether made on the same or different stars, or on the same or different nights, he cannot account for the great difference found by the Abbé De la Caille, in the result of several observations taken by himself, and a friend of his, at land, which ought to agree still nearer with one another than those made at sea. Mr. M. cannot conceive that such able observers could be liable to an error of 5′ in measuring the distance of a star from the moon's limb, if their instruments were not faulty. The most likely and the most common cause of error lies in the speculums and dark glasses; for if these are not ground truly parallel, which he is afraid they very often are not, by the common methods, they may easily produce a refraction of some minutes.

As a proof how near different observations made in the compass of an hour or two will agree in giving the same longitude, February 11th, by 10 different observations of the distance of the moon from the sun, he made the longitude, reduced to noon as usual, 28° 57′, 29° 50′, 29° 16′, 29° 22′, 29° 53′, 28° 59′ 29° 30′, 29° 48′, 29° 30′, 29° 30′; none of which differ above half a degree from 29° 22′, which is the medium of them all. March 18th, by 4 different observations of the distance of Pollux from the moon, he found the longitude 23° 52′ twice, and 24° 8′ twice. He never found that a single observation would give the longitude above a degree different from the medium resulting from 3 or 4 observations, and seldom above half a degree; which argues, that the error of any single observation never exceeded 2 minutes, and seldom 1 minute.

From the whole, Mr. M. congratulates the curious astronomer and ingenious mariner, that the method of finding the longitude, proposed by Sir Isaac Newton, is by the improvement of the theory, of which he laid the foundation, and, by the great perfection to which our artists have carried the construction of instruments, rendered practicable in our times, at sea as well as at land, to a de-

gree of exactness sufficient to make it of great and valuable utility to the extensive navigation and commerce of our native country.

XCIV. Reasons for a Lunar Atmosphere. By Mr. Samuel Dunn. p. 578.

It has been a question which has been long debated among astronomers, whether the moon has an atmosphere or not? and the question it seems is not yet undecided. The best astronomers told him they never could discover any atmosphere about the moon. But this being unsatisfactory, Mr. D. began to consider by what methods, not already used, this problem was likely to be solved; and, among several others, thought of one, which he thought had not been used before, viz. by a nice examination of the two ends of Saturn's ring, at the time when the planet is on the dark edge of the moon. For the ring of Saturn being of a considerable length, and gradually emerging or immerging almost at right angles, either from or to the dark disk of the moon, the two extremities of this ring, and the body of Saturn, being duly observed, if both the preceding and subsequent extremities of the ring, and the body of Saturn also, should happen to appear not perfectly defined, exceedingly near to the moon's dark limb, but perfectly defined a little farther from it; by such an appearance, he concluded it might be strongly presumed, that there is a lunar atmosphere; and for want of such appearance, that there is none.

Such an observation he made 16th instant, (June 1762) or rather 17th, past 2 in the morning; the particulars of which follow.

The 16th at noon he set a pendulum clock, by which this observation was made, to solar time, by the sun's transit over the meridian. He waited to make this observation, but could not see the moon till $14^h\ 22^m$, when she emerged from dark still clouds into a most clear and serene sky, nothing could be finer for observation; and thus she continued during the observation, and long after it. The telescope being rightly adjusted, at $14^h\ 21^m\ 3^s$, he saw a faint point of light, where the emersion afterwards appeared; but this faint point of light appearing and disappearing by alternate fits, he could not know if it was part of Saturn or of one of his satellites, till it was $14^h\ 21^m\ 13^s$, when this point of light was grown a little brighter and larger, and therefore he judged it was the tip of the ring just emerging. Yet it appeared so dull and hazy, that he had suspected his telescope, if he had not known it to have been rightly adjusted.

At $14^h\ 22^m\ 4^s$, the preceding part of the ring was emerged, and it appeared more bright; and now the body seemed emerging or emerged, but so very hazy and ill defined, both the body and the ring confused together, closely on the moon's dark limb, that he should not have taken it for Saturn, but for a comet emerging from behind the moon, had he not known otherwise from the tables, or seen Saturn the preceding mornings.

At $14^h\ 22^m\ 30^s$, the preceding end of the ring more plain and bright, the subsequent end of the ring more dull, and the body at this time appeared a little more distinct than before.

At $14^h\ 22^m\ 34^s$, the subsequent end of the ring appeared most dull, and the preceding end clear; after which, in some short space of time, the whole ring and body of Saturn appeared sharply and well defined.

Therefore, he concluded, that this diversity of appearance must have arisen from the effects of an atmosphere of the moon.

XCV. An Account of the Comet seen at Paris in June 1762. *By Mons. De la Lande.* p. 581.

Longitude of the ascending node............	11^s	19^0	23′
Longitude of the perihelion.................	3	15	14
Inclination		34	45

Passage through the perihelion May 28, at $15^h\ 27^m$, middle time.

Perihelion distance 1.0124, supposing the distance of the sun from the earth to be 1.

They were not able to observe this comet later than the 5th of July. It was even at too great a distance on that day; and was but ill observed from the 30th of June. This comet resembles none of the 49 comets, whose elements are already known.

XCVI. Minutes of the Observation of the Transit of Venus over the Sun, June 6, 1761, *taken at Calcutta in Bengal, Latitude* 22° 30′, *Longitude East from London nearly* 92°. *By Mr. Wm. Magee,* p. 582.

The appulse uncertain, but very apparent at	8^h	11^m	35^s
The centre of Venus on the sun's limb	8	16	35
The interior contact at the ingress	8	24	40
Interior contact at the egress.............	2	15	55
Centre of Venus on the sun's limb at the egress	2	24	0
Total egress	2	32	0

The above observations were minuted from a stop watch of Mr. Ellicott's, having no pendulum clock or time piece. The weather being cloudy for several days before that of observation, there was no opportunity of ascertaining the error of the watch; but on the day of observation he found, on comparing the watch with a meridian line in the town hall, that when the centre of the sun's image was on the meridian line, the time by the watch was $4^m\ 10^s$ past 12. Comparing the watch the 7th, 8th, and 9th June with the meridian line, he found it had gained nearly 2 minutes each day, the time by the watch June 9 being $10^m\ 5^s$, when the sun was on the meridian. It took about 5^s to wind the

watch up every day, which he carefully observed. From the foregoing remarks, he made the following corrections.

Centre of Venus on the sun's limb at ingress	8^h	12^m	54^s
Interior contact at the ingress	8	20	58
Interior contact at the egress	2	11	34
Centre of Venus on the sun's limb at the egress	2	19	38
Total egress	2	27	38
And if the same time be allowed for the first semidiameter of the planet coming on as the other 3, it must have commenced at 8^h 4^m 50^s, so that the total duration was	6	22	48

XCVII. A further Account of the Case of the Family at Wattisham, in Suffolk, whose Limbs mortified. By Charlton Wollaston, M.D., F.R.S. Dated Pall Mall, Oct. 29, 1762.* p. 584.

As the Society may be curious to know some further particulars relating to this singular calamity, I thought it might not be improper to acquaint them that most of the unhappy sufferers have survived it. The father is perfectly recovered; except that the two fingers, which were particularly affected, remain in some degree contracted.

The mother is still alive. In my former account I mentioned that one of her feet had separated at the ancle; and that the other leg was perfectly sphacelated to within a few inches of the knee, but not then taken off. Some little time afterwards the husband broke off the tibia, which was quite decayed, about 3 inches below the knee: the fibula was not decayed; so the surgeon sawed it off. The stumps of both legs still continue unhealed; and as the ends of the bones in both of them seem to be carious, and the woman will not consent to any further operation, they may perhaps never heal. The mortification however has not in this limb, nor indeed in any one of these cases, spread beyond the original separation. Her right arm is considerably wasted, and the fingers contracted. The eldest girl Mary died within a few weeks after I saw her. The 2d girl Elizabeth is perfectly well: the sores quite healed. The 3d girl Sarah is not yet well. Her foot separated at the articulation of the os scaphoides with the astragalus. The os calcis and astragalus are both of them carious, and probably keep the wound from healing. The 2 boys are perfectly recovered; and seem in every respect as healthy as possible.

I have taken all the pains I could to inquire into the cause of so remarkable a disorder; and Mr. Bones, the minister of the village, who knew the family before this misfortune happened to them, and has ever since been indefatigable

* See p. 626.

in his attention and tenderness to them, has also made all the inquiry in his power: but we have not been able to find that there was any thing particular either in their diet or manner of life, to which it could be attributed. The corn with which they made their bread was certainly very bad: it was wheat, that had been cut in a rainy season, and had lain on the ground till many of the grains were black and totally decayed: but many other poor families in the same village made use of the same corn, without receiving any injury from it. One man lost the use of his arm for some time; and still imagines himself that he was afflicted with the same disorder as Downing's family: but by what I could learn from him, there seemed to be no reason for this supposition. He is long since perfectly recovered.

XCVIII. Observations on the Tides in the Island of St. Helena. By the Rev. Nevil Maskelyne, A.M., F.R.S. Dated St. Helena, Jan. 26, 1762. p. 586.

For this purpose Mr. M. had a post about 10 feet long erected in a convenient place in the harbour before James's fort, which was the properest situation that could be found, being to the leeward part of the island, where ships may ride at anchor safely all the year round. One side of it was painted black, over which white strokes were painted at the distance of 3 inches, which were marked with the figures 1, 2, 3, &c. according to the order of the strokes, reckoning upwards from the bottom. The water sunk down to about the figure 0 at the new and full moons. The following example of his method of making these observations may serve to give an idea of the whole. When the water sunk, he took its altitude on the post at the lowest point, and immediately as it rose again, he took it at the highest, and repeating the experiment in this manner, he at last took a mean of all the observations for the true height of the water. But the medium of the lowest and highest which immediately succeed each other seldom differed much from the medium of them all. The numbers on the post by which the altitudes were taken, are at the distance of 3 inches from each other, as before observed. ☽ November 16, 1761, $5^h\ 16^m$ P.M. he took the following observations of the altitude of the water by the side of the post 11, 12; $9\frac{1}{2}$, $13\frac{1}{2}$; 10, $12\frac{1}{2}$; 9, $14\frac{1}{2}$; 9, $12\frac{3}{4}$; $9\frac{1}{2}$, $13\frac{1}{2}$; 10, 13; 9, 14; 9, 13; 9, 14. The medium of all is $11\frac{4}{10}$ for the true altitude of the water. Mr. Mason at the same time by about as many observations found $11\frac{5}{10}$.

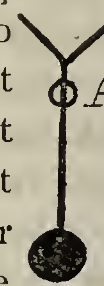
Some times, when the rise and fall of the water was very quick, instead of taking the more regular rise and fall of the water, which succeed each other at longer intervals, he took notice of every the least rise and fall, in which case he had an assistant to write them down as fast as he told them. As an example of this ☿ December 2, $11^h\ 15^m$ A.M. by a mean of 69 observations taken in this manner, he found the altitude of the water to be $3\frac{45}{100}$; and at $11^h\ 21^m$ A.M. by

a mean of 52 observations, taken at the more regular rising and fallings of the water, he found the altitude to be $3\frac{68}{100}$. He always looked at his watch before he began to note the height of the water, and looked at it again when he had finished the experiment; the medium of the two times he set down as the true time of the observation. The times set down are exact to the minute.

Mr. M. says nothing with respect to any conclusions that may be drawn from the above observations, except that the greatest rise and fall of the water, observed at the sysigies of the sun and moon, is about 13 divisions of the post, or 39 inches; and that the smallest rise and fall in the quadratures, is somewhat less than 7 divisions of the post, or about 20 inches; and that the mean time of high water happens 2^h 15^m after the moon's passing the meridian, though in the course of every fortnight the said interval is very much varied by the different influence of the sun at different times, as the theory requires.

XCIX. Extract of a Letter from M. de la Lande, at Paris, to the Rev. Nevil Maskelyne, F. R. S. Dated Paris, Nov. 18, 1762. p. 607.

I am glad that you have proved, from your own experience, the exactness of the observations of the distance of the moon from stars for finding the longitude at sea, as M. de la Caille had done in 1753. I am as fully convinced as you can be of these advantages, and am not a little pleased to learn that you are about printing a concise method of computing the corrections of refraction and parallax.



In the sector, which our members of the academy carried with them to the north, the plumb-line descends from an angle as in this figure, so that it is obliged to fall into the vertex of the angle; and it passes over a point A, with which it is made to correspond, by the help of a microscope. It is a pity that Mr. Sisson neglected so essential a circumstance in your sector, but that is not your fault. The sector, with which M. de la Caille made all his observations, and which is come into my hands since his death, has a fine needle at the centre, from which the silver wire is suspended by a loop, thus ....

M. Pingre, who is returned from the island of Rodrigues, has found the parallax of the sun to be the same as I have done; namely $9''\frac{2}{5}$. I am not surprized that you find it to be only $8''\frac{3}{5}$, since the Swedish observations, which appear to me to be very good, make it still less than you have found it. These uncertainties arise from our not having the difference of the meridians of the Cape, Rodrigues, Tobolski, Paris, and London, well determined. You are therefore quite right to collect together the observations of Jupiter's satellites, which will serve to find these longitudes. I thank you for those which you have sent me, and I have hereto added those of the first satellite which were made at Paris in 1761, for one can scarcely employ any but these for this purpose.

On the 19th of July 1763 we shall have an occultation of Antares by the moon, on the 2d of November an occultation of Mars, on the 8th of September one of Mercury: they will be very proper for determining the difference of longitude between London and Paris. In 1764 there will be a still greater number. But if you have an inclination to undertake a labour of this kind, you may meet in the memoirs of the academy with occultations of stars observed at different times, and find some corresponding ones made at London, whence you may deduce the difference of the meridians of these two cities, which we may be ashamed to say we are uncertain of to 20^s. For whether it be $9^m\ 15^s$ or $9^m\ 40^s$ is difficult to determine: I mean of Paris and Greenwich.

I beg you will answer for me the questions proposed to me by our worthy friend Dr. Morton on the part of Mr. Dunn. I observed the exit of Venus at Paris with a telescope of 18 feet, and an eye glass of $2\frac{1}{2}$ inches focus, and with a smoaked glass which was sufficiently dark, but I was not uncertain so much as a single second. M. Messier observed with a Gregorian telescope of $2\frac{1}{2}$ feet, magnifying very nearly the same as mine, and he agrees very well with me. M. Maraldi had a refracting telescope of 15 feet, but he was tired at the time; and M. de la Caille had a refracting telescope of M. Dollond's, which was not well put together, and did not terminate objects distinctly. I took for the moment of the contact the 1st instant of Venus's limb raising the sun's limb in the slightest manner. The account of these observations will be in the memoirs of the academy for 1761, which is almost printed off.

We reckon the longitude between Greenwich and Paris to be $9^m\ 20^s$: but I do not know what are the observations upon which it is founded. The preceding observations will contribute hereto.

C. The Observations of the Internal Contact of Venus with the Sun's Limb, in the late Transit, made in different Places of Europe, compared with the Time of the same Contact observed at the Cape of Good Hope, and the Parallax of the Sun from thence determined. By James Short, A.M., F.R.S.* p. 611.

In the summer of the year 1760, the R.S. resolved to send some fit persons to proper places of the globe, in order to observe the transit of Venus, which was to

* Mr. James Short, an eminent optician and constructor of telescopes, was the son of a joiner at Edinburgh, where he was born in 1710, and died at Newington Butts, near London, in 1768, consequently at 58 years of age. When 10 years old, his parents being both dead, he was placed in Heriot's charity hospital at Edinburgh. Having manifested however uncommon talents for mechanics, &c. 2 years after, he was sent to the high school of that city, where he so much distinguished himself in classical learning, that his friends thought of qualifying him for a learned profession. After 4 years spent at the high school, in 1726 he entered as a student in the university of Edinburgh, where he passed through a regular course of study; took his degree of master of arts;

happen on the 6th of June, 1761. In consequence of this resolution, they appointed Messrs. Maskelyne and Waddington to go to the island of St. Helena, and Messrs. Mason and Dixon to go to Bencoolen, a settlement belonging to the East India Company on the island of Sumatra. Two reflecting telescopes of 2 feet focal length each, with an object glass micrometer of 40 feet focus adapted to one of them, an astronomical clock, and an equal altitude instrument, were ordered by the Society for each of those places. The munificence of his late and present majesty, patrons of the R. S. defrayed the expence.

Mr. Maskelyne and his assistant arrived at St. Helena in the month of April 1761; but Mr. Mason and his assistant, being detained at Plymouth by an accident, on their arrival at the Cape of Good Hope in the month of April 1761, found it was too late to reach Bencoolen, and therefore resolved to stay at the

and at the earnest intreaties of his relations, attended the divinity hall, and in 1731 passed his trials to fit him for a preacher in the church of Scotland. Soon after this however the mind of our young artist began to revolt against the idea of a profession so little suited to his talents; and having had occasion to attend a course of Mr. Maclaurin's mathematical class in the college, he there so much distinguished himself, that the professor took great notice of him, and invited him often to his house, where he had an opportunity of knowing more fully the extent of his capacity. In 1732, Mr. M. kindly permited Mr. S. to make use of his rooms in the college for his apparatus, where he began to work in his new profession of telescope making, under the eye of his eminent master and patron; who, in a letter about 2 years after to Dr. Jurin, mentions the proficiency made by Mr. Short in constructing reflecting telescopes in these words: "Mr. Short, who had begun with making glass specula, is now employing himself to improve the metallic. By taking care of the figure, he is enabled to give them larger apertures than others have done; and, upon the whole, they surpass in perfection all that I have seen of other workmen." The figure which Mr. S. gave to his great specula was parabolical: which he did however not by any rule or canon, but by practice and mechanical devices. Mr. S. continued from this time to practise his art as a regular profession, with great success; so that when, in the year 1736, he was called up to London, at the desire of queen Caroline, to give instructions in mathematics to William, Duke of Cumberland, he had cleared the sum of 500l. by the profits of his business. Towards the end of the same year he returned again to Edinburgh; and having made several useful improvements in his art during his stay in England, he now prosecuted it with fresh vigour and success. In 1739, being then again at London, the Earl of Morton took Mr. S. with him on a tour to the Orkney isles, and engaged him there to adjust the geography of that part of Scotland. He returned to London with the Earl, and finally established himself there in the line of his profession. In 1743 he was employed by Lord Thomas Spencer, to make a reflector of 12 feet focus, the largest that he ever constructed, except those for the king of Spain, and some others of the same focal distance, with great improvements and higher magnifiers. The telescope for the king of Spain was finished in the year 1752, which, with its whole apparatus, cost 1200l. But the instrument made for Lord Thomas Spencer, having fewer accompaniments, was purchased for 600 guineas. From the great profits and success of his trade, Mr. S. left at his death a fortune of 20,000l.

Mr. S. was a good general scholar, and well skilled in optics and mathematical learning. He was a very useful member of the R. S. and wrote a number of excellent papers in the Philos. Trans. And his determination of the sun's parallax at about $8\frac{2}{3}''$, from his ingenious calculations on the transit of Venus, in the above Memoir, has been pretty generally adopted by astronomers.

Cape, and to make their observations there, and it was extremely fortunate they did so, for, by reason of cloudy weather, Mr. Maskelyne was hindered from making the proper observations, and in that case, the observation of the internal contact at the egress at Bencoolen, when compared with the same observation at Greenwich, could have determined nothing with regard to the parallax of the sun.

To determine the parallax of the sun, by means of the observations of the internal contact of Venus with the sun's limb, made at two different places, it is absolutely necessary that the difference of longitude between these two places be exactly determined. For this purpose Mr. Mason applied himself assiduously to observe the eclipses of Jupiter's satellites, and Mr. Green, the assistant observer at Greenwich, observed as many of the same eclipses, as the unfavourable season would allow. Dr. Bevis and myself likewise observed the same eclipses in Surry-street, London. The insufficiency of these sort of observations in determining the longitude of places where accuracy is required, is well known to those who have the practice of them.

By comparing the observations of the first and second satellites made at the Cape with those made in Surry-street, the difference of longitude between Greenwich and the Cape comes out, on a mean, $= 1^h\ 13^m\ 30^s$, and rejecting those of the 2d satellite, which are always more uncertain than those of the first, I fix the difference of longitude between Greenwich and the Cape of Good Hope $= 1^h\ 13^m\ 35^s$, which I have made use of in the following computations.

In order to ascertain the sun's parallax with more certainty, I have compared the observation of the internal contact at the egress at the Cape, with the observations of the same contact made at 15 different places in Europe. But before proceeding any farther I shall mention the times of such observations whence I had them; and the longitudes of those places, and whence I likewise had those.

Internal contact at	h. m. s.	From	Longitude	h. m. s.	From.
Greenwich........at..	8 19 0..	Phil. Trans....	from the Cape .. =	1 13 35 w.	Phil. Trans.
Shirburn castle ....at..	8 15 10..	ditto.........	from Greenwich =	0 4 1 w.	ditto.
	8 15 14..				
Saville house ......at..	8 18 22..	ditto	from Greenwich =	0 0 30 w.	ditto.
Leskeard ....at..	8 0 21..	ditto	from Greenwich =	0 18 32 w.	ditto.
Paris ....at..	8 28 25..	ditto	from Greenwich =	0 9 10 E.	Con. des Tems.
	8 28 29				
Bologna..........at..	9 4 54..	ditto	from Paris .... =	0 36 5 E.	ditto
	9 5 0				
Rome............at..	9 9 36..	a private letter	from Paris .... =	0 40 37 E.	ditto
Drontheim........at..	9 1 49..	a private letter	from Greenwich =	0 43 58 E.	a private letter
Upsal............at..	9 28 3..	Phil. Trans....	from Paris .... =	1 1 10 E.	Phil. Trans.
	9 28 9				
Stockholm........at..	9 30 8..	ditto	from Paris .... =	1 3 10 E.	Con. des Tems.
	9 30 11				
Hernosand ......at..	9 28 52..	Swedish acts ..	from Paris .... =	1 2 12 E.	Phil. Trans.
Calmar ....at..	9 23 40..	ditto	from Stockholm =	0 6 27 w.	Swedish acts

Internal contact at	h. m. s.	From	Longitude	h. m. s.	From.
Abo ....at*	9 45 59.	. Phil. Trans....	from Paris .... =	1 19 17 E.	Phil. Trans.
Tornea ....at..	9 54 8.	. ditto	from Paris .... =	1 27 28 E.	ditto
Cajaneburg ......at..	10 8 59.	. Swedish acts ..	from Stockholm =	0. 39 20 E.	Swedish acts
Cape of Good Hope at..	9 39 48.	. Phil. Trans....	from Greenwich =	1 13 35 E.	Phil. Trans.
	9 39 52.				

As there were several observers at some places, I have therefore set down the two extreme observations, that I may afterwards take the mean of them. One reason I apprehend why the observers, even at the same place, differ some seconds in the time of the contact, may arise from this, that some of them judged of the contact when there was no light between the limb of Venus and that of the sun, others did not imagine the contact to happen till they lost a part of the circumference of Venus; I shall therefore set down the difference between the observers at the same place, that an estimate may be formed of the limit of this error.

	Observers.		Diff. in obs.
At	Greenwich......	3...... =	0 sec.
	Bologna........	4...... =	6
	Paris†..........	6... .. =	16
	Shirburn castle ..	2...... =	4
	Cambridge......	2...... =	7
	Stockholm	2...... =	3
	Upsal..........	3...... =	6
	Tornea	2...... =	14
	Cape of G. Hope	2...... =	4
	Carlscrona......	2...... =	6

The mean therefore of all these comes out to be $= 6^s.6$, an error that may be committed in judging of the contact, even at the same place.

After having computed the parallaxes of longitude and latitude, on the supposition that the sun's parallax was $= 8\frac{1}{2}''$, for each of the above places, I compared the observation at each place with the observation at the Cape of Good Hope, and deduced the sun's parallax from each, as may be seen more fully in the following table.

1		2		3	
h. m. s.	m. s.	h. m. s.	m. s.	h. m. s.	m. s.
9 39 50 Cape	6 8 Cape	9 39 50 Cape	6 8 Cape	9 39 50 Cape	6 8 Cape
1 13 35=D. M.	1 11 Greenw.	1 17 36=D. M.	1 12 Shirburn	1 14 5=D.M	1 11 Saville house
8 26 15	7 19	8 22 14	7 20	8 25 45	7 19
8 19 0 Greenwich	− 4″	8 15 12 Shirburn	− 18″	8 18 22 Saville house	+ 4″
7 15 sun's parallax	= 8″.42	7 2 sun's parallax	8″.15	7 23 sun's parallax	= 8″.57
4		5		6	
h. m. s.	m. s.	h. m. s.	m. s.	h. m. s.	m. s.
9 39 50 Cape	6 8 Cape	9 39 50 Cape	6 8 Cape	9 39 50 Cape	6 8 Cape
1 32 7=D. M:	1 4 Leskeard	1 4 25=D. M.	54 Paris	0 28 20=D. M.	29 Bologna
8 7 43	7 12	8 35 25	7 2	9 11 30	6 37
8 0 21 Leskeard	+ 10″	8 28 27 Paris	− 4″	9 4 57 Bologna	− 4″
7 22 sun's parallax	= 8″.69	6 58 sun's parallax	= 8″.42	6 33 sun's parallax	= 8″.41

* In the Phil. Trans. it is at 9h 46′ 59″, but that this is a mistake, in writing down the minutes, may be easily proved.—Orig.

† M. de la Lande, in a letter to Mr. Maskelyne, says that M. de la Caille observed with a tele-

7

h. m. s.		m. s.		
9 39 50	Cape	6 8	Cape	
0 23 48	=D. M.	13	Rome	
9 16 2		6 21		
9 9 36	Rome			+ 5″
6 26				
		sun's parallax = 8″.61		

8

h. m. s.		m. s.		
9 39 50	Cape	6 8	Cape	
0 29 32	=D. M.	2 38	Dronth.	
9 10 18		8 46		
9 1 49	Drontheim			− 17″
8 29				
		sun's parallax = 8″.23		

9

h. m. s.		m. s.		
9 39 50	Cape	6 8	Cape	
0 3 15	=D. M.	2 21	Upsal	
9 36 35		8 29		
9 28 6	Upsal			− 0″
8 29				
		sun's parallax = 8″.50		

10

h. m. s.		m. s.		
9 39 50	Cape	6 8	Cape	
0 1 15	=D. M.	2 18	Stockh.	
9 38 35		8 26		
9 30 10	Stockholm			− 1″
8 25				
		sun's parallax = 8″.48		

11

h. m. s.		m. s.		
9 39 50	Cape	6 8	Cape	
0 2 13	=D. M.	2 26	Hernos.	
9 37 37		8 34		
9 28 52	Hernosand			+ 11″
8 45				
		sun's parallax = 8″.68		

12

h. m. s.		m. s.		
9 39 50	Cape	6 8	Cape	
0 7 42	=D. M.	1 59	Calmar	
9 32 8		8 7		
9 23 40	Calmar			+ 21″
8 28				
		sun's parallax = 8″.86		

13

h. m. s.		m. s.		
9 39 50	Cape	6 8	Cape	
0 14 52	=D. M.	2 30	Abo	
9 54 42		8 38		
9 45 59	Abo			− 5″
8 43				
		sun's parallax = 8″.58		

14

h. m. s.		m. s.		
9 39 50	Cape	6 8	Cape	
0 23 3	=D. M.	3 5	Tornea	
10 2 53		9 13		
0 14 8	Tornea			+ 28″
8 45				
		sun's parallax = 8″.07		

15

h. m. s.		m. s.		
9 39 50	Cape	6 8	Cape	
0 38 5	=D. M.	2 59	Cajaneburg	
10 17 55		9 7		
10 8 59	Cajaneburg			+ 11″
8 56				
		sun's parallax = 8″.33		

I shall explain this table by the example of the observation at Greenwich compared with the observation at the Cape, which is the first in the table. Thus, $9^h\ 39^m\ 50^s$ is the mean of the times of the internal contact observed at the Cape, $1^h\ 13^m\ 35^s$ is the difference of longitude between Greenwich and the Cape, this being subtracted from the time of the observed contact at the Cape, leaves $8^h\ 26^m\ 15^s$ for the time the observers at Greenwich should have seen the contact, if there had been no parallax of Venus; subtracting therefore the time of the observed contact at Greenwich from this time, the remainder $7^m\ 15^s$ is the effect of the parallaxes of longitude and latitude at the two places of observation. But the effect of these two parallaxes at the Cape, on the supposition of the sun's parallax being = $8\frac{1}{2}''$, is = $6^m\ 8^s$, by which quantity of time the observers at the Cape should have seen the internal contact later than at the centre of the earth; and the effect of the same parallaxes at Greenwich is = $1^m\ 11^s$, by which quantity of time the observers at Greenwich should have seen the internal con-

scope of Mr. Dolland's construction, which was not well fitted up; it may therefore be presumed that there is some mistake in the observation of M. Maraldi, because his observation is later than that of M. de la Caille, and differs so considerably from the rest.—Orig.

tact sooner than at the centre of the earth. The sum therefore of these two quantities is = $7^m\ 19^s$, by which quantity of time the observers at Greenwich should have seen the internal contact sooner than the observers at the Cape in absolute time, had the sun's parallax been = $8\frac{1}{2}''$. But the difference in absolute time as found by observation as above, is only = $7^m\ 15^s$, therefore the sun's parallax by supposition, viz. $8''.5$, is to the parallax of the sun found by observation, as $7^m\ 19^s$ is to $7^m\ 15^s$, which gives $8''.42$ for the sun's parallax, on the day of the transit, by this observation, which numbers and result are set down in number 1st, and so of all the rest in the table. By taking a mean of the results of these 15 observations, the parallax of the sun, on the day of the transit, comes out = $8''.47$, and by rejecting the 2d, the 8th, the 12th, and 14th results, which differ the most from the rest, the sun's parallax, on the day of the transit, by the mean of the 11 remaining ones, is = $8''.52$.

We have received from Sweden several observations of the total duration of the transit from the internal contact at the ingress, to the internal contact at the egress, and also the observation of the same duration by M. Chappe at Tobolsk in Siberia, and by several persons in the East Indies; but the differences between these durations are too small to determine with any accuracy, the sun's parallax from them, by comparing the duration at one place with the duration at another. The greatest difference between them, and the duration at Tobolsk, which is the least, amounting only to $2^m\ 50^s$, and the least difference amounting only to $1^m\ 4^s$: in which small quantities the unavoidable errors of observations must bear a considerable proportion, and yet by comparing 15 total durations observed at different places with the total duration observed at Tobolsk, I find the annexed results of the sun's parallax from each of them.

Cajaneburg .... =	9s.	50
Calmar .... =	9	0
Calcutta .... =	9	50
Hernosand .... =	9	50
Abo................ =	8	75
Stockholm .... =	11	0—
Stockholm .... =	10	75—
Upsal .... =	7	25
Upsal .... =	6	70
Upsal .... =	9	0
Tornea.............. =	12	0—
Tornea.............. =	14	0—
Madras .... =	9	70
G. Mount .... =	8	50
Tranquebar.......... =	8	75

The mean of these 15 results gives the sun's parallax = $9''.56$; and if we reject 4 of them, which differ the most from the rest, the mean, of the remaining 11, gives the sun's parallax = $8''.69$.

However, on calculation by another method, I find so great an agreement between them, on the supposition that the sun's parallax is = $8\frac{1}{2}''$, that I have determined the sun's parallax from them also, as a corroboration of the sun's parallax being very nearly the same as found by the observations of the internal contact; and this is done in such a manner that each observation of the total duration at any one place, determines the sun's parallax independent of any observation of the same duration made at any other place, and in which an exact knowledge of the longitude, of the latitude, and of the time at the place of ob-

servation, is not required; all that is necessary to be known, is the time of duration from the internal contact at the ingress, to the internal contact at the egress, and that the clock moved equably during the interval of the contacts, and that the least distance of the centres of the sun and Venus, as seen from the centre of the earth, is also known.

The method I have followed in this inquiry, was by finding the total duration at the centre of the earth: in order to find this, it was necessary to know the least distance of the centres of the sun and Venus as seen from the earth's centre. By the measurements of the distance of the limb of Venus from the sun's limb taken at Savile-house, and also by the like measurements taken by Mr. Haydon at Leskeard, I found, on the above supposition of the sun's parallax, that the least distance of the centres, as seen from the centre of the earth, was $= 9' 32''$. The total duration therefore at the centre of the earth was $5^h\ 58^m\ 1^s$. I have compared the several observations of the total duration with this central duration, and from each I have determined the sun's parallax, as may be seen more fully in the following table. I have inserted in this table the alteration of duration by one second of the sun's parallax at each place, by which may be seen the quantity of error in the determination of the sun's parallax arising from any quantity of error in the observation.

The times of the total duration at those different places I have taken from the Phil. Trans.; only those of Calmar and Cajaneburg I have taken from the Swedish acts; the internal contact at the ingress at Cajaneburg, in those acts, is at $4^h\ 18^m\ 5^s$, whereas it should be at $4^h\ 19^m\ 5^s$, as may be easily proved, this being an error in writing down the minutes, which has happened more than once in these observations, occasioned by the hurry of writing down the times of the observations. The observations in the East Indies I have taken from letters sent by the East India Company to the R.S. I have also been obliged to make a correction of the minutes in the observation at Tranquebar and at the Grand Mount, a place about 8 miles to the S.W. of Madras.

1	2	3	4	5	6	7	8	
Cajaneb.	Calmar.	Calcutta.	Hernos.	Abo.	Tobolsk.	Stockolm	Stockolm	
h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	
5 58 1	5 58 1	5 58 1	5 58 1	5 58 1	5 58 1	5 58 1	5 58 1	Duration at the centre of the earth.
8 5	7 21	7 30	7 36	7 46	9 3	7 34	7 34	Effect of parallaxes, from a parallax of 8.5
5 49 56	5 50 40	5 50 31	5 50 25	5 50 15	5 48 58	5 50 27	5 50 27	Duration from a parallax of 8″.5 of the sun.
5 49 54	5 50 39	5 50 36	5 50 26	5 50 9	5 48 50	5 50 45	5 50 42	Duration observed.
− 2	− 1	+ 5	+ 1	− 6	− 8	+ 18	+ 15	Diff. of observation and a parallax of 8″.5
9° 48′	0° 44′	39° 30′	4° 4′	5° 43′	26° 50′	3° 24′	3° 24′	Altitude of the sun at ingress without refract.
57″	52″	53″	54″	55″	64″	54″	54″	Alteration of duration by 1″ of ⊙'s parallax.
8″.53	8″.52	8″.49	8″.48	8″.61	8″.63	8″.16	8″.22	Parallax of the sun from the observation.

9	10	11	12	13	14	15	16	
Upsal.	Upsal.	Upsal.	Tornea.	Tornea.	Madras.	G. Mount	Tranq.	
h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	
5 58 1	5 58 1	5 58 1	5 58 1	5 58 1	5 58 1	5 58 1	5 58 1	Duration at the centre of the earth.
7 33	7 33	7 33	8 7	8 7	6 33	6 33	6 24	Effect of parallaxes, from a parallax of 8.5
5 50 28	5 50 28	5 50 28	5 49 54	5 49 54	5 51 28	5 51 28	5 51 37	Duration from a parallax of 8″.5 of the sun.
5 50 7	5 50 2	5 50 26	5 50 9	5 50 21	5 51 43	5 51 20	5 51 33	Duration observed.
− 21	− 26	− 2	+ 15	+ 27	+ 15	− 8	− 4	Diff. of observation and a parallax of 8″.5
3° 29′	3° 29′	3° 29′	9° 33′	9° 33′	29° 27′	29° 25′	28° 44′	Altitude of the ⊙ at ingress without refract.
54″	54″	54′	57″	57″	46″	46″	45″	Alteration of duration by 1′ of ⊙s' parallax.
8″.89	8″.98	8″.54	8″.24	8″.03	8″.17	8″.67	8″.59	Parallax of the sun from the observation.

This table explains itself. If we therefore take the mean of the sun's parallax arising from each of those 16 total durations, it will be found = 8″.48; and if we reject the observations of number 7, 8, 9, 10, 12, 13, 14, which differ the most from the rest, the mean of the 9 remaining ones gives the sun's parallax = 8″.55, agreeing to a surprizing exactness with that found by the observations of the internal contact at the egress.

The observations at Tobolsk and Cajaneburg I consider as very good ones, these differing in the total duration only 6 seconds: this error in observation, from what is gone before, may be very easily allowed. The observation of the internal contact at the ingress at Stockholm is I believe too soon, and the uncertainty of this observation may be easily granted, when it is considered that the sun was only 3 or 4 degrees above the horizon at that time, and we find a difference of 22^s between the observers at Upsal, where the sun was about the same altitude. This I apprehend was owing to the undulation on the limb of the sun, occasioned by the vapours near the horizon: but the same reason cannot be given for the observation at Tornea, where the sun was about 10° high, at the time of the internal contact at the ingress, unless there was an undulation at that altitude also, which may have been the case, though not mentioned. I have

reason to believe that the error of observation at Tornea is at the egress, where indeed the two observers differ considerably.

The parallax of the sun being thus found, by the observations of the internal contact at the egress, $= 8''.52$ on the day of the transit, the mean horizontal parallax of the sun is $= 8''.65$.

I cannot help taking notice on this occasion, of a method employed by some astronomers, of determining the diameter of Venus by the duration of the egress over the sun's limb: for I am fully satisfied that the best eye, assisted by the best telescope, and in the best and clearest air, could not see the very last contact of Venus with the sun's limb, but must have lost sight of it several seconds before she really had left the sun's limb; and this will the more plainly appear, when it is considered that every second of the diameter of Venus took up about 19^s of time in passing over the sun's limb. And to show this further, and in a stronger light, I shall mention the following particulars. Mr. Canton measured the diameter of Venus, and found it $= 58''$, but by his duration of the egress, the diameter of Venus is $= 57''.8$: the same diameter was measured by myself at Savile-house, and fonnd $= 59''$, but by the duration of the egress observed there, the diameter of Venus is $= 58''.6$. Mr. Mason also at the Cape measured the diameter of Venus, which he found $= 59''.\frac{1}{4}$, but by his duration of the egress her diameter is found $= 57''.0$: and therefore I must conclude that the diameter of Venus, found by the duration of the egress, must be always less than the true diameter for the reason given above. And since I am on this subject, I shall likewise mention the times of duration of the egress from several diameters of Venus. If the diameter of Venus be supposed $= 57''$, then the duration of the egress at London should have been $= 18^m\ 9^s$; if the diameter be $= 58''$, then the duration at London will be found $= 18^m\ 28^s$; and if the diameter be $= 59''$, then the duration of the egress at London will be found $= 18^m\ 47^s$. The diameter of Venus being $59''$, and the diameter of the sun $= 31'\ 31''$, the duration of the egress at Stockolm is $= 18^m 43^s$, at Paris $= 18^m\ 45^s$, at the Cape of Good Hope $= 18^m\ 8^s$, and at Rome $18^m\ 38^s$. The duration of the egress at this last place was observed $= 18^m\ 31^s$; and if we suppose the diameter of Venus $= 58''$, and the diameter of the sun $= 31'\ 33''$, the duration of the egress at Rome will be found $= 18^m\ 18^s$; which duration being less than the observed duration, it therefore follows that the diameter of Venus was more than $58''$ on the day of the transit; and the duration of the egress at Paris, observed by M. de la Lande and P. Clouet, by Mr. Mallet and Mr. Bergman at Upsal, by M. Chappe at Tobolsk, and by myself at Savile-house, prove the same thing.

Observations of the transit of Venus and Mercury over the sun have been reckoned by astronomers (if the sun's parallax is known) as very proper to determine the differences of the longitudes of the places of observation, even in many respects preferable to the observations of Jupiter's satellites. I shall therefore set down in the annexed table the longitudes from Greenwich observatory of the different places, where the late transit of Venus was observed, drawn from the said observations.

Places.	Long. in time.				Latitude.			
Greenwich Observatory	00^h	00^m	00^s		51°	28′	37″	N
Shirburn castle	0	3	47	W	51	39	22	N
Leskeard, Cornwall	0	18	47	W	50	26	55	N
Paris	§0	9	10	E	48	50	14	N
Bologna, Italy	0	45	15	E	44	29	36	N
Rome	0	49	38	E	41	53	54	N
Drontheim, Norway	0	44	16	E	63	26	10	N
Upsal	1	10	16	E	59	51	50	N
Stockolm	1	12	17	E	59	20	30	N
Hernosand	1	11	7	E	60	38	0	N
Calmar	1	5	28	E	56	40	30	N
Abo	1	28	18	E	60	27	0	N
Tornea	1	36	44	E	65	50	50	N
Cajaneburg	1	51	47	E	64	13	30	N
Tobolsk, Siberia *	4	32	52	E	58	12	22	N
Madrid	0	13	26	W	40	25	0	N
St. John's, Newfoundland †	3	31	12	W	47	32	0	N
Madras	5	20	10	E	13	8	0	N
Rodrigues	4	12	34	E	19	40	40	S
Cape of Good Hope	1	13	31	E	33	55	42	S
Calcutta, Bengal	5	53	44	E	22	30	0	N
St Petersburg	2	1	29	E	59	56	0	N
Tranquebar	5	18	8	E	10	56	0	N
Pondicherry	5	15	50	E	11	56	30	N
Kingston, Jamaica ‡	5	6	52	W				
Port Royal, Jamaica	5	7	2	W				

§ If the observation at Savile-house be compared with the observation of M. de la Lande at Paris, the difference of longitude between the royal observatories at Greenwich and Paris is = $9^s\ 16^m$.

* The latitude of this place was sent to the Royal Society, but no longitude of it, and therefore the internal contact at Tobolsk could not be compared with that at the Cape of Good Hope for the purpose of the sun's parallax.

† Mr. Professor Winthrop went, at the expence of the province of Massachuset's Bay, to St. John's in Newfoundland, to observe the transit of Venus, which he did with great care, and as much exactness as the low situation of the sun at that time would permit. The internal contact happened there at $4^h\ 47^m\ 17^s$. He had no other way of determining his longitude from Greenwich at that time of the year, but by taking the distance of a star from the moon, which gave him $3^h\ 20^m\ 56^s$ for his longitude from Greenwich, and therefore his observation of the internal contact could not be compared with the same observation at the Cape.

‡ The longitude of Kingston in Jamaica is determined by the observation of the transit of Mercury over the sun on the 25th of October 1743 o. s. mentioned in the Phil. Trans.: the effect of the parallaxes is considered, and here included.

The longitudes of Tornea and Madras are determined from the contact at the

egress, because I have good reason to believe that the observation at the egress at these two places was not correct, and the observation of the contact at the ingress is more certain than that of the egress, and the observers at the ingress at these two places agree to 2″.

The elements I made use of in the preceding calculations are

The sun's diameter	= 0° 31′ 31″
The diameter of Venus	= 0 0 59
Horary motion of Venus in her path	= 0 3 59.8
Angle of the orbit of Venus with the ecliptic	= 8 30 10
Distance of the centres of the sun and Venus as seen from the centre of the earth	= 0 9 35
Difference of the parallaxes of the sun and Venus	= 0 0 21.35

I shall now give the method I followed in these calculations.

In pl. 15, fig. 2, let FG represent the horizon, ZVH a vertical circle passing through the centre of Venus, PVR a circle of declination, BV a circle of latitude, EC the ecliptic, OVN the orbit of Venus, VL the parallax of altitude, VN the parallax of longitude, LN the parallax of latitude, ZVP the angle of the vertical with the circle of declination, BVP the angle of the equator with the ecliptic, ZVB the angle of the vertical with the circle of latitude, EVO the angle of the orbit of Venus with the ecliptic, ZVO the angle of the orbit of Venus with the vertical, ZP the complement of the latitude of the place, VP the complement of the declination of the planet, ZPV the horary angle or distance of the planet from the meridian, ZV the complement of the altitude of the planet.

In the triangle ZPV, the sides ZP and PV and the angle ZPV are given, therefore the angle ZVP may be found, and also the side ZV; and as the parallax of altitude is to the horizontal parallax as the cosine of the apparent altitude is to the radius, therefore LV is found. BVP added to or subtracted from ZVP, as the nature of the case requires, leaves the angle ZVB; the angle ZVB subtracted from BVO leaves ZVO = to the angle of the orbit of Venus with the vertical, ZVO = LVN. Therefore in the right-angled triangle LNV, the angle LNV, the angle LVN being given, and the side LV, the side VN, = the parallax of longitude, and the side LN, = to the parallax of latitude, may be found. The parallax of longitude is reduced to time by knowing the horary motion of Venus in her orbit or path. Thus the value of one second of longitude is known in time. But to reduce the parallax of latitude to time, in fig. 3, let ECP represent the ecliptic, OrB the path of Venus over the sun as seen from the centre of the earth, LRD the path of Venus as affected by parallax at any one place, Cr the nearest distance of the centres of the sun and Venus as seen from the centre of the earth, CR the nearest distance of the same centres as seen from the place of observation, Rr or NV the parallax of latitude, CS the sun's semidiameter, VS the semidiameter of Venus,

NV the difference of the semichords rv and RV, cv and CV = the difference of the semidiameters of the sun and Venus.

In the right-angled triangles crv and CRV, two sides are given, therefore the other sides rv and RV may be found; the difference of these two sides NV being reduced to time, by the horary motion of Venus in her path, will give the time answering to the parallax of latitude. The parallax of longitude being added to or subtracted from the parallax of latitude, as the case requires, will give the retardation or acceleration of the contact of the place of observation, after or before the contact as seen from the centre of the earth.

In all the above calculations, I have considered the place of Venus, with respect to the centre of the sun, both in right ascension and declination; and I have neglected the fractions of seconds in the results of the parallaxes of longitude and latitude, having always taken the second that was nearest to the fractional part.

I take this opportunity of acquainting the Royal Society, that I have, by means of an achromic object-glass micrometer of 40 feet focus, adapted to a reflecting telescope of two feet focal length, measured the least and greatest diameters of the sun, and I find the apogeal diameter = 31′ 28″, and the perigeal diameter = 32′ 33″.

CI. Some Suggestions concerning the Preventing the Mischiefs which happen to Ships and their Masts by Lightning. By Wm. Watson, M.D., F.R.S. p. 629.

These observations were drawn up in consequence of what had happened to the Harriot packet, in her passage to New York. 'This vessel was struck with lightning, which split the mainmast, main top mast, and main top gallant mast in pieces, ripped up the partners of the main mast, broke down the bulk head between the steerage and the hold, tore off the locks from the cabin doors, burnt the tarpaulin off the main hatches, made several holes between the coomings of the hatches and the deck, rendered all the compasses useless, broke one of the beams between decks, stove the boat, wounded one of the men very much, and the rest were stunned for some time. Most of the rigging was burnt off the mast head. The whole caused such a smoke in the ship, that taking her to be on fire below, they threw water a considerable time into the cabin; but providentially no other damage was done.'

A few years since a ship belonging to Capt. John Waddel, was almost beaten to pieces by thunder and lightning, of which a particular account is published in the Phil. Trans.* And very lately the main mast of the Bellona, a 74 gun ship, was split in pieces by the lightning, which happened in Jan. 1762. What hap-

* Vol. xlvi. p. 111.—Orig.

pened to the Harriot packet is no more than what often happens to a ship at sea, or to a church, house, or other edifice on land, when the lightning has entered into it, and cannot procure an easy passage out of it. The attempting to procure this easy passage, and thereby avert the mischiefs attending the want of it, is the more particular subject of this communication.

A few years ago the nature of thunder and lightning, which are both to be considered as different appearances of the same meteor, was very little understood. Our predecessors in all ages regarded it as an instrument of Divine vengeance. They stood too much in awe of it to consider it closely; and though the Greeks and Romans* were in possession of some observations which might have led them to a more intimate knowledge of it, they were not apprized that what they saw had any relation therewith. It was not, till by experiments and observations on the nature and properties of electricity, and comparing them with the phenomena of thunder and lightning, we were informed that electricity and thunder arose from the same cause; or to speak nearer the truth, were different modifications of the same meteor; that they varied in nothing essential, and only differed in being in degree greater or less.

The same means, which taught us the management of one, give us great reason to believe that many of the mischiefs may, by a proper and well disposed apparatus, be prevented of the other. A quantity of electricity, accumulated to a degree sufficient to destroy a large animal, will innocently discharge itself through the smallest wire. And Mr. de Romas in France has found that one of his kites, when flown with a cord composed of hemp and wire, will silently and without any report bring down the matter of thunder from a cloud; though when the apparatus has been altered, and an easy passage has been denied to it, the streams of fire have been seen an inch thick, and 10 feet long, and the report has been equal to that of a pistol. It was owing to this easy passage of lightning being interrupted, that occasioned the death of professor Richmann at Petersburg by his own apparatus.

There is great reason to think, that the mischiefs arising from thunder and lightning happen always near the place where the explosion is made; as those persons who have been present when great mischiefs have been done, universally agree, that when these accidents have happened, the report of the thunder has instantly succeeded the flash of the lightning. As the progress of light is nearly instantaneous, and that of sound somewhat more than 1100 feet in a second of time, the thunder and lightning happening in the same instant proves the explosion to have been very near. We are therefore to guard against the thunder clouds which are near us. The mast of every ship, which is beset on its tops

* See Plutarch in the life of Lysander, Pliny, Seneca, Cæsar, Livy, &c.—Orig.

with those bright lights, which our mariners call comazants, and are the feu St. Elme of the French, and were the Castor and Pollux of the ancients, is within the sphere of action of a thunder cloud. Anciently, when these were seen, they were only considered as the attendants of a storm, and no consequence was drawn from them; but now, (since Dr. Franklin's admirable discovery of conducting lightning from the clouds, we know them to be no other than a modification of the same meteor, which constitutes thunder and lightning) they demonstrate that danger is near, and therefore we should do our utmost to prevent its effects. This in Dr. W.'s opinion would be done, if a wire of iron or any other metal were connected with the spindles and iron work at the tops of masts of ships, and conducted down the sides of the masts, and from thence in any convenient direction so disposed as always to touch the sea-water. By these means, the accumulation of the matter of thunder and lightning will be prevented, to a considerable distance from the ship, by its being discharged silently by the wire, which will not be done by the masts; as these from their height, figure, and constituent parts, without an apparatus of this kind, tend to direct and conduct the lightning into the ship. But for a further explanation on this head, Dr. W. refers to p. 215, vol. 48 of the Phil. Trans., where he had considered this matter more at large.

The applying wire to the masts of ships will be neither difficult nor expensive; as a brass wire of the thickness of a large goose quill, will in most cases be large enough to answer this purpose. He prefers brass wire to iron, as less liable to rust; and any metal corroded by rust, to the centre, ceases to be of any use in directing the lightning, in the degree hoped for and expected by this apparatus. He declines entering into a minute detail of the rationale of this process; but from analogy only he mentions that the same quantity of gunpowder, which confined in a close place, will throw down a tower, or rend a rock, will, when fired loose in the open air, be almost inoffensive.

Thunder storms are very frequent and severe in Pennsylvania, and great mischiefs often happen from them; Dr. W. was informed by Dr. Franklin, that since an apparatus of the kind above mentioned, placed at the tops of the houses, has been generally used at Philadelphia, not a single instance of mischief from lightning had happened in that city. He informs further, that at Philadelphia in a thunder storm, the lightning was seen to strike the ridge of a house, on which an apparatus of this sort was erected. The lightning, like a ball of fire, ran from the ridge of the house to the apparatus, and in running down, it melted the conducting wire, without doing any damage to the house. This shows the expediency of applying either large wires or small rods, in which the melting will most probably be prevented; notwithstanding it has been repeatedly found, that

though the wire has been melted, it has never failed of first answering the purpose of a conductor, and preventing the mischiefs threatened by the lightning.

Though the mischiefs arising from lightning are not very frequent in Great Britain, yet at times they are severe enough to be very alarming. The damage occasioned by a thunder storm in July 1759 in London, and in various other places at no great distance from it, are very fresh in our memories. Dr. W. submits it therefore how far it would be attention misapplied to think of an apparatus of this sort in his Majesty's powder magazine, erecting at Purfleet. The expence would be trifling; and every argument which is produced of their expediency in preventing mischiefs arising from lightning on board of ships, will have more force in this instance; where frequently an immense quantity of gunpowder must be collected within a comparatively very small space.

CII. On the Case of the late Rev. James Bradley, D.D. Astronomer Royal. By Daniel Lysons, M.D. p. 635.

Dr. Bradley had laboured under a great oppression of spirits for a long time; and for several years before his death frequently complained of a pain in his back, sometimes attended with difficulty in the discharge of his urine, which he apprehended to proceed from the gravel. On Wednesday June 30, 1762, he rode out for the air, and on coming home complained of pain in his back, and made a large quantity of water. At 5 o'clock the next morning he found himself labouring under a total suppression of urine, from which time he never voided any without the assistance of the catheter. With its assistance however about a quart was drawn off every 12 hours, excepting one intermission; when on account of the difficulty of introducing the catheter, none was drawn off from Friday morning July the 9th, till 8 o'clock on the Saturday evening. But both before and after that time the urine was regularly drawn off every morning and evening to the time of his death, on the 13th of July.

During his illness he often complained of pains in the abdomen. And his head was frequently disordered, especially when a stool was coming away; but after that had passed off, he was always more cool and reasonable. It was the opinion of Dr. Jones, who attended him constantly in the country, as well as of Dr. Lewis, and Dr. Lysons who visited him occasionally from Oxford, that his pains were inflammatory, though not violently so. But where the inflammation was exactly seated, they could not precisely determine; as it seemed often to shift its situation, and the patient was himself incapable of giving them the necessary description, his weak state obliging him to signify his meaning more by signs than words, and those not always intelligible. As nothing positive could therefore be said with regard to the seat of the disorder, the friends of the deceased desired that his body might be opened; and Dr. Jones and Dr. L. being present at the operation,

the latter physician minuted down such appearances as presented themselves to their view, and collected the following observations.

The small intestines, the exterior coat of the stomach, and concave part of the left lobe of the liver, were all considerably inflamed. The gall bladder was very large and full of bile. The fat inclosed in the cellular membrane, surrounding the right kidney, was considerably wasted, and very much indurated; and appeared to adhere more firmly than usual to the external surface of the kidney. On removing this kidney with its fat, all the parts adjacent appeared much inflamed. The whole kidney was soft, and contained matter so disseminated through its whole substance, that it issued out on pressure from every part; in the same manner as an absorbed fluid does from the pores of a sponge. No stone or gravel were found in the pelvis, or any other part of the right kidney. The left kidney was nearly of the same pulpy substance with the right; equally contained matter, though not so large a quantity, and was equally free from stone and gravel. The vena cava, and the emulgent vein of the left kidney, were remarkably large. The aorta was ossified near its bifurcation into the crural arteries.

Two unnatural tumours grew on the left and lower side of the pelvis internally, near the junction of the os pubis with the ischium. They were contiguous to each other; in circumference severally something less than a walnut: and both taken together were 3 inches or more in length. When cut through, they had the appearance of glands, and one of them contained matter, disseminated through its substance in the same manner as the kidneys. On examining the bladder, the prostate gland was found enlarged and indurated, and the internal coat of the bladder itself inflamed. But neither the bladder, nor the ureters, contained any stone or gravel. No morbid appearance was observed in the liver, lungs, or any other of the parts, besides those above mentioned.

From the above observations it appears that this case was a general inflammation in most of the contents of the abdomen, and that the suppression of urine was probably a symptom in consequence of the swelling and induration of the prostate gland: which thereby closing the neck of the bladder, made the use of the catheter necessary. That the constitution was become extremely purulent. But as these collections of matter do not appear to have destroyed any of the vital functions, so it seems reasonable to believe that the immediate cause of his death was a general inflammation, and consequential sphacelus, in some of the abdominal contents.

Instances of abscesses formed in the kidneys, from the lodgment of calculi, are not unfrequent; but then the papillæ of the kidneys being irritated and inflamed by the stone, and in consequence the secretory tubes dissolved into matter, the secretion is thereby destroyed, and a suppression of urine always takes place in regard to that kidney. Two cases are indeed mentioned, the one by Eustachius,

3

the other in the Miscellanea Curiosa, where the kidneys in one of the subjects, are said to have been found putrid, in the other semiputrid, and no calculus in either. Such cases as these are very uncommon, and bear some resemblance to the case before us; in which it is very remarkable, that though matter was intimately distributed through every part of the kidneys, yet the tubuli forming the urinary organs of secretion remained sound, and properly qualified to perform their functions even till death; as appeared by the urine being drawn off every 12 hours, till near the time of the patient's decease, and the bladder being found distended with urine on opening the body. Whereas in the case recorded in the Miscellanea Curiosa, after a total suppression of urine, the bladder was found small and contracted, no urine having been excreted from the kidneys into it.

That the matter did not in the present case insinuate into, or in any manner disturb the urinary secretion is evident; since no pus was ever observed in the urine either before or after the introduction of the catheter. How this extraordinary case comes to be so particularly circumstanced seems worthy of consideration.

CIII. Experiments to Prove that Water is not Incompressible. By John Canton, M. A., F. R. S. p. 640.

Having procured a small glass tube of about 2 feet in length, with a ball at one end of it, of an inch and a quarter in diameter; Mr. C. filled the ball and part of the tube with mercury; and keeping it with a Fahrenheit's thermometer in water which was frequently stirred, it was brought exactly to the heat of 50 degrees; and the place where the mercury stood in the tube, which was about $6\frac{1}{2}$ inches above the ball, was carefully marked. He then raised the mercury by heat to the top of the tube, and sealed the tube hermetically; and when the mercury was brought to the same degree of heat as before, it stood in the tube $\frac{3}{100}$ of an inch higher than the mark. The same ball and part of the tube being filled with water exhausted of air, instead of the mercury; and the place where the water stood in the tube when it came to rest in the heat of 50 degrees, being marked, which was about 6 inches above the ball; the water was then raised by heat till it filled the tube; which being sealed again, and the water brought to the heat of 50 degrees as before, it stood in the tube $\frac{43}{100}$ of an inch above the mark.

Now the weight of the atmosphere, or about 73 pounds avoirdupois, pressing on the outside of the ball and not on the inside, will squeeze it into less compass.* And by this compression of the ball, the mercury and the water will be

* See an account of experiments made with glass balls by Mr. Hooke, (afterwards Dr. Hooke,) in Doctor Birch's History of the Royal Society, vol. 1, p. 127.

equally raised in the tube; but the water is found by the experiments above related, to rise $\frac{11}{100}$ of an inch more than the mercury; and therefore the water must expand so much more than the mercury by removing the weight of the atmosphere.

In order to determine how much the water was compressed by this, or a greater weight, he took a glass ball of about an inch and $\frac{6}{10}$ in diameter, which was joined to a cylindrical tube of 4 inches and $\frac{2}{10}$ in length, and in diameter about $\frac{1}{100}$ of an inch: and by weighing the quantity of mercury that exactly filled the ball, and also the quantity that filled the whole length of the tube; he found that the mercury in $\frac{23}{100}$ of an inch of the tube, was the 100000th part of that contained in the ball; and with the edge of a file he divided the tube accordingly.

This done, he filled the ball and part of the tube with water exhausted of air; and left the tube open, that the ball, whether in rarefied or condensed air, might always be equally pressed within and without, and therefore not altered in its dimensions. Now by placing this ball and tube under the receiver of an air-pump, he could see the degree of expansion of the water, answering to any degree of rarefaction of the air; and by putting it into a glass receiver of a condensing engine, he could see the degree of compression of water, answering to any degree of condensation of the air. But great care must be taken in making these experiments, that the heat of the glass ball be not altered, either by the coming on of moisture, or its going off by evaporation; which may easily be prevented by keeping the ball under water, or by using oil only, in working the pump and condenser.

In this manner he found by repeated trials, when the heat of the air was about 50 degrees, and the mercury at a mean height in the barometer, that the water will expand and rise in the tube, by removing the weight of the atmosphere, 4 divisions and $\frac{6}{10}$, or one part in 21740; and will be as much compressed under the weight of an additional atmosphere. Therefore the compression of water by twice the weight of the atmosphere, is one part in 10870 of its whole bulk.*

The famous Florentine experiment which so many Philosophical writers have mentioned as a proof of the incompressibility of water, will not, when carefully

* If the compressibility of the water was owing to any air that it might still be supposed to contain, it is evident that more air must make it more compressible; he therefore let into the ball a bubble of air that measured near $\frac{6}{10}$ of an inch in diameter, which the water absorbed in about 4 days; but he found on trial, that the water was not more compressed by twice the weight of the atmosphere, than before. The compression of the glass in this experiment, by the equal and contrary forces acting within and without the ball, is not sensible: for the compression of water in two balls, appears to be exactly the same, when the glass of one is more than twice the thickness of the glass of the other. And the weight of an atmosphere, which he found would compress mercury in one of these balls but $\frac{1}{3}$ part of a division of the tube, compresses water in the same ball 4 divisions and $\frac{6}{10}$.—Orig.

considered, appear sufficient for that purpose: for in forcing any part of the water contained in a hollow globe of gold through its pores by pressure, the figure of the gold must be altered; and consequently the internal space containing the water diminished; but it was impossible for the gentlemen of the academy *del Cimento* to determine, that the water which was forced into the pores and through the gold, was exactly equal to the diminution of the internal space by the pressure.

CIV. On the Solar Eclipse, October 16, 1762. *By Mr. Samuel Dunn.* p. 644.

Mr. D. observed the solar spots instead of the sun's eclipse.

CV. Remarks on the Catarrhal Disorder, which was very frequent at London and in its Neighbourhood in May 1762; *and on the Dysentery, which prevailed the following Autumn.* By W. Watson, M.D., F.R.S.* p. 646.

In the beginning of May, 1762, there was at London and in its neighbourhood a disease, very epidemic, though not fatal, which had sometime before been very prevalent both in Italy and Germany. It continued during the course of the month, and some part of June. In it the breast was very much affected, and it was very frequently attended with a fever. It is nearly the same disease which was at London in April and May 1743, and then called influenza, the name applied to it in Italy. Dr. W. remarks that it is very well described by Dr. Huxham in the 2d vol. of his work, entitled, De Aere et Morbis Epidemicis, p. 101. Tho' of the same catarrhal kind, it was by no means so severe or so fatal as the disease of Feb. 1733, of which there is likewise a history in the 1st vol. of the above mentioned work, p. 80. The disorder, though very general, seemed to attack the women more severely than the men. Much bleeding did harm; and where there was no fever, which was frequently the case, the patients recovered equally well without it. Even without bleeding, or other evacuations, some, more especially women and lax-fibred men, were much debilitated during its whole continuance. The blood in most was not sizy; but the crassamentum was tender and the serum bilious. Where the heat was great, gentle emetics brought up much bile, and very much lessened the inflammatory state of the disease. The rest was to be left to blisters, if the cough was very troublesome and the stricture on the breast severe, balsamic medicines, gentle opiates, and light broths; carefully avoiding cordials of every denomination and volatiles. Towards the end of the disorder, after gentle evacuations by stool, decoctions of Cort. Peruv. were of signal service, both in recruiting the strength, and carrying off the remaining cough.

In the disorder of 1743, the skin was very frequently inflamed, when the fever

* Of this catarrhal disorder (influenza) and dysentery a more complete account was afterwards published, in an elegant Latin dissertation, by Sir G. Baker.

ran high; and it afterwards peeled off in most parts of the body: but this was not observed to happen in the present disorder.

This autumn (1762) there was a disease, which had not been in Dr. W.'s remembrance epidemic at London. Very few of the physicians in London had seen this disorder as it had appeared then; but Dr. Huxham mentions it as frequent at Plymouth in the year 1743, in his treatise De Morbis Epidemicis, vol 1, p. 90, where it is observed that many of the children which fell under his care voided the vermes teretes. In the course of his practice Dr. W. found many of Dr. H.'s observations exceedingly well founded, and collected from them very useful remarks. Dr. Sydenham has left an admirable history of this disease, as it appeared at London in the year 1669, and the 3 subsequent years. To this work, as well as to what Dr. Huxham has given on this subject, Dr. W. was very much obliged.

As the dysentery is most frequently an autumnal disease, and as Dr. W. had not seen any person afflicted with it in the latter part of Dec. he flattered himself that the cold and frosty weather had put a stop to its progress. This disorder, though very general, most frequently attacked weak persons, and those recovering from other diseases, women during their lying-in, and children. The dysentery in some was attended with a fever, in a high degree inflammatory; in others it was without any fever. When it was attended with a fever, bleeding and gentle evacuations by stool with liberal dilution did great service. When there was no fever, as well as in those whose fever had been relieved by the methods before mentioned, if the irritating pain in the bowels, bloody or mucous discharges with the tenesmus continued, after the excrementitious sordes had been carried off, nothing relieved more than drinking large quantities of very small mutton broth, without salt, so as to be discharged but little altered. This not only warmed and nourished the patient, but diluted the acrimony, and served as a most comfortable fomentation to the whole intestinal canal. Clysters of this with Tinct. Theb. he directed to be given 3, or even if the symptoms were urgent, 4 times a day. When these symptoms were abated, as most persons were exceedingly debilitated and their appetite almost gone, light decoctions of Cort. Peruv. greatly hastened the recovery.

He had the misfortune to see 3 children die of 4 or 5 years old, after the severity of the disease was over. Their bowels had for a week or more been free from pain. They were without fever. Their discharges by stool both bloody and mucous were in a manner gone: yet they were so much debilitated, and their stomachs so languid, that they obstinately refused every species of nourishment by the mouth; nor would they retain nutritious clysters; so that in the end they sunk from absolute inanition. In 2 of these, which by his direction were opened, he found their gall bladders turgid with high coloured viscid bile.

In both, the stomach and bowels were perfectly empty, and their bodies emaciated to a great degree. In one, neither the stomach nor bowels were in the least degree inflamed or discoloured; except that a very few of the veins were preternaturally enlarged on the surface of the cæcum and colon. In the other, there had been an inflammation on about 10 inches of the jejunum; but that had been resolved; as the bowel was almost restored to its natural colour, and was not in its texture, even after death, more tender than the rest. The other viscera had not the least change of their colour, but exhibited a sound and natural appearance. Another child which he saw, was seized with a dysentery, attended with a very ardent fever, which, notwithstanding his utmost endeavours to relieve it, carried off the poor infant on the third day. Several, almost the whole, of this child's discharges by stool were nothing but blood. On opening the body after death, the whole of the intestines were in a very great degree inflamed, and of an intensely deep red colour, and the contents of the abdomen were inexpressibly fetid. Throughout the whole course of the disease, keeping the patient moderately warm, and promoting his perspiration, was of great importance; and the not sufficiently attending to this, he more than once saw followed by fatal effects.

CVI. Observation of some Solar aad Lunar Eclipses at Leyden. By Mr. John Lulofs. From the Latin. p. 650.

Of the lunar Eclipse May 8, 1762.

True time.

At $2^h\ 32^m\ 46^s$ A dense penumbra on the disk.

2 36 41 The true shadow touched Kepler.

Of the solar Eclipse, Oct. 17, 1762.

True time.

At $7^h\ 17^m\ 50^s$ The eclipse had begun, digit $\frac{3}{4}$.

8 3 32 The greatest obscuration, 5.58 dig.

9 3 12 End of the eclipse.

The lunar Eclipse, Nov. 1, 1762.

True time.

At $7^h\ 30^m\ 26^s$ A dense penumbra.

7 40 30 The true shadow at Grimaldi.

8 31 49 Digits observed were $6^\circ\ 27'\ 39''$.

CVII. An Account of the Gardenia. By Daniel C. Solander. M. D.* p. 654.

The Gardenia is at present well known among the English gardeners by the

* Charles Daniel Solander, M. D. celebrated for his knowledge in natural history, was the son of a Swedish clergyman, and was born in the year 1736 in the province of Norland. In 1760 he travelled

name of the Cape Jasmine, though it has been but a few years in this country. It was first brought here, 1744, from the Cape of Good Hope by Capt. Hutchenson, in the Godolphin Indiaman, and by him presented to Richard Warner, Esq., of Woodford Row, Essex; in whose garden it long remained without the least sign of vegetation; but at last proved to be the most beautiful shrub that has been introduced among us for a long time. And indeed the botanic world is as much indebted to the abovenamed gentleman, for his skill and care in the preservation of the plant, as for his generosity in communicating it to the public.

When this plant first appeared, it was thought a new and unknown one to the European botanists; and though it came to blossom freely, the flowers unfortunately proved double. For notwithstanding the fructification is the only material thing in plants, whence they can be sufficiently known and described, yet double flowers are really a kind of monsters in the vegetable kingdom, as their principal parts are too much altered and distorted, for any thing to be determined from them with certainty. It therefore still remained a difficulty to ascertain what tribe this shrub belonged to; and the only way of forming any judgment, was by considering all its parts accurately, comparing them with other known plants, and thus by analogy finding out its affinity, and thence its proper place in the vegetable

into England, and was recommended by Linnæus, whose pupil he had been, to Mr. Peter Collinson, and other botanists, as well as to George Edwards, the well-known ornithologist. By Mr. Collinson he is said to have been first recommended to the trustees of the British Museum, where he was some time afterwards appointed assistant librarian in the department of natural history. When Dr. Solander first arrived in England, his principal attention seems to have been turned to botany. Linnæus therefore particularly recommended him to the notice of Edwards in order that he might improve his knowledge in ornithology. The following is an extract from Linnæus's letter to Edwards on this subject.

"Has tibi, vir nobilissime, traditurus literas D. Dan. Solander meus totus est: hic in animum induxit Angliam adire, ut cognitione proficiat apud nobilissimos Anglos, apud quos hæc scientia hodie unice floret. Imprimis vero tua authoritas eum allicuit, qui summum suum habet oblectamentum in animalium historia. Est imbutus varia cognitione zoologica, sed, ut verum fatear, minus in ornithologicis versatus quam in reliquis partibus: te itaque præceptorem habere avidissimus est." In 1768 Dr. Solander accompanied Mr. (now Sir Joseph) Banks, his warm and liberal benefactor, in the voyage made round the globe by the celebrated Capt. Cooke, and during the various opportunities afforded, was indefatigable in collecting plants and other objects of natural history. In the year 1773 he succeeded, on the death of Dr. Maty, to the office of under librarian at the British Museum, in which situation he continued till his death, which took place in May, 1782, in consequence of an apoplectic stroke. As to the person of Dr. Solander, we are informed by the authors of the Biographical Dictionary, in 15 vols. 8vo, that "he was a short, fair man, rather fat; with small eyes, and good humoured expression of countenance." He was considered as a person of very extensive knowledge, to which was added a mode of communication not only remarkable for its readiness, but for its peculiar modesty. There are said to be some papers by Dr. Solander scattered in the memoirs of various philosophical societies; but in the Transactions of the Royal Society of London the present paper, relative to the Gardenia, seems to have been his only production.

system. At first it was thought to be a species of jasmine,* probably from some distant likeness and the fragrancy of its flowers; but as it hardly agreed in any other particular, it was afterwards doubted if it could properly be referred to that tribe,† and at last John Ellis, Esq., F.R.S. declared this plant to be a distinct genus, and gave it the name of gardenia.‡ This gentleman concluded that the plant then in question, must be very different from a jasmine, as well from the unlikeness in its leaves and stipulas, as principally from the seed-vessel being placed below the receptacle of the flower; but not choosing to advance this on his own authority, he sent an account of it with dried specimens to Dr. Linnæus at Upsal, whose known extensive skill in every part of natural history, has rendered his opinion among all the professors of that science to be of the best authority. The Dr. answered that the situation of the seed-vessel, and the peculiarity of the calyx, were sufficient to persuade him of its being a new genus; but as the stamina must be uncertain in double flowers, he could not then undertake to determine its characters. However soon afterwards Dr. Linnæus wrote word that he had found a single flower of this same plant, among some specimens from the East Indies, and no longer scrupled to agree to Mr. Ellis's determination of the gardenia. There wanted nothing then but an account of the fruit; and especially the number of seeds; and Mr. Ellis, who was well acquainted with observations on the most minute parts of nature, soon discovered that the seed-vessel contained rudiments of many seeds; though it seems the veracity of this particular has been much questioned; which doubtless has arisen from the imperfect state that all fruits and seeds commonly appear in, after double flowers, as in the present case. But by good fortune, at Mr. Carteret Webb's at Bushbridge, Dr. S. discovered a specimen of this shrub in perfect fruit, gathered by Mr. Cunningham, in the East Indies, where that gentleman travelled for discovery of natural curiosities. On dissecting the fruit for examination, he found that the generical characters of the gardenia given by Mr. Ellis in the Phil. Trans., vol 51, p. 929, were very complete. Dr. S. adds a few particulars, that could not be seen in an imperfect or immature fruit.

The seed-vessel when ripe, is egg-shaped, outwardly ribbed from the descending wings of the flower-cup, and within divided into two cells by a thin membranaceous partition. The seeds are many, at least more than 50 in each cell, compressed and surrounded with a mucilaginous substance. The mucilage here mentioned was so little hardened in the fruit he examined, that the seeds themselves were quite soft and inclined to be moist. Recollecting that Dr. Plukenet had figured many of Mr. Cunningham's plants, Dr. S. had recourse to his Gazo-

* Miller Dict. and fig. † Ehret fig. ‡ Phil. Trans. 1760, p. 929.

phylacium, and there found an engraving of this plant, pl. 448, n. 4, and that it was twice mentioned in his Amaltheum, pages 29, 212.

From the observations which Dr. Plukenet,* Dr. Petiver, and Mr. Ray† had received from Mr. Cunningham, Dr. S. learned that the Chinese use the seeds of gardenia jasminoides as a scarlet dye; and as the mucilaginous substance in which the seeds are involved, seems to be very copious and rich of colour, he imagines must be worth inquiry, whether this shrub may not be found, and transported to such of the British colonies where it might be propagated; and perhaps become one of the most useful plants, as it is now one of the most beautiful. He tried these seeds in water, spirits, and other liquors, and always found them tinge the menstruum yellow, notwithstanding they had been gathered near 80 years.

To confirm himself in the discovery he had made, he obtained leave to look over the collections of dried plants in the British Museum, where many are preserved, that can no where else be met with, and by the assistance of the assiduous Mr. Empson, he found several good specimens of this valuable shrub, viz. in Hort. Sicc. XX, p. 25, 86; XCIV, p. 130; CCXLVII, p. 25; CCLXXXIX, p. 33; and CCCXXXI, p. 90; all gathered in the East Indies by Mr. Cunningham. The greater part of these specimens were in fruit, but one or two with perfect blossoms, and they were so exactly corresponding with Mr. Ellis's account, that he could find nothing to alter or add to it.

There is however one thing he would not omit mentioning, as it may in some measure account for the unequal number of the divisions in the double blossoms; it is that some of the specimens at the British Museum have their calyx divided into 5, and others into 6 sigments or wings, which show that the inequality is not altogether peculiar to the double flowers; and Dr. S. had drawings made from the best samples he could find in the collection, the better to explain what he has said; viz. pl. 15, fig. A, shows a specimen with a single blossom; fig. B, another with the fruit, both gathered in China by Mr. Cunningham; fig. C, a capsul with only 5 divisions in the calyx (which Dr. S. supposes to be the natural number) taken from another dried specimen of the same gentleman's; fig. D, a transverse section of the same capsule, to show the two cells, with many seeds in them; and fig. E represents the seeds of their natural size.

It may not be amiss here to insert what is said relative to the names of this

* Semina tinctoribus inserviunt, iis enim ab indigenis Sinensibus optime tingitur nobilis ille color, quem escarlatinum nostrates vocant, ut nos monuit vir multiplicis industriæ, atque indefessi laboris hac in parte D. Jacobus Cunninghamus. Plukn. Amalth. p. 29.—Orig.

† Hujus fructus celebris est, et in frequenti usu apud Chinenses ad colorem coccineum, seu scarlatinum tingendum. Ray Hist. III. p. 233.—Orig.

shrub, by such botanical writers as he had an opportunity to consult. The first author that gives an account of this plant, is Dr. Plukenet, after him Mr. Petiver, and Mr. Ray; but none of them have given a true botanical name or description, much less referred it to its proper class, order, or genus; and notwithstanding so many good specimens were preserved in the botanical collections of Sir Hans Sloane, now in the British Museum, it was not further noticed till the ingenious Mr. Miller, of Chelsea, gave the description and drawing in his Gardener's Dictionary and Figures of Plants, from the plant he saw at Mr. Warner's garden. Mr. Ehret soon afterwards published a most elegant figure of it, and Mr. Ellis at last completed the botanical description, in the Phil. Trans.

Those gentlemen have mentioned this shrub, under the following names: Arbuscula Sinensis, myrti majoris folio, vasculo seminali hexagono, ad singulos angulos alis foliaceis munito, quæ porrectæ vasculi coronam efformant, Umki Sinensibus dicta. Plukn. Amalth. p. 29.

Umki, alias Umuy; cujus fructum ad colorem escarlatinum tingendum inservit; florem fert rosaceum, album, hexapetalum. Plukn. Amalth. p. 212. tab. 448. f. 4.

Frutex cynosbati fructu alato, tinctorio, barbulis longioribus coronato. Petiv. Mus. p. 498. Ray. Hist. III. p. 233.

Jasminum foliis lanceolatis oppositis integerrimis, calycibus acutioribus. Mill. Dict. n. 7. Mill. fig. 180.

Jasminum? ramo unifloro pleno, petalis coriaceis. Ehret. fig.

Gardenia jasminoides. Ellis, Philos. Trans. 1760, p. 929, tab. 23.

One circumstance still remains to be inquired into, namely, the native place of this shrub. That it grows spontaneously in China and the neighbouring countries, he does not in the least doubt, because Dr. Linnæus has had his specimen from thence; and Mr. Cunningham tells us, in his time, it was found there in such plenty, that they could collect and use its seeds for dying. Neither does he doubt that Capt. Hutchenson procured the plant he brought over from the Cape of Good Hope, especially as there are specimens of it with double blossoms among the curious plants that were brought over from that place to Mr. Desmarets, now in the British Museum, Hort. Sicc. CCLXI, p. 30. But as those have double flowers, and having never heard of any with single blossoms being gathered in that country, Dr. S. can scarcely believe it is an indigenous plant there, but rather imagines that it must have been brought thither from the East Indies, either by accident or for their gardens.

Dr. S. once more repeats, that as this plant may probably be of real benefit by improving the art of dying, he would beg leave therefore to recommend it to all public-spirited gentlemen, to use their best endeavours, for discovering and bringing over from the East Indies some single blossomed plants, or the seeds,

of the Gardenia; and afterwards to consider of the properest place for planting and cultivating them for so valuable an end.

CVII. Of the Male and Female Cochineal Insects, that breed on the Cactus Opuntia, or Indian Fig, in South Carolina and Georgia. By John Ellis, Esq.* p. 661.

Mr. E. hearing that this insect bred in great abundance on the Cactus Opuntia of Linnæus's Species Plantarum, p. 468, in South Carolina and Georgia, where it is a native and grows in great plenty, as well as on the Cactus Coccinellifer of the same author, which grows in Mexico, and has been for many years introduced into Jamaica, he wrote to Dr. Alexander Garden, of Charles Town, South Carolina, to send him some of the joints of the Cactus Opuntia, with the insects on it; which he did the latter end of the year 1757. These specimens were full of the nests of this insect, in which it appeared in its various states, from the most minute, when it walks about, to the state when it becomes fixed, and wrapt up in a fine web, which it spins about itself.

The female (which was here alive and in plenty) is well described by Mons. Reaumur, Dr. Brown of Jamaica, and by Dr. Linnæus, in his System of the animal kingdom, under the title of Coccus Cacti Coccinelliferi, p. 457, N° 17, from a living insect sent him from Surinam, by Mr. Rolander, in the year 1756; but neither Reaumur, Brown, nor Linnæus had ever seen the male. As this genus of insects is placed by Dr. Linnæus under the hemipeteræ or half winged, it may be necessary to know that he comprehends in this class not only those whose wings are half covered with a crustaceous case, but such also as have wings only on one sex.

In order to find out the male fly, Mr. E. examined all the webs in these specimens, besides a large parcel which the Dr. had sent picked off from the plants in Carolina; and at last discovered 3 or 4 minute dead flies with white wings; these he moistened in weak spirit of wine, and examining them in the microscope, he discovered their bodies to be of a bright red colour, which convinced him of their being the true male Cochineal insect: to be confirmed in his opinion, he immediately communicated his discovery to Dr. Garden, which he accompanied with an exact microscopical drawing, and desired he would send some account of their œconomy, with some male insects of his own collecting, which he was so kind to do in the spring, with some observations on them, which are as follows.

"In August 1759 I catched a male Cochineal fly, and examined it in your aquatic microscope. It is seldom a male fly is met with. I imagine there may be 150 or 200 females for one male. The male is a very active creature and well

* It is to be observed that the species here described by Mr. Ellis, is not the true or genuine cochineal cultivated in Mexico, but is very strongly allied to it, though of an inferior kind as to use.

made, but slender in comparison of the females, which are much larger and more shapeless, and seemingly lazy, torpid and inactive. They appear generally so overgrown, that their eyes and mouth are quite sunk in their rugæ or wrinkles, nay their antennæ and legs are almost covered by them, and are so impeded in their motions from these swellings about the insertions of their legs, that they scarcely can move them, much less move themselves. The male's head is very distinct from the neck, the neck being much smaller than the head, and much more so than the body. The thorax is elliptical, and something longer than the head and neck together, and flattish underneath: from the front there arise two long antennæ (much longer than the antennæ of the females) which the insect moves every way very briskly. These antennæ are all jointed, and from every joint there come out 4 short setæ placed 2 on each side. It has 3 jointed legs on each side, and moves very briskly and with great speed. From the extremity of the tail, there arise 2 long setæ or hairs, 4 or 5 times the length of the insect. They diverge as they lengthen, are very slender, and of a pure snow white colour. It has 2 wings, which take their rise from the back part of the shoulders or thorax, and lie down horizontally like the wings of the common fly, when the insect is walking: they are oblong, rounded at the extremity, and become suddenly small near the point of insertion: they are much longer than the body, and have 2 long nerves, one running from the basis of the wing along the external margin, and arches to meet a slender one that runs along the under and inner edge; they are quite thin, slender, transparent, and of a snowy whiteness. The body of the male is of a lighter red than the body of the female, and not near so large."

To this description of Dr. Garden's, which agrees very nearly with the microscopical drawings of both sexes of this insect, A and C, pl. 16, it may be added that the female has a remarkable proboscis or awl-shaped papilla, that arises in the midst of the breast. This Linnæus calls the rostrum, and thinks it the mouth: if so, besides the office of supplying it with nourishment during the time of its moving about, it is the tube through which the fine double filament proceeds, with which it forms its delicate white web, in order to accommodate itself in its torpid state, during its pregnancy; till the young ones creep out of its body to shift for themselves, and form a new generation. In this torpid state the legs and antennæ grow no more, but the animal swells up to an enormous size in proportion to its first minute creeping state. The legs, antennæ, and proboscis, are so small with respect to the rest of the body, that they cannot be easily discovered without very good eyes or magnifying glasses; so that to an indifferent eye, it looks full as like a berry as an animal.

This was the occasion of that contest mentioned by Pomet and other authors, which subsisted so many years, whether it was an animal or a vegetable production. But if persons of curiosity will give themselves the trouble to soak a

few grains of the common cochineal of the shops in warm water for 24 hours, they will observe them to swell up to their original shape; so that the legs, antennæ, and proboscis may be discovered. What is remarkable in the proboscis is, that we shall find in many of them the ends of two fine hairs or filaments remaining, with which it forms its web, not unlike the silk worm; which always spins its cocoons with 2 threads, which as they come out, unite together with the natural gluten of the animal. And if this animal, thus expanded by moisture, be opened in a watch glass with a fine lancet in a little water, a great number of eggs, with the young animals in them, may be discovered, which will exhibit a very agreeable scene of a most vivid crimson hue.

As soon as the female insect is delivered of its numerous progeny, it becomes a mere husk and dies; so that great care is taken in Mexico, where it is principally collected, to kill the old ones while big with young, to prevent the young ones escaping into life, and depriving them of that beautiful scarlet die so much esteemed by all the world.

In plate 16 are exhibited several views of both the male and female insect in various states and positions, and also both in their natural size, and as magnified by the microscope; the former marked with the small letters of the alphabet, and the latter with the corresponding capitals. Thus,

A, denotes the female cochineal insect on its back magnified. B, the same on its belly magnified. C, the male cochineal insect as it walks, magnified. D, the male cochineal insect with its wings extended, magnified. E, the male cochineal insect in a side view flying, magnified. F, the male insect as it is found without wings, magnified. G, the silk-bag which the male insect spins before its wings are expanded. H, the silk-bag cut open which discovers the head of the male insect magnified. I, the appearance of the female when it first begins to spin, magnified. K, L, M, the front, back, and side views of the female cochineal insect, when it comes to perfection, and big with young, magnified.

a and b, the natural size of the female cochineal insect when it creeps about. c, d, e, the natural size of the male cochineal fly in three different views. f, the male insect as it is found without wings. g, the silk-bag of the male fly. h, the top of the silk-bag cut open to show the head of the male fly. i, the female before it spins. k, l, m, the natural size of the female cochineal when it becomes fit for use, in three views.

END OF THE FIFTY-SECOND VOLUME OF THE ORIGINAL.

I. Of the Sun's Distance from the Earth deduced from Mr. Short's Observations relating to the Horizontal Parallax of the Sun. By Peter Daval, Esq., V.P. of R.S. to James Barrow, V.P. of R.S. p. 1. *Vol. LIII.*

According to Mr. Short, the mean horizontal parallax of the sun is 8″.65. Now this parallax is the angle which the semidiameter of the earth subtends, as seen from the sun. Therefore as 8″.65 is to 360° (the whole periphery of a circle) so is the semidiameter of the earth to the periphery of the orbit of the earth round the sun. But 8″.65 is very nearly the 149826th part of 360°, as may be easily proved by division. According to the latest observations, the mean semidiameter of the earth is 3958 English miles, which being multiplied by 149,826, produces 593,011,308 miles for the circumference of the orbit of the earth. The distance of the earth from the sun is the semidiameter of this orbit: and the periphery of the circle is to its semidiameter very nearly as 6,283,185 to 1. Therefore if we divide 593,011,308 by 6,283,185, the quotient, which is very nearly 94,380,685, will give the mean distance of the earth from the sun in English miles.

As the orbit of the earth is an ellipsis, not a circle, the distance of the earth from the sun will be greater in its aphelion, and less in its perihelion than here assigned.

II. Observation of a Comet, which appeared in May 1759, made at the Hague. By Peter Gabry, I.V.D., F.R.S. p. 3.

Mr. Gabry observed the place of the comet on several days, as below.

May 2d	9^h, its longitude ♍	19°	12′	24″, latitude	28°	40′	5″ s.
3	$9\frac{1}{2}$	17	11	40	27	20	20
6	$9\frac{1}{2}$	12	51	7	22	37	24
7	$9\frac{1}{2}$	11	59	14	21	1	44

III. Observation of a Fiery Meteor, made at the Hague, Dec. 21, 1758. By M. Peter Gabry, F.R.S. From the Latin. p. 5.

About 8 in the evening of that day a remarkable light, or shining meteor, appeared in the western part of the heavens, though the sky was then hazy and calm. The air was temperate, the thermometer indicating 47°. Sometimes the western sky seemed to burn, and the air with the lower clouds as if changed into flames and smoke. At the same time also there shot up bright flames almost to the zenith.

A little after, the shining appeared very white, like a continued mass of fire; at first thin and weak; then increasing, it would extend from the north to beyond the west, not unlike the light appearing in the horizon, just before sun-rise.

Then it would assume a red colour, so as it seemed to be a fire broke out in the vicinity of the city; and at times ascending higher above the horizon, and illuminating the surrounding houses. This meteor disappeared at half past 8 o'clock.

IV. On a Remarkable Decrease of the River Eden, in Cumberland. By Wm. Milbourne, Esq. p. 7.

In the night between the 28th and 29th of December, 1762, the river Eden, at Armathwaite, fell at least 2 feet perpendicular. The decrease of the water was so sudden, that several trouts and young lampreys had not time to save themselves, but were found the next morning frozen to death. Of the former, eye-witnesses can speak to 15, of the latter 200, all found in the extent of no more than 40 yards. And several dozens of young lampreys were easily taken up alive by the hand in the shallows. The suddenness of the water's decrease may be so far ascertained as follows. The miller of Armathwaite-mill left off grinding at 12 o'clock that night, there not being then sufficient water to work the mill. He went to the mill the next morning at 6, and there was not then water enough to turn the wheel round. It has not been known, that the river Eden was ever so low at this place, by a foot, in the driest summer. The water continued in this state till about 11 o'clock of the morning of the 29th, and then gradually increased (no rain or snow falling) till about 1 in the afternoon, by which time it had risen about a foot perpendicular.

V. Of the Rain fallen in a Foot-square at Norwich. By Mr. Wm. Arderon, F. R. S. p. 9.

	1749.	1750.	1751.	1752.	1753.	1754.	1755.	1756.	1757.	1758.	1759.	1760.	1761.	1762.
	wine pints.	wine pints.	wine pints.	wine pints.	wine pints.	wine pints.	wine pints.	wine pints.	wine pints.	wine pints.	wine pints.	wine pints.	wine pints.	wine pints.
Jan.		5.2	10.4	8.6	8.	9	5.5	9.9	14.4	6.9	7.4	5.5	4.3	12.8
Feb.		8.5	4.6	9.8	14.6	5.6	4	4.7	4.4	13.6	2	18.7	10	12.9
Mar.....		6.2	14.6	8.7	3.5	10	9.8	12.4	13.9	6.7	11.1	5.8	2.5	5.1
April....		8.9	11.7	8.4	8.7	6.4	10.5	22.6	13.7	3.9	8.8	1.5	4	2.2
May		4.3	10.3	11.9	5.3	8.2	5.5	6.6	8.2	1.3	4.7	7.8	12.2	4.3
June....		8.4	6.2	12.2	4.7	9.6	5	15.7	1.5	9.3	14.4	8.5	11.2	4.5
July		9.7	25.8	19.5	11.6	11.3	18.7	9.2	16.3	26.5	8.3	9.2	6.5	16.6
Aug.....		4.7	9 4	12.2	16.8	17.4	17.9	22.4	30.4	17.7	12.5	17.9	12.8	27.3
Sept.....		4.2	15.2	4.3	3.2	0.7	12.9	9.3	7	21.2	14.2	16.9	7	10.2
Oct.		16.2	9.3	2.4	21.2	6.7	9.6	5.3	12	2.5	6.8	20.5	23.4	39.6
Nov.....	4.2	13.8	16.6	9.5	18.	7.7	21	8.7	8.2	5.8	13.7	13.1	10.9	5.2
Dec.....	9.1	10.6	12.5	17.1	18.5	12.9	7.6	5.4	12	12.5	9.2	8.5	11.3	3
		100.7	146.6	124.6	134.1	105.6	128	132	142	127.9	113.1	133.9	116.1	143.7
Total inches deep.		20.18	29.38	24.9	26.8	21.1	25.6	25.4	28.5	25.5	22.7	26.8	23.3	28.8

VI. Effects of Electricity applied to a Tetanus, or Muscular Rigidity, of four Months' Continuance. By Wm. Watson, M.D., F. R. S., &c. p. 10.

Catherine Field, a girl in the Foundling Hospital, aged about 7 years, and otherwise a healthy child, having been disordered a few days with what were considered as complaints arising from worms, was observed, on Thursday, July 8, 1762, to open her mouth with great difficulty. This particular circumstance increased so much, that by the Sunday following, when Dr. W. first saw her, her teeth were so much confined, it was with difficulty that even liquids could be admitted into her mouth. She had 2 days before parted with 2 worms, and had several very offensive stools. Her breath was now, and had been for some days, very fetid. Though her jaw was locked very close, she was without pain; even in the temporal and masseter muscles, whose office is to bring the under jaw to the upper; and which, in this instance, were tense, hard, and spasmodically affected. She was feverish, her pulse was quick, and her flesh hot; and she had had but very little sleep.

On Monday, July 12, he visited this poor girl in consultation with Dr. Morton. They found she had had a restless night; her fever was high, and it was infinitely difficult to introduce any thing between her teeth. As there had been no wound, no eruption repelled, they were of opinion, from her offensive breath, and other indications, that the spasm of her jaw was symptomatic, either of worms or foul bowels. Whatever was admitted into her mouth, was swallowed without difficulty; neither in this state of the disease was her breathing at all affected. For near 3 weeks the disorder confined itself to the jaw, during which time she was constantly feverish. At times indeed her fever ran very high, and her pulse beat 130 strokes in a minute. At other times it beat only about 100; but never for these 3 weeks was it slower than that number.

Notwithstanding their best endeavours, the disease not only continued, but the rigidity communicated to the muscles of her neck, so that she could not move her head in the least: and from pains shooting down her back, they had reason to apprehend, and which indeed did soon after happen, that the muscles of her back would soon likewise be rigid. After the back was affected, the disease extended very fast; so that by the end of Sept. almost all the muscles of her body were rigid and motionless. To be somewhat more particular; the rigidity from the temporal and masseter muscles had extended to the cheeks, to the neck, breast, abdominal muscles, all those of the back, the right arm, the hips, thighs, legs, and feet. Nor were they by any force, that could be exerted with safety, to be extended. By the rigidity and contraction of the large and long muscles of the back, the os sacrum and hips were drawn towards the shoulders; so that the spine formed a very considerable arch. By the superior strength of the flexor muscles of the thighs to that of the extensors, the legs were drawn up

almost to the thighs. Of all her limbs, the left arm only preserved any motion. Of this the joint of the shoulder was rigid, that of the elbow extremely impaired; but the wrist, hand, and fingers, were reasonably pliant. The various muscles subservient to the motions of the eyes, eyelids, lips, and tongue; as well as those internal ones at least, which assist in performing the offices of respiration and deglutition, did not seem in the least to partake of the rigidity.

From the end of Sept. to the middle of Nov. the disease, though it had exerted all its power, was at a stand. The feverish heat had left her, and her pulse beat generally between 80 and 90 strokes in a minute. But during this interval the poor patient was seized many times, both in the night and in the day, with violent convulsions in those muscles of the eyes, face, and right arm, which had any mobility left. These were so severe, that in her weak and wretched state, her attendants imagined every attack would put an end to her distresses. In this state, partly from the severity of the disease, and partly from the very small quantity of food which could be given to her, and which was only through a small opening made by extracting 2 of her teeth, and without which she must inevitably have been starved, she was emaciated in a most extraordinary manner. Her belly was contracted, and drawn inwards towards the spine. Her whole body to the touch felt hard and dry, and much more like that of a dead animal than a living one. This, added to the very great distortion of her back and lower limbs, heightened the disagreeable spectacle, and called to mind that admirable passage of Aretæus (cap. vi.), who when treating of and contemplating this disease, calls it 'inhumana calamitas, injucundus aspectus, triste intuenti spectaculum, et malum insanabile.' And he subjoins, that 'their distortions are such, that they cannot be known by their most intimate friends; which in this case was most strictly true.

During the continuance of this disorder, which had lasted more than 4 months, nothing had been omitted that they could suggest for her relief. While worms or foul bowels could be suspected to have occasioned this illness, as her stools were at first very offensive, and she had voided 2 worms, vermifuges of the most celebrated kind, linseed oil both by the mouth and by clysters, and such other medicines as tend both to carry off or destroy the worms, and cleanse the bowels, were assiduously administered. But no relief arising from these, bleeding with leeches at the temples, when her fever ran high, blisters behind the ears, round the neck, on the head, and in various parts of her body, were from time to time applied, as the disorder seemed to indicate. Nor during this time were antispasmodic remedies of various kinds omitted, and that in very liberal doses. Among these, as in several cases of locked jaws, related by authors of undoubted credit, opiates had been found to have been attended with great success, tinctura thebaica was copiously given. So that between the 12th of July

and the end of the month, more than 900 drops of that tincture were taken: a large quantity for so young a person! This they sometimes thought had a good effect, as the jaw was at times somewhat loosened; but this advantage was temporary, and the stricture soon returned as severe as before. Though this medicine, given in large doses, did not affect her head, but only gave her quiet nights, yet it was occasionally obliged to be suspended; as her pulse was at times much sunk, and her sweats cold and clammy. Volatile liniments were liberally used to the rigid parts, and warm bathing was continued for many weeks, with much friction, while in the warm water. After warm bathing had been so long tried without sensibly good effect, cold bathing, recommended by Hippocrates (Περὶ νȣσων, lib. iii.) for the cure of this disease, was directed; and she was dipped several times, without being apparently the better or worse for it.

From the end of Sept., as what had been done hitherto had not been able to prevent the rigidity extending itself, they desisted from attempting to relieve her by medicine, and determined to nourish and support her; and wait to observe, though it was scarcely to be expected, whether nature unassisted would point out any crisis for her relief. This attention was continued to the middle of Nov. without any other alteration than that her convulsions increased in their force; and every day, by those who were about her, was expected to be the last; and which was an event, as the prospect was so unpromising, much to be wished for. Dreadful however as her situation was, she was still alive: they were desirous therefore of omitting nothing that in the least might be expected to relieve her.

Dr. W. had heretofore many times observed, that in paralytic limbs, the muscles of which had for a considerable time ceased to be subservient to the will of the patient, he had been able, by means of electricity, to make any muscle he thought proper contract itself, and act as a muscle, without the patient's being able to controul it. He had seen in one instance the good effects of electricity, in restoring to the hands and arms of a paralytic almost their accustomed strength, and voluntary motion; but these good effects, the greatest part of them at least, were only temporary, and the patient relapsed. But he had never seen or known the effects of electricity in the contrary affection, viz. rigidity of muscles. He was very desirous therefore of trying its effects in this instance, and of shaking the rigid muscles by electricity; especially as he could have it done with very little pain, and no danger to the patient. He just now mentioned, that he was able in paralytic persons to make any particular muscle at his will exert its action. This was to be effected by simple electrizing only; but by modifying and altering the apparatus of the charged vial, he was able to do much more. It was 17 years before, that he discovered, and communicated at that time, that by means of the electric circuit he could cause the electricity to pervade any muscle, any number of muscles, or whatever part of the body he pleased, without affecting

the rest with that unpleasing sensation. Many experiments relating to this matter were printed in the 44th vol. of the Phil. Trans.

But to return to the patient: he ordered her to be electrized about the middle of Nov. This was done every day, or every other day, for about 20 minutes, by simply electrizing the muscles subservient to the motion of the lower jaw, her neck, and her arms. This at first was very difficult to be atchieved; as she was not capable of being placed in a chair to be electrized by herself, and as an assistant could scarcely hold her on account of her being greatly distorted. It with difficulty however was done. After about a fortnight the convulsions left her, and her sleeps were longer and more quiet; but the rigidity continued the same. After this, such parts of her body, as were thought expedient, were made part of the electric circuit, and were shaken by the explosion of the charged vial. These applications were at first more particularly made to the temporal and masseter muscles (the parts first affected) and to the muscles of the neck and arms; afterwards to those of her back, hips, thighs, and legs. Care was taken to moderate the shocks in a manner, not to be too severe; and she was electrized every 2d, and sometimes every 3d day.

These fits, which were of the epileptic kind, left her in about a fortnight from her being electrized, and had not afterwards returned, even in the slightest degree. In about a fortnight more her jaw was looser, and the muscles of her neck and arms had a large share of motion; and it was very observable, that as her muscles increased in their power of motion, they increased in their size, and the patient in her strength. By the end of January, by continuing the electricity, every muscle in her body was loose, and subservient to her will; and she could not only stand upright, but walk, and even run like other children of her age. With her strength, she had so far recovered her flesh and colour, that her appearance was that of a reasonably healthy child; and her breath had quite lost its late offensive smell. The only parts of her body not quite so loose as the rest were the temporal and masseter muscles, which were the parts first affected by the disease. This prevented her opening her mouth quite so wide as she formerly could; but this hindrance was so little, as not then to be noticed, unless hinted at beforehand. She went to school, lived at large, and went out every day when the weather was seasonable; but the electrizing was still continued, though not so constantly and regularly as before. This he proposed should be continued until the return of warm weather. In the last week this child was presented to the committee of the Foundling Hospital, where several of the governors, who were apprised of her case, expressed their amazement at so unexpected a recovery.

It is here to be observed that, except the muscles subservient to the motion of her jaw, none so long continued their rigidity as those of the back, denomi-

nated longissimi dorsi by anatomists. These, when almost all the other muscles of the body were loose, remained tense and hard; and by drawing the loins up towards the shoulders, continued the arch of the spine before mentioned. As the patient was so much emaciated, these muscles might be traced, on each side of the spine, from their origin to their insertion; and for a considerable time after she was in other respects recovering, these felt hard like twisted cords. At length however, by directing the electricity through them, and the parts near them, in a very liberal quantity, these likewise gave way, and became as loose as any other muscles of her body.

In proportion as a matter is extraordinary, the proofs to support its reality should be extraordinary. That excellent maxim, 'nil temere credere,' should never be lost sight of in our inquiries, otherwise novelty and the love of the marvellous will be apt to mislead us. On the other hand, the indulgence of an extravagant pyrrhonism may prove equally detrimental in every endeavour to extend the bounds of science. It may prevent the giving due weight to matters of real information, and hinder their being made useful. For his part, he should think it an indignity offered to the R. S. to lay before them any extraordinary phenomenon, which was supported only by a slight degree of evidence. On the contrary, when a number of concurrent circumstances tend to establish a fact, we ought not in a certain degree to refuse our assent to it, though somewhat out of the common course. Thus in the present case; when an unusual disease of several months' continuance, and when the patient was supposed to be reduced to the last extremity; when medicines and applications of every kind, celebrated by the ablest writers and practitioners both ancient and modern, had been tried with little or no effect, at least with regard to the rigidity; when during a course of electrizing no medicines or applications of any kind were made use of; when likewise during this course, the patient voided no worms, had no purgations, eruptions on the skin, or kindly imposthumations, which might have been considered as critical discharges, and to have brought about the cure; when none of these things happened, and the patient under electrizing only, and that at a very severe season of the year, had been restored to perfect health, he could not refuse his assent in believing it effected by the power of electricity. That so active a principle, when properly directed to the diseased parts, should have important effects, no one could doubt who had been in the least conversant with it. Though at the same time he confessed being well apprised of the salutary effects of warm weather in restoring a more perfect motion to torpid limbs, that had the electrizing been begun in March, and continued to the end of May, though attended with the same success as in the present instance, he could not have suppressed his doubts of the warm weather greatly contributing to it. But as this

was done during the depth of winter, and that a severely cold one, no scruples in his mind could arise on this head.

Perhaps some might be of opinion, that even the cold weather contributed to cure this disorder. But it is well known that warmth relaxes the animal fibres, and that cold constipates and braces them. In this case, the muscles, composed of minute fibres, were as rigid and tense as they well could be, even in a diseased and obstructed state. If cold therefore contributed any thing, it was to make this case worse. And this was conformable to the opinion of Aretæus, (lib. i. cap. vi) who, among the causes of the disease, reckons intense cold; and says, "that for this reason the winter of all the seasons is most productive of this disease." He subjoins, "that women are more subject to it than men, on account of the coldness of their constitution." Celsus (lib. ii. cap. i) likewise expressly asserts, that cold sometimes is the cause of it, and in another part of his excellent work he says, "that the greatest caution should be used to defend the patient from cold; and that therefore the fire in his room should be constant." He also recommends warm bathing, both in water and oil, as conducive to the cure of the disease. To these may be added the sentiments of Cælius Aurelianus, (De Morb. Acutis, lib. iii. cap. vi, viii) who considers that cold is frequently the cause of this disease. He recommends various kinds of warm external applications, such as warm bathing, rubbing the affected parts with warm oil, the application of warm cataplasms, bags of heated bran, or linseed. With Celsus, this author recommends that attention be given to the warmth of the patient's chamber. How far therefore, for the reasons and authorities before-mentioned, cold weather could probably assist in the cure of this case, needed not in his opinion be insisted on.

March 27th, 1763, the patient continued well, her jaw was as loose as ever. The electrizing had been discontinued above a month, and she was in every respect perfectly recovered.

July 8th, 1763, the patient was perfectly well, and there remained not the least indications of her having been diseased.

VII. On the late Mild Weather in Cornwall; and on the Quantity of Rain fallen there in the Year 1762. *By the Rev. Wm. Borlase, M. A., F. R. S.* p. 27.

Our winters in Cornwall are generally more mild than any where in this island, but Mr. B. did not remember so wide a difference as that of the present season there and London. In November, on the 12th, 13th, 14th, our frost began, mostly attended with hoar frosty mornings: here and there a pool of still water had a film over it, scarcely strong enough to bear an egg, not a large pebble; and the frost was always over before noon. Frost of the same degree on the 18th and 20th, hoar frost only the 26th. Frost, but of no greater degree, Dec. 5th,

6th, and 7th. Hoar only on the 11th. On the 14th and 15th frost, but of the above degree only: a little sleet on the 31st post meridian. To the 22d no frost or snow. On these coldest days the thermometer was never so low as 38°, but on 3 days only, viz. Dec. 14th and 15th, and Jan. 9th. It will appear that our cold was nowise excessive, when it is considered that the balm of Gilead, in the natural open ground, has not suffered: the myrtles are in perfect health; the mignonettes in flower; the cluster rose and white violet in bloom at Christmas; and the scarlet double ranunculus is full blown; the double hyacinths have formed their bells, and some are now ready to unfold. The whole quantity of rain fallen in 1762 was $29\frac{9}{10}$ inches.

VIII. A Delineation of the Transit of Venus expected in the Year 1769. *By Mr. James Ferguson.* p. 30.

Besides the delineation, Mr. F. points out some places that might be very proper situations for observing that transit, viz. Wardhuys in Norwegian Lapland, and the Solomon isles in the great South Sea; or that any other place near the north cape will be just as well for the northern observers; and Tuberon's isle, or St. Bernard's, or the Fly islands, in the great South Sea, will answer as well for the southern.

IX. An Account of an Appulse of the Moon to the Planet Jupiter, observed at Chelsea. By Mr. Samuel Dunn. p. 31.

The alteration of the angles of position made by the cusps of the moon, and a planet to which the moon makes a near appulse, will always enable those astronomically inclined to determine from observation, the longitudes of places, by the naked eye, and a clock or watch set to apparent or equal time. Such an observation Mr. D. made at Chelsea, Dec. 25, 1762, at $11^h\ 0^m\ 30^s$ apparent time. Jupiter's distance from the moon half a degree, when in the line of the moon's cusps. Lat. 51° 29′ 5″, Long. 41″ west of Greenwich.

X. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1762, *pursuant to the Direction of Sir Hans Sloane, Bart. By John Wilmer, M. D.* p. 32.

This is the 41st presentation of this kind, completing to the number of 2050 different plants.

XI. Observations made by Mr. John Bartram, at Pennsylvania, on the Yellowish Wasp of that Country.* p. 37.

Mr. B. saw several of these wasps flying about a heap of sandy loam; they settled on it, and very nimbly scratched away the sand with their fore feet, to

* This insect belongs to the Linnæan genus *Sphex*.

find their nests, while they held a large fly under their wings with one of their other feet; they crept with it into the hole that leads to the nest, and staid there about 3 minutes, when they came out. With their hind feet, they threw the sand so dexterously over the hole, as not to be discovered; then taking flight, soon returned with more flies, settled down, uncovered the hole, and entered in with their prey.

This extraordinary operation raised his curiosity to try to find the entrance, but the sand fell in so fast that he was prevented, till by repeated essays he was so lucky as to find one. It was 6 inches in the ground, and at the farther end lay a large maggot, near an inch long, thick as a small goose quill, with several flies near it, and the remains of many more. These flies are provided for the maggot to feed on, before it changes into the nymph state, then it eats no more till it attains to a perfect wasp.

XII. An Account of the Plague at Aleppo. By the Rev. Tho. Dawes, Chaplain to the Factory there. Dated Aleppo, Oct.* 26, 1762. p. 39.

Mr. D. here states that Aleppo and the adjacent country for 6 years past had been in a very terrible situation, afflicted during the greatest part of that time with many of the Almighty's severest scourges. Its troubles were ushered in by a very sharp winter in 1756-7, which destroyed almost all the fruits of the earth. The cold was so very intense, that the mercury of Fahrenheit's thermometer, exposed a few minutes to the open air, sunk entirely into the ball of the tube. Millions of olive-trees, that had withstood the severity of 50 winters, were blasted in this, and thousands of souls perished merely through cold. The failure of a crop of the succeeding harvest occasioned a famine with all its attendant miseries. In many places the inhabitants were driven to such extremities, that women were known to eat their own children, as soon as they expired in their arms, for want of nourishment. Numbers of persons from the mountains and villages adjacent came daily to Aleppo, to offer their wives and children to sale for a few dollars, to procure a temporary subsistence for themselves; and hourly might be seen in the streets dogs and human creatures scratching together on the same dunghill, and quarrelling for a bone, or a piece of carrion, to allay their hunger. A pestilence followed close to the heels of the famine, which lasted the greatest part of 1758, and is supposed to have swept away 50 or 60 thousand souls in Aleppo and its environs.

Having in a former letter given an account of the earthquakes of 1759 and 1760, Mr. D. proceeds to the description of another scourge, the plague, which after lying dormant from the autumn and through the winter, made its appearance again in Aleppo at the end of March 1761, to the great consternation of

* A complete history of this memorable plague was published by Dr. Patrick Russell, in 1791.

the inhabitants. The infection crept gently and gradually on, confined chiefly to one particular quarter, till the beginning of May, when it began to spread visibly and universally. They shut up on the 27th, and their confinement lasted 96 days. The fury indeed of the contagion did not continue longer than the middle of July, and many of the merchants went abroad with caution early in August; but as the British consul had no urgent business to induce him to expose himself to any risk, his family remained in close quarters till they could visit their friends with tolerable security. As an addition to the uneasiness of their situation, the earthquakes returned the latter end of April, though with no great violence, except the first shock, and that much less terrible than those of 1759. They felt 6 or 7 within the week, and 4 more at long intervals during their imprisonment; but as they were all slight, their apprehensions soon subsided. At their release from confinement the last day of August, they flattered themselves with the hopes of a speedy release from danger; but it pleased God to order it otherwise. In all the plagues with which Aleppo had been visited in the 18th century, the contagion is said to have regularly and constantly ceased in August or September, the hottest months in the year; and it is pretty certain, that it disappeared about that time in 1742, 1743, 1744, and 1760; but unfortunately the year 1761 proved an instance of the fallacy of general observations on this dreadful subject; for from the end of March 1761 to the middle of September 1762, scarcely a day passed without some deaths or fresh attacks from the distemper; and though the violence of it ceased in the autumn, yet on an average it was fatal to at least 30 persons in every week, from that time to the end of the winter. In February 1762 they were pretty healthy: hearing but of few accidents, and those in the skirts of the city, they once more began to entertain some faint hopes of a farther exemption, but they were of very short duration: in March the infection spread again, and in April increased with such rapidity, that they were obliged to retire to close quarters on the 26th of that month, and did not go abroad again until the 28th of August, when the burials were reduced to about 20 a day; the infection gradually decreased till the middle of September, after which time no accident had been heard of.

Mr. D. wishes he could with any precision determine the loss in the 2 summers 1761-62; but, in times of such general horror and confusion, it is in a manner impossible to come at the exact truth. If you inquire of the natives, they swell the account each year from 40 to 60,000, and some even higher; but, as the eastern disposition to exaggeration reigns at present almost universally, little accuracy is to be expected from them: this however is certain, that the mortality of 1762 was very considerable, perhaps not much inferior to any in the 18th century. Some of the Europeans had been at no small pains and expence to procure a regular and daily list of the funerals during their confinement, and

their account amounted to about 20,000, from the 1st of April to the 1st of September 1762, and about one-third less the preceding summer. This calculation Mr. D. was inclined to think was pretty right, though there were some strong objections against a probability of being able to procure a just one in such circumstances: for the Turks keep no register of the dead, and have 72 different public burial places in the 7 miles circumference of the city, besides many private ones within the walls. The Christians and Jews, who are supposed to be rather less than a 7th part of the number of inhabitants, have registers, and each nation one burial place only; their loss in 1762 was about 3500 in the 5 months.

Mr. D. mentions that during the months of June and July, (the greatest part of which the burials were from 2 to 300 a day) the noise of men singing before the corpses in the day, and the shrieks of women for the dead both day and night, were seldom out of their ears. Custom soon rendered the first familiar to him, but nothing could reconcile him to the last; and as the heat obliged them to sleep on the terrace of their houses in the summer, many of his nights' rest were disturbed by these alarms of death.

All the English had been so fortunate as to escape infection in their houses, though each year 4 or 5 Europeans had been carried off, and each year the plague broke out in 2 houses that join to the English consul's. In one of them died a Franciscan priest, after 2 days' illness, whose bed was placed about 6 yards distance from Mr. D.'s. He believes he was in no great danger, as a wall 9 or 10 feet high separated their terraces; but had he known his situation, he should have moved farther off. The year before, he was thrown into a very great agitation of mind for a few days, by the death of his laundress's husband; for the very day he died of the plague, his servant had received his linen from his house, and he had carelessly put on some of it, even without airing. This accident happened many weeks after they were open, and his illness was industriously kept a secret. The last month of his confinement in 1762 passed very heavily with him indeed; for he found his health much disordered. Whether it proceeded from a cold he catched in his head by sleeping in the open air in some very windy nights; from want of exercise; or from the uneasiness of his mind naturally attending his melancholy situation, he knew not; but his nerves seemed all relaxed, his spirits in a state of dejection unknown to him before, and his head so heavy and confused, that he could neither write nor read for an hour together with application or pleasure. Since his release, he had passed a month at a garden about an hour's ride from the city, for the sake of exercise and fresh air, and found himself much relieved by it, though his head was far from being then clear.

Among many anecdotes relating to this plague that he had heard, the following seemed somewhat extraordinary: and yet, as they were well attested, he had no

reason to doubt of the truth of them, viz. last year as well as this (1762) there had been more than one instance of a woman's being delivered of an infected child, with the plague sores on its body, though the mother herself had been entirely free from the distemper.

A woman that suckled her own child of 5 months, was seized with a most severe plague, and died after a week's illness; but the child, though it sucked her, and lay in the same bed with her during her whole disorder, escaped the infection. A woman upwards of 100 years of age was attacked with the plague, and recovered; her two grandchildren, of 10 and 16, received the infection from her, and were both carried off by it.

While the plague was making terrible ravage in the island of Cyprus, in the spring of 1760, a woman remarkably sanguine and corpulent, after losing her husband and 2 children, who died of the plague in her arms, made it her daily employment from a principle of charity to attend all her sick neighbours, that stood in need of her assistance, and yet escaped the infection. Also a Greek lad made it his business for many months to wait on the sick, to wash, dress and bury the dead, and yet he remained unhurt. In that contagion 10 men were said to die to 1 woman; but the persons to whom it was almost universally fatal, were youths of both sexes. Many places were left so bare of inhabitants, as not to have enough left to gather in the fruits of the earth; it ceased entirely in July 1760, and had not appeared in the island afterwards.

The plague seems this year (1762) to have been in a manner general over a great part of the Ottoman empire. They had advice of the havoc it had made at Constantinople, Smyrna, Salonicha, Brusa, Adena, Antioch, Antab, Killis, Ourfah, Diarbekir, Mousol, and many other large towns and villages. Scanderoon, for the first time he believed in the 18th century, had suffered considerably; the other Frank settlements on the sea coast of Syria had been exempted, excepting a few accidents at Tripoli, which drove the English consul, Mr. Abbott, into a close retirement for a week or two, but the storm soon blew over.

XIII. Observations on Sand Iron. By Mr. Henry Horne,* p. 48.

Mr. H. procured, from Mr. Adams the Virginia merchant, a quantity of the black sand, and in order to estimate its comparative weight with that of iron ore, he procured some of the richest ore he could get, which having reduced to powder, he filled an ordinary tea-cup with it. He afterwards filled the same cup with some of the sand, and on comparing the weights with each other, he found that the weight of the sand was to that of the ore as 3 to 2; and having taken notice how readily the sand was attracted by the magnet, he was convinced that

* Commonly called magnetic sand. It is a black oxyd of iron.

the sand must certainly contain a very considerable quantity of iron, and therefore determined to make trial of it. He was however, for some time, interrupted in his design, by information he received from a friend, that such an inquiry had been made many years before, by a member of the R. S., and of chemical knowledge, but without success; and that the experiments were published in the Phil. Trans., vol. xvii, p. 624.*

Having thoroughly considered those experiments, they appeared to Mr. H. far from decisive, and that if Dr. Moulin had placed more confidence in the power of the magnet, and less in his menstruums, he would rather have concluded that there might be some sorts of iron ore which his menstruums would not touch in the moist way, nor any regulus be produced from them in the dry, as he made use of them, which yet might, under some other hands, be subdued by more apt and powerful methods than any which at that time he was acquainted with.

Mr. H. however apprehended he might draw this conclusion from his experiments, viz. that the sand was not simply iron, but that it was strongly united with a fixed and permanent earth, which could not be separated from it without some powerful means; but he could not think this a sufficient objection to the prosecution of an experiment, which, if it succeeded, might be attended with very happy consequences. Proceeding therefore on this supposition, he mixed up about 8 or 9 oz. of the sand, with a proportional quantity of a strong corrosive flux, which he put together into a crucible, and committed it to a very strong fire in an excellent wind-furnace, where he kept it for between 2 and 3 hours, hoping by this means to have answered the intended purpose; but he confesses he was not a little surprised that, after the crucible was taken from the fire, he could not find a single grain of metal in the remaining contents.

This disappointment greatly puzzled him, till having thoroughly examined into the unexpected event, without being able to discover any reason sufficient to incline him to recede from his former opinion, as to the component parts of the sand, he concluded that the flux might possibly be a very improper one; for though it might have effected the intended separation, yet it might at the same time be sufficiently powerful to divide the particles of the metal, when separated, so very minutely, as to be capable of subliming and carrying them off imperceptibly; and finding the contents greatly diminished, so that the quantity remaining bore but a small proportion to that which was first put into the crucible, he concluded that this must really have been the case, and that some very different method must be pursued in order to produce the desired effects. He immediately determined to make a 2d trial, in which he proceeded in the following manner. He took the same quantity of sand made use of in the former expe-

* Vol. iii, p. 495 of these Abridgments.

riment; and first he spread it, without any addition to it, upon an iron plate over a strong fire, where he gave it a very powerful roasting, to try if by that means he could not relax, and loosen the component parts to such a degree, as to make the separation and reduction of the metal more easy, when he should bring it into the furnace. When he had so done, he mixed it up with a flux of a very peculiar but gentle nature, which he had before made use of for other purposes with great success, and committed it, as in the former experiment, to the furnace, where he urged it by a very strong fire for about 3 hours, and on taking it out, he found the event answerable to his most sanguine expectations; for in the bottom of the crucible he found, as near as he could remember, rather more than half of the sand he had put into the crucible reduced to a very fine malleable metal.

Being fully convinced, by the experiment, that this black sand was a very rich iron ore, he acquainted some of his friends with it, who being largely engaged in trade to those parts of the American colonies, where he was informed this sand was to be easily procured, and in very large quantities, he was in great hopes an account of this nature would have inclined some of the gentlemen in that part of the world, to have prosecuted so useful a discovery in a larger way; and he owns he had often wondered that an affair of such consequence should have lain dormant for so many years.

However he was a few months preceding the above date, pleasingly surprised to find in the hands of Mr. Collinson, not only a pamphlet, but a letter on the subject addressed to the Society for the Encouragement of Arts and Manufactures, by Mr. G. Elliott, who relates that, though previous to his attempt of making iron from this sand, he met with nothing but what was discouraging from the most skilful persons to whom he proposed his design, yet that he had such a persuasion in his own mind of the practicability of the thing, that he could not rest till he had made a trial, and the event proved encouraging much beyond his expectations, insomuch that he could scarcely believe the trial had been fairly made, till a 2d trial evinced with certainty, that 83 lb. of the sand would produce a bar of excellent iron weighing 50 lb.: a prodigious yield indeed, and far beyond what Mr. H. had ever heard of from the richest iron ores that are any where to be found; most of the ores he had ever met with or heard of, yielded little more than half in pig metal, and which suffered a waste of near $\frac{1}{6}$ part to make tolerable good bar iron, and much more if he was rightly informed, when the iron was intended for more valuable purposes, such as being drawn into wire, &c.

On this subject Mr. Horne received the following letter from Mr. Elliot; dated Killingworth, Oct. 4th, 1762.

'I understand by Mr. Collinson, that you have seen, and greatly approve of,

the sample of sand iron which was sent; that you are desirous to know how it was made, and whether it can be made in large bars. The little bar you saw was cut off from a bar of 52½ lb., the first that was made at my son's work, the first that was ever made in America, and probably the first that was ever made in the world, in that manner, and so large a bar. I never heard of any attempt made upon the iron sand till that of your's 20 years ago, of which Mr. Collinson gave me an account in his letter.

As to the manner of making the iron, it is wrought or smelted in a common bloomary, in the same manner as other iron ore is smelted; excepting this difference, this iron sand is so pure, so clean washed, that there is not a sufficient quantity of cinder or slag to promote and perform the smelting, therefore we add either the slag which issues from other iron, or else add some bog mine ore, which abounds with cinder; in this way it is as capable of being wrought as rock ore or bog mine. I was in hopes that if this iron sand could be wrought at all, the particles being so very fine, it would smelt very quick; but herein I found myself mistaken, every particle has a will of its own, and must have its own particular smelting, for instead of its being performed in less time, it took more than common iron ore, but upon further experience, and more acquaintance with this sand, the workman has shortened the operation from 5 hours down to 3: if by any means it might be reduced to the same time with pig iron, it would be a most useful improvement. If you can afford any directions to hasten the operation, I should be greatly obliged for any instructions. There is so much of this sand in America, that I am apt to think that there is more iron ore in this form of sand than in mines.

I have written an essay on the subject, which I hope Mr. Collinson will let you see, as I hope to see what you are about to publish. My son has a steel furnace, which was erected several years before the act of parliament prohibiting them in the plantations: he has converted some of the sand iron into steel, of which I send you a sample; as also a sample of the iron. As my son had no instructions for making steel, we were forced to hammer out the skill by various trials as we could; so conclude that he is still imperfect, and wants your help and direction to bring it to perfection, in which art I understand you are a perfect master, and withal kind enough to offer your assistance; for which I am very thankful, and look upon it as an additional favour, if you will be pleased to indulge me with the benefit of your correspondence, for I live in a corner of the world where such information, as I trust you are able to furnish, will be highly beneficial. Previous to my attempt of making iron from sand, I proposed my project to those who were the most skilful in those affairs, but met with nothing but what was discouraging; yet after all had a persuasion of the practicability of

the thing to a degree next to enthusiasm, so that I could not rest till I had made trial. I am glad that the iron has such qualities as to meet with your approbation; I knew that the iron was good, but did not know that it was so good as your superior knowledge has found it. I want to know what such iron will sell for in England, whether it will be worth while to send it. This black sand is a treasure that has long lain hidden from the world, and is what may render the colonies more valuable to Great Britain.'

(P. S. The bars of iron which have hitherto been made of sand, are from 50 to 50 gross, hope in time to have them reach to 70 lb. weight each; experience must determine that matter; we can do better than at the time the essay was written. We have been visited with a long and sore drought, have done nothing for a long time for want of water.)

The samples which accompanied this letter were 2 small bars, weighing only a few ounces, one of the iron made from the sand, the other of steel made from the same iron. These bars Mr. H. tried, and found that the bar of steel worked extremely well under the hammer, was very pure and clean, and free from flaws. On the contrary the bar of iron turned out much otherwise, for though it appeared to bear the force of the hammer, as well as the steel, yet it was not near so pure, but broke out in flaws and hollows, almost through the whole of the bar, and which a welding heat would by no means bring into proper union; this however engaged Mr. H. to try a different method, which was, when the bar was reduced into a proper size for the purpose, to double it up 3 times, one part of the bar upon the other, and to try if it would then bear welding and become more consistent, and by this means he found the end perfectly well answered; for it bore the force of the fire and the hammer, and became in a manner perfectly sound. This severe trial proved to a demonstration, that the iron possessed all that agreeable toughness and ductility, for which the Spanish iron is so deservedly famous, without partaking of that vile red-short quality, for which the latter is very remarkable, and manifestly tends to prove the excellency of this sand iron, when reduced into bar iron under proper care and circumspection. This sand is so pure, and so clean washed, that their first method of reducing the sand to bar iron proved too tedious, for want of some of those adventitious materials, to promote and perform the smelting, and which always accompanies the common ore, whether it be of the rock or bog kind; which materials mixing with the matter, made use of by way of flux, and uniting with the ashes of the fuel employed in melting down the ore, is usually run into a thick opaque glassy substance, forming as it were a covering over the metal, which by its gravity naturally sinks to the bottom; this the workmen call cinder. Now the want of this matter rendering the operation too tedious, Mr. H. finds they had recourse either to this cinder brought from other iron works, or to a quantity of the bog-

mine, which he doubts not would abundantly furnish matter for cinder. If they had used only the first, and that properly chosen, it might very probably have been of some service, without doing any material injury to the metal; but if the bog mine is used, though the surface might be apparently more, yet in all likelihood the injury would be infinitely great; and he was inclined to believe that something of this kind occasioned the difference observed between the 2 bars above mentioned, viz. that the one might have been reduced by the help of more pure materials, and the other by the assistance of their bog mine, whose constituent parts abounding with many impurities, some of which, by mixing with the metal, may have occasioned the defects above complained of, and which required so severe an operation both of the fire and hammer to separate from it. He was therefore of opinion, that as the prosecution of this useful discovery deserved the greatest encouragement, if the Society of Arts and Manufactures should take it under their patronage, the premium they might think proper to propose should rather be given to the person who should produce the purest metal, than to him who should produce the greatest quantity; for otherwise he was afraid they would be deprived of what he should esteem the most valuable part of this discovery, he meant the obtaining a more pure, and better kind of iron, than any they had hitherto been possessed of.

XIV. On the State of the Cold at Berlin last Winter, dated Feb. 12, 1763. By Simon Peter Pallas of that place, M.D. p. 62.

We have had great frosts here, as indeed all over Germany. Dec. 27, a little after seven o'clock in the morning, the cold was excessive, the mercury in the thermometer of Fahrenheit stood at 4 degrees under 0, which is 15 degrees under 0 of Reaumur's scale, than which the cold in 1740 was but very little more intense. Mr. Euler, junior, observed the same day the thermometer at the same degree; about 8 and at 9 o'clock of that day, the mercury in the barometer stood at the height of 30″ 1‴, the like of which never had been observed at Berlin before.

XV. Of a Remarkable Darkness at Detroit, in America. By the Rev. James Stirling. p. 63.

Detroit, 25th Oct. 1762.

SIR,

Tuesday last, Oct. 19, 1762, we had almost total darkness most part of the day. The darkness continued till 9 o'clock, when it cleared up a little. We then for the space of about a quarter of an hour saw the body of the sun, which appeared as red as blood, and more than 3 times as large as usual. The air all this time, which was very dense, was of a dirty yellowish green colour. We were obliged

to light candles to see to dine, at one o'clock, though the table was placed close by two large windows. About 3 the darkness became more horrible, which augmented till half past 3, when the wind breezed up from the s.w., and brought on some drops of rain, or rather sulphur and dirt, for it appeared more like the latter than the former, both in smell and quality. Mr. S. took a leaf of clean paper, and held it out in the rain, which rendered it black whenever the drops fell upon it; but when held near the fire turned to a yellow colour; and when burned it fizzed on the paper like wet powder. During this shower the air was almost suffocating with a strong sulphureous smell; it cleared up a little after the rain. There were various conjectures about the cause of this natural incident. Some imagined it might have been occasioned by the burning of the woods; but Mr. S. thinks it most probable, that it might have been occasioned by the eruption of some volcano or subterraneous fire, whence the sulphureous matter may have been emitted in the air, and contained in it until meeting with some watery clouds, it has fallen down together with the rain.

XVI. Of a Remarkable Marine Insect. By Mr. Andrew Peter Du Pont.* p. 57.

In a calm on his voyage to England, on board the Friendship, Capt. Thompson, two persons swimming took up this most singular creature floating on the surface. Its motions muscular. Its length a little more than 1 inch. Four small and short horns, probably its eyes. It protruded them in the water only; an orifice in the front part, seemingly its mouth. Two round spots opaque, possibly respiracula. The mid-line of the back part appeared through a common magnifier like a silver leaf, and was in continual undulating motion, either from the muscles or circulation of juices. Two side lines extending the whole creature's length, and ending in one in the tail of a deep blue. The claws, or tentacula, end in a deep blue; a silvery cast intermixed with the blue over the whole back, or upper parts, where the blue is lighter. It can turn itself on the back by a muscular contraction of the head part, the tail, and ramified arms inwards. The inferior parts are white. It died the third day, though the water was shifted once every day.

XVII. On the late Transit of Venus. By M. Wargentin, Sec. to the Royal Acad. of Sciences in Sweden. p. 59.

The observations on the last transit of Venus over the sun, made at the Cape of Good Hope, are excellent, and seem to decide, that the horizontal parallax of the sun is 8″.1 or 8″.3 at most. Mr. W. had before found it to be that quantity,

* The animal here described and figured by Mr. Du Pont is not an insect, but belongs to the Linnæan tribe of Mollusca, and is the *Doris radiata* of the Gmelinian edition of the Systema Naturæ.

from the observations made in Europe compared together; but the observations made at the Cape confirm it with the greatest evidence.

It is of importance to be assured of the longitude of the places where the observations were made. M. W. endeavoured to determine them the best he was able, by observations of the eclipses of Jupiter's satellites, made at the same places. And he sets down a list of all the observations of these satellites made at different places the last year. It is pity that Messieurs L'Abbe Chappe, and Rumoski, did not succeed in observing several eclipses of the satellites, at Tobieske and Selenginsk, the better to confirm the longitudes of those places. However it appears that the difference between the meridians of Greenwich and Tobieske, is scarcely more than 4^h 32^m 55^s. That between the meridians of Greenwich and Selenginsk, to judge from the 3 immersions observed there, should be but 7^h 6^m 0^s, but from other considerations he thinks it must be 10 or 15^m more. If the longitude of these places should be more exactly determined, he is persuaded that we should obtain the parallax of the sun to nearly the 10th of a second, so exact the observations made at Selenginsk and Tobieske and the Cape appear to be.

Mr. Planman at Cajaneberg, observed the transit as follows;

The beginning of the entrance................ at	3^h	59^m	56^s M
Total immersion of Venus, or interior contact......	4	18	5
Second interior contact, or beginning of the exit....	10	7	59
Total emersion..............................	10	26	22

Mr. Planman made use of a telescope of 20 or 21 feet; the latitude of Cajaneburg is 64° 13′ 30″; the difference of meridians between Greenwich and Cajaneburg is sufficiently determined by observations on the eclipse of the moon May 18, 1761, made at Stockholm and Cajaneburg.

*XVIII. Remarks on the Censure of Mercator's Chart, in a posthumous Work of Mr. West of Exeter. By Mr. Samuel Dunn.** p. 66. *Dated Sept.* 4, 1762.

Mr. D. wishes to know if any paper has been printed in the Phil. Trans., concerning a sphere being inscribed in a hollow cylinder, and swelling its surface to the sides of the cylinder, thus to construct a more true and accurate chart for the purposes of navigation, than that which was invented by Edward Wright, and has long gone under the name of Mercator. The reason why he asks this is, he says, because there is lately published a posthumous work of one Mr.

* This gentleman was a native of Crediton, in Devonshire, where he kept a mathematical school for several years; but afterwards removed to Chelsea, where he followed the same occupation. He was well skilled in nautical calculations, and was a good practical astronomer. Besides several papers inserted in the Phil. Trans., he was also the author of some separate treatises on mathematical subjects, and published an Atlas in folio, which has been held in much estimation. He died in good circumstances, and left an estate of about 30 pounds a year, to support a mathematical school in his native town, the first master of which was appointed in 1793.

West, of Exeter, revised by J. Rowe, in which it is strongly insisted on, that the graduation of Mercator's chart is erroneous, and that the same, if rightly correspondent with the loxodromiques or rhumbs, should be graduated as a line of natural tangents, from the equinoctial to the poles. Now this error might have passed the less observed, but the Critical Review of last month sets it forth as a masterly performance, and a thing of the greatest merit and importance in navigation.

That there is a respect due to Edward Wright for his invention, that his principles are true, that Mr. West, or his editor, and both (if both of the same opinion) are false, is most certain. That the characters and abilities of Dr. Halley, Sir Jonas Moore, Mr. William Jones, Mr. James Hodgson, Mr. Haselden, and many others, for they are almost numberless, both of higher and lower mathematicians, who have written on the certainty and utility of Wright's chart, that the characters and abilities of these able geometricians are attacked by Mr. West and his editor, and by the Critical Reviewers, is plain, and that this will have great weight with many not over well acquainted with geometry, is no less plain.

But there are other circumstances; Edward Wright himself gives the very same construction by his words, as Mr. West does, though his tables make out quite another thing, that is, both Wright and West say expressly, the sphere being in the hollow cylinder, and the equinoctial remaining fixed without swelling, while the other parts swell towards the poles, the chart will be formed. But in this, Wright has badly expressed his own thoughts, for his tables make it that the equinoctial must either swell or contract itself. And this is very excusable in Edward Wright, for at that time geometricians had no notion of fluxions, or the increase of magnitude by local motion. Mr. West and his editor have therefore fallen into this error; they have taken the words, but not the sense of Edward Wright, and the Critical Reviewers vindicate them, and make it as if this property had been communicated to the Royal Society by Mr. West. The proposed demonstration of this tangential property, at page 58 of Mr. West's book, is no demonstration at all; there is nothing more plain than that in order to have the meridians at equal distances, the degrees of latitude must be enlarged to the same proportion in every part, as the circular meridians are nearer towards the poles, which proportion is as the cosine of the latitude to the radius.

XIX. A Defence of Mercator's Chart against the Censure of the late Mr. West, of Exeter. By Mr. Wm. Mountaine, F.R.S. p. 69.

The greatest single advantage that the important business of navigation ever received, was from the invention of the mariner's compass; and next to this, the projection of a true nautic practical chart claims place. This last was performed by that great improver of navigation Mr. Edward Wright, as appears by his

book intitled 'Certain errors in navigation detected and corrected,' published about the year 1599. In chap. 2d, he tells us, 'that the errors in the plain chart had been complained of by divers, as namely by Martin Cortese, Petrus Nonius, and even Gerardus Mercator seemeth to have corrected them, in his Universal Map of the World; yet none of them had taught any certain way how to amend such gross faults:' And in his Preface he declares, 'that by occasion of Mercator's map, he first thought of correcting so many and great absurdities in the common Sea Chart, but the way how this was by him done, he neither learnt of Mercator, nor of any man else.' Wright's method (erroneously called Mercator's) was at this time then adopted; has continued ever since in use; and has been improved by some of the greatest mathematicians who have flourished since that time; and though sometimes attacked, yet it has been found impregnable.

The first person who charged Mr. Wright with errors in his tables of rhumbs, is Simon Stevins, in his large volume of mathematical remembrances, which Wright himself plainly confutes in a subsequent edition of his book: now Stevins does not condemn the principles, but only asserts that his tables have some faults in them, and endeavours to prove that the 4th rhumb, at 78° of longitude, ought to have 61° 26′ of latitude, whereas Wright makes it only 61° 14′. Hence the great difference is no more than 12 minutes; and what inconvenience hence can arise to the mariner in such a run, were this the fact? But it turns out otherwise; for this difference is reduced to less than one minute (even according to Stevins's own way) as evidently appears from Wright's answer in page 214. If every rhumb is then found to possess its true latitude in this chart, at every degree and minute of longitude, without any sensible or explicable error (to make use of our author's own words) it follows, that the degrees of latitude are duly encreased, or that the table of meridional parts is true.

Doctor Halley has given a curious method of dividing the nautical meridian, and of performing the problems in sailing according to the true chart, in the Philos. Trans., N° 219, by a method different from Mr. Wright's, but so nearly corresponding in practice, that this alone is a sufficient testimony in favour of this author. Our worthy brother Mr. John Robertson, in his excellent Elements of Navigation, Vol. 2, page 358, expresses himself thus: "Now though a table thus made (Wright's table of meridional parts constructed to minutes) be abundantly sufficient for all nautical purposes; yet had the secants of smaller parts than minutes been taken, the table would have been more correct; and therefore Mr. Oughtred, Sir Jonas Moore, Doctor Wallis, Doctor Halley, and others, have been induced to find methods of constructing those tables with more accuracy, than by the addition of secants to every minute. But a table

of meridional parts, constructed by the most accurate method, only shows that Mr. Wright's tables do no where exceed the true meridional parts by half a minute, and this only near the pole; for, in latitudes as far as navigation is practicable, the difference is scarcely sensible."

About the year 1720, a curvilinear sea chart made its appearance, said to be done by Henry Wilson, the publishers of which represented Wright's chart as puzzling, difficult, and false. But these groundless assertions were rationally answered by Mr. Thomas Haselden, afterwards master of the Royal Academy at Portsmouth, in a letter and pamphlet addressed to Dr. Halley about the year 1722.

In the year 1755 was published a book intitled, "The art of sailing upon the Sea," by W. E.* which initial letters are sufficient to point out the ingenious author. In page 74, he says, "It is demonstrable, by the method of fluxions, that the length of the part of the meridian line in Mercator's chart, which represents the difference of latitude of two places on the globe, is equal to the difference of the log. tangents of half the complements of the two latitudes, multiplied into the number 2.30258509, and that product into the radius of the sphere." And in the Scholium to his Fundamental principles, page 75. "In the few foregoing propositions, I have demonstrated the truth of the chief methods of sailing now in use; and deduced them from their genuine principles, and fixed them on their proper foundations: by which the reader will be enabled to see that this theory is not founded on false principles; but on such as are solid and true; and consequently that all calculations built on it may be depended on as exact."

Notwithstanding these, Wright's method is charged with great imperfection by the late Mr. West of Exeter, in his posthumous work, Mr. West therein declares that "the errors of the plain chart are corrected, in a great measure, by Mercator's or Wright's chart; though the latter is not a true projection of the sphere in any shape; nor indeed is it pretended to be such by Mr. Wright, one of its inventors."—The first part of this paragraph surely contains a contradiction; for how can the errors in the plain chart be in a great measure corrected by a projection that is not true in any shape? And in answer to the latter part,—Mr. Wright has no where made such concessions. And further, Mr. West blends Wright and Mercator together, when at the same time it does not appear that the latter ever published any principles of this kind of projection to the world.

In the 20th article of the book, Mr. West has laid down a method of constructing a nautical chart, which he asserts to be "the first representation of

* William Emerson.

the terraqueous globe ever yet invented, in which the meridians, parallels, and rhumbs, are justly and truly projected in right lines, for the latter cannot be so projected in Mercator."—If they cannot be so projected in Wright's, they cannot in his; for in both, the meridians are said to be right lines and parallel, and therefore the rhumbs must be right lines also, or how can they intersect the meridians so situated at equal angles? He also says in his scholium, that "It does not appear that Mercator or Wright ever thought of this projection; for the meridian line here is manifestly a line of tangents; whereas in their projection, it is a collection of secants."

What Mercator's thoughts were on this matter when he formed his universal map, I know not, as he has left us no account of it; but what Wright's were, he has very plainly told us in his aforesaid book; and whether his primary conceptions, and preparative modulus, do not only take in the whole, but also the very manner, of Mr. West's construction, will better appear on a due comparison of their respective methods.

By comparing the two modes of construction together, it is not difficult to discover that Mr. West's derives its original from Wright's; for right lines drawn from the centre through all the points in the spheric surface, and terminating in the concave surface of the tube, are secants, and the tube becomes a tangent line to all those respective secants: and, does not Wright's uniform dilatation, by the 2d law of motion, produce the same? West stops here, and gives us a chart at once; Wright calls these his geometrical lineaments only, by which he obtains a rectilinear planisphere, and from which he demonstrates the principles on which his table of meridional parts are founded. And that he does not esteem this as a chart completed, but only his apparatus, and preparative work, which requires yet to be applied and moulded into a true nautical chart, is evident from the next paragraph, "Now then (says he) let us diligently consider of the geometrical lineaments, that is, the meridians, rhumbs, and parallels of this imaginary nautical planisphere, that we may in like manner express the same in the mariner's chart: for so undoubtedly we shall have therein a true hydrographical description of all places in their longitudes, latitudes, and directions, or respective situations each from other according to the points of the compass in all things correspondent to the globe, without either sensible or explicable error."

And hence he proceeds to the proof and application of these his lineaments, to the construction of his table of latitudes, as he calls it; which is, in this edition, computed to minutes of parallel distance, but with a little contrivance in the calculus to reduce the same yet somewhat nearer the truth. Notwithstanding this care and nicety in computation, he is duly sensible that his increments of latitude calculated to minutes, though without any sensible error, are

yet not absolutely true, because they ought to flow with a uniform and uninterrupted motion; he therefore cautiously guards against critical remarks on it in the following paragraph:

"In this table, it was thought sufficient to use such exactness, as that thereby (in drawing the lineaments of the nautical planisphere) sensible error might be avoided. He that listeth to be more precise, may make the like table to decades, or tens of seconds, out of Joachimus Rheticus his Canon Magnus Triangulorum: notwithstanding the geometrician that desireth exact truth, cannot be so satisfied neither; for whose sake and further satisfaction, I thouht good to adjoin also this geometrical conceit of dividing a meridian of the nautical planisphere. Let the equinoctial and a meridian be drawn upon a globe: let the meridian, divided into degrees, minutes, seconds, &c. roll upon a straight line, beginning at the equinoctial, the globe swelling in the mean time in such sort, that the semi-diameter thereof may be always equal to the secant of the angle, or arch contained between the equinoctial and semi-diameter insisting at right angles upon the foresaid straight line: the degrees, minutes, seconds, &c. of the meridian, noted in the straight line, as they come to touch the same, are the divisions of the meridian in the nautical planisphere: and this conceit of dividing the meridian of the nautical planisphere may satisfy the curious exactness of the geometrician; but for mechanical use, the table before mentioned may suffice."

I have carefully endeavoured, says Mr. M., not to mistake the true sense and meaning of Mr. West's proposition in any part of it; if I have not, I cannot pronounce what kind of chart may be formed from his tangent line being made the line of latitudes, or that meridian line on which the tangents are to determine the sections of their respective parallels: I shall only observe, that if the meridians be right lines, and parallel to each other, the rhumbs must be right lines also; but by this tangential projection, these will be deflected from their true bearings, or make the angles of the courses too great, unless some expedient be devised to accommodate this error; and if the rhumbs be not right lines, such chart will then be embarrassed with more difficulties in practice than Mr. Wright's.

XXI. Of a Species of Ophrys, supposed to be the Plant which is mentioned by Gronovius in the Flora Virginica, p. 185, under the Name of Ophrys scapo nudo foliis radicalibus ovato-oblongis, dimidii scapi longitudine. By George Dionysius Ehret, F. R. S.* p. 81.

The root of this plant, from which many fleshy fibres branch, is composed of

* *Ophrys lilifolia.* Lin.?

the foot stalks of the leaves, which envelope each other in such a manner, that they form a kind of bulbous root. From this bulb proceed two oval-shaped, nervous, smooth leaves, having membraneous convolute petioli or footstalks. These encompass a triquetrous scapus, or a single stalk arising from the centre of this root, which produces many flowers of a singular construction. These flowers are supported by small peduneuli, or flower stalks, of a bloody-red colour, which swell into seed-vessels, having at their base an acute denticle. This very singular plant blowed, for the first time in England, in the year 1758, in the curious exotic garden of Mr. Peter Collinson; who received it from Mr. Bertram of Philadelphia.

Mr. Clayton has described a plant, in the Flora Virginica, page 185, under the name of "Bifolium scapo e medio duorum foliorum nudo, aphyllo, ad exortum tenui, paulatim versus apicem accrescente, sex vel septem capsulas sustinente: radice fibrosa carnosa viridi, foliis obvolutâ, humi jacente; fibras paucas emittente, cui radix anni superioris contigua et integumentis marcidis evoluta pellucida adhæret." This description seemingly corresponds with the present plant; but yet Mr. Clayton's character of the several parts of the flower is very different from those here observed; and though it may be thought to come near to an epidendrum, yet it is neither an epidendrum nor a bifolium. This plant, however, should be ranged among the first order of Linnæus's class of gynandria diandria, which consists of several genera.

XXII. New Experiments in Electricity: In a Letter from Mr. Ebenezer Kinnersley, to B. Franklin, LL.D., F. R. S. Dated Philadelphia, Mar. 12, 1761. p. 84.

Exp. 1. Mr. K. placed himself on an electric stand, and, being well electrized, threw his hat to an unelectrized person, at a considerable distance, on another stand; and found that the hat carried some of the electricity with it; for, on going immediately to the person who received it, and holding a flaxen thread near him, he was found electrized sufficiently to attract the thread.

Exp. 2. He then suspended, by silk, a broad plate of metal, and electrized some boiling water under it, at about 4 feet distance, expecting that the vapour, which ascended plentifully to the plate, would, on the principle of the foregoing experiment, carry up some of the electricity with it; but was at length fully convinced, by several repeated trials, that it left all its share of it behind.

Exp. 3. He put boiling water into a coated Florence flask, and found that the heat so enlarged the pores of the glass, that it could not be charged. The electricity passed through as readily, to all appearance, as through metal; the charge of a three-pint bottle went freely through without injuring the flask in

theleast. When it became almost cold, he could charge it as usual. Would not this experiment convince the Abbé Nollet of his egregious mistake? For, while the electricity went fairly through the glass, as he contends it always does, the glass could not be charged at all.

Exp. 4. He took a slender piece of cedar, about 18 inches long, fixed a brass cap in the middle, thrust a pin horizontally and at right angles, through each end, (the points in contrary directions) and hung it, nicely balanced like the needle of a compass, on a pin about 6 inches long, fixed in the centre of an electric stand. Then electrizing the stand, he had the pleasure of seeing what he expected; the wooden needle turned round, carrying the pins with their heads foremost. He then electrized the stand negatively, expecting the needle to turn the contrary way; but was extremely disappointed, for it went still the same way as before.

After the above experiments with the wooden needle, he formed a cross of 2 pieces of wood of equal length, intersecting each other at right angles in the middle; hung it, horizontally, on a central pin, and set a light horse, with his rider, on each extremity; on which the whole being nicely balanced, and each courser urged on by an electrized point, instead of a pair of spurs, he was entertained with an electrical horse-race.

Exp. 5. Let a person in the negative state, out of doors, in the dark, when the air is dry, hold, with his arm extended, a long sharp needle, pointing upwards; and he will soon be convinced that electricity may be drawn out of the air; not very plentifully, for being a bad conductor, it seems loth to part with it; but yet some will evidently be collected. The air near the person's body having less than its natural quantity, will have none to spare; but, his arm being extended as above, some will be collected from the remoter air, and will appear luminous as it converges to the point of the needle. Let a person electrized negatively present the point of a needle, horizontally, to a cork ball suspended by silk, and the ball will be attracted towards the point, till it has parted with so much of its natural quantity of electricity as to be in the negative state, in the same degree with the person who holds the needle: then it will recede from the point; being it seems attracted the contrary way by the electricity of greater density in the air behind it.

Exp. 6. He set the thermometer on an electric stand, and kept it well electrized a considerable time; but this produced no sensible effect. Which shows that the electric fire, when in a state of rest, has no more heat than the air and other matter in which it resides.

Exp. 7. A large charge of electricity sent through wires in contact, even that of a case of 35 bottles, containing above 30 square feet of coated glass, will

produce no rarefaction of the air included in a tube. Which shows that the wires are not heated by the fires passing through them.

Exp. 8. When the wires are about 2 inches apart, the charge of a three-pint bottle darting from one to the other, rarefies the air very evidently. Which shows that the electric fire must produce heat in itself, as well as in the air, by its rapid motion. The charge of the case of bottles sent through the brass wire consumed great part of it into smoke. The thermometer appeared quite opaque with it.

Exp. 9. He suspended a piece of brass wire, about 24 inches long, with a pound weight at the lower end; and, by sending the charge of the case of bottles through it, discovered a new method of wire-drawing. The wire was red hot, the whole length well anealed, and above an inch longer than before. A 2d charge melted it; it parted near the middle, and measured when the ends were put together, 4 inches longer than at first.

Exp. 10. That he might have no doubt of the wire's being hot as well as red, he repeated the experiment on another piece of the same wire, encompassed with a goose-quill filled with loose grains of gunpowder; which took fire as readily as if it had been touched with a red hot poker. Also tinder, tied to another piece of the wire, kindled by it. He tried a wire about twice as thick, but could produce no such effects as that. Hence it appears, that the electric fire, though it has no sensible heat when in a state of rest, will, by its violent motion, and the resistance it meets with, produce heat in other bodies when passing through them, provided they be small enough. A large quantity will pass through a large wire without producing any sensible heat; when the same quantity passing through a very small one, being there confined to a narrower passage, the particles crowding closer together, and meeting with greater resistance, will make it red hot, and even melt it. And hence lightning does not melt metal by a cold fusion, as we formerly supposed. But when it passes through the blade of a sword, if the quantity be not very great, it may heat the point so as to melt it, while the broadest and thickest part may not be sensibly warmer than before. And when trees or houses are set on fire by the dreadful quantity which a cloud, or the earth sometimes discharges, must not the heat by which the wood is first kindled, be generated by the lightning's violent motion through the resisting combustible matter.

Mr. K. then adds a dissertation, showing, from experience, the usefulness of pointed conductors to houses, in securing them from a stroke of lightning.

XXIII. Observations on Electricity, and on a Thunder-storm. By Mr. Torbern Bergman, F. R. S. Acad. Reg. Upsal. Soc. From the Latin, p. 97.

With regard to the electrical experiments with island crystal, given by Mr. Delaval, in the Philos. Trans., vol. 52, p. 355, Mr. B. here states, that after often repeating those experiments, he always found a contrary result. Thus, he exposed various pieces of this crystal to 12 degrees of cold of the Swedish thermometer, filled with quicksilver, being the degrees of cold below the freezing point of water, of which there are 100 between the freezing and boiling points of water. He then rubbed it after the space of some hours, but without producing any except a very small degree of electricity. Next day he repeated the experiment with a greater degree of cold, but with still less success. He next heated a small piece, hoping thus to eradicate its whole force; but unexpectedly he found its electric virtue not at all destroyed, but much increased. This being found to be the case with all the specimens he could collect, he suspects there are different kinds of this crystal, endued with such different properties. And indeed Mr. Delaval had said as much in his paper.

The thunder-storm happened at Upsal, Aug. 24, 1760; but there is nothing very remarkable in the account.

XXIV. Remarks on Swallows on the Rhine: in a Letter from Mr. Achard, in Privy-Garden, to Mr. Peter Collinson, F.R.S., p. 101.

In the latter end of March I took my passage down the Rhine to Rotterdam: a little below Basil the south bank of the river was very high and steep, of a sandy soil, 60 or 80 feet above the water. I was surprized at seeing near the top of the cliff, some boys, tied with ropes, hanging down, and searching, as we were informed, the holes in the cliff for swallows, or martins, which took refuge in them, and lodged there all the winter, until warm weather, and then they came abroad again.

The boys being let down by their comrades to the holes, put in a long rammer with a screw at the end, as is used to unload guns, and, twisting it about, drew out the birds. For a trifle I procured some of them. When I first had them they seemed stiff and lifeless. I put one in my bosom, between my skin and shirt, and laid another on a board, the sun shining full and warm upon it. That in my bosom revived in about ¼ of an hour; feeling it move, I took it out to look at it, and saw it stretch itself on my hand, but perceiving it not sufficiently come to itself, I put it in again: in about another quarter, feeling it flutter pretty briskly, I took it out and admired it. Being now perfectly recovered, before I was aware, it took its flight, but the covering of the boat prevented me from seeing where it went: the bird on the board, though exposed to

a full sun, yet, I presume from a chilliness in the air, did not revive to be able to fly.

Remarks by Mr. Collinson.—What I collect from this gentleman's relation is, that it was the practice of the boys, annually to take these birds, by their apparatus and ready method of doing it; and the frequency of it was no remarkable thing to the watermen. Next it confirmed my former sentiments, that some of this swallow-tribe go away, and some stay behind in these dormitories all the winter. If my friend had been particular as to the species, it would have settled that point.

XXV. The Properties of the Mechanic Powers Demonstrated; with some Observations on the Methods that have been commonly used for that Purpose. By Hugh Hamilton, D.D., F.R.S.* p. 103.

This paper may be read with more advantage in an improved edition of it given in this author's ingenious volume of Philosophical Essays, the 3d edition of which was published in the year 1772.

XXVI. On some Subterraneous Apartments, with Etruscan Inscriptions and Paintings, discovered at Civita Turchino in Italy. By Joseph Wilcox, Esq. F.S.A. p. 127.

Civita Turchino, about 3 miles to the north of Corneto, is a hill of an oblong form, the summit of which is almost one continued plain. From the quantities of medals, intaglios, fragments of inscriptions, &c. that are occasionally found here, this is believed to be the very spot, where the powerful and most ancient city of Tarquinii once stood; though at present it is only one continued field of corn. On the south-east side of it runs the ridge of a hill, which unites it to Corneto. This ridge is at least 3 or 4 miles in length, and almost entirely covered by several hundreds of artificial hillocks, called by the inhabitants Monti Rossi. About twelve of these hillocks have at different times been opened; and in every one of them have been found several subter-

* Dr. Hamilton was born in 1728, and died Dec. 1, 1805; consequently at 77 years of age. He was educated at Trinity College, Dublin, where he obtained a fellowship, and was professor of Natural Philosophy. He afterwards received promotion in the church: first as dean of Armagh; then in Jan. 1796 he was elected bishop of Clonfert; and 3 years after he was translated to the see of Ossory; being preferred to those dignities, without solicitation, from his high character for piety, learning, and attention to the duties of his profession. His writings, too, in several branches of science, entitle him to rank among the highest ornaments of the university of which he was a member. The above is his only communication in the Philos. Trans. His other scientific publications, are the Philosophical Essays, above-mentioned; and a treatise on Conic Sections, in 4to. 1758; being probably the most elegant work ever produced on that subject.

ranean apartments cut out of the solid rock. These apartments are of various forms and dimensions: some consist of a large outer room, and a small one within; others of a small room at the first entrance, and a larger one within: others are supported by a column of the solid rock, left in the centre, with openings on every part, from 20 to 30 feet. The entrance to them all is by a door of about 5 feet in height, by 2 feet and a half in breadth. Some of these have no other light but from the door, while others seem to have had a small light from above, through a hole of a pyramidical form. Many of these apartments have an elevated part that runs all round the wall, being a part of the rock left for that purpose. The moveables found in these apartments consist chiefly in Etruscan vases of various forms; in some indeed have been found some plain sarcophagi of stone with bones in them. The whole of these apartments are stuccoed, and ornamented in various manners: some indeed are plain, but others, particularly 3, are richly adorned; having a double row of Etruscan inscriptions running round the upper parts of the walls, and under it a kind of freize of figures in painting: some have an ornament under the figures that seems to supply the place of an architrave. There have been no relievos in stucco hitherto discovered. The paintings seem to be in fresco, and are in general in the same stile as those usually seen on the Etruscan vases: though some of them are much superior perhaps to any thing as yet seen of the Etruscan art in painting. The paintings, though in general slight, are well conceived, and prove that the artist was capable of producing things more studied and more finished: though in such a subterranean situation, almost void of light, where the delicacy of a finished work would have been in a great measure thrown away; these artists (as the Romans did in their best ages, when employed in such sepulchral works) have in general contented themselves with slightly expressing their thoughts. But among the immense number of those subterranean apartments which are yet unopened, it is to all appearance very probable that many, and many paintings and inscriptions, may be discovered, sufficient to form a very entertaining, and perhaps a very useful work: a work which would doubtless interest all the learned and curious world, not only as it may bring to light, if success attends this undertaking, many works of art, in times of such early and remote antiquity, but as perhaps it may also be the occasion of making some considerable discoveries in the history of a nation, in itself very great, though to the regret of all the learned world at present almost unknown. This great scene of antiquities is almost entirely unknown even in Rome. Mr. Jenkins, then resident at Rome, was the first and only Englishman who ever visited it.

XXVII. Of a new Peruvian Plant, lately introduced into the English Gardens; the several Characters of which differ from all the Genera hitherto described. By George Dionysius Ehret, F. R. S.* p. 130.

This plant blowed in the Physic Garden at Chelsea, and flourished there in great perfection in the year 1761. It produced abundance of branches, which spread themselves on the surface of the ground: these branches were greatly multiplied by side ones, which grow alternately; and are smooth towards the ground, and streaked towards the top. Each joint is furnished with many ovate-shaped leaves, having membranous ciliated footstalks. This plant was also richly ornamented with abundance of buds and flowers: the flowers being of a sky-blue, with a dark embroidered purple bottom, made a beautiful appearance.

Mr. Philip Miller proposed to honour this plant with the name of Walkeria, in gratitude to Dr. Richard Walker, who, by his indefatigable pains, and at a large expence of his own, had founded a Physic Garden in the University of Cambridge, to incite and extend the study of Botany there.

XXVIII. Observations on two Ancient Roman Inscriptions discovered at Netherby in Cumberland. By the Rev. John Taylor, LL.D. p. 133.

The inscriptions were discovered at Netherby in Cumberland, the former in the year 1762, the other early in the present century: they both make mention of Marcus Aurelius Salvius, tribune of the cohors prima ælia Hispanorum milliaria equitata. The former also points out the particular emperor, M. Aurelius Severus Alexander, in whose reign it was engraved: and almost directs us to the very year; which must have been either the 226th or 229th of the Christian æra, for in those two years was that emperor consul: and one of those consulates this stone alludes to, in the last words of it; which Dr. T. reads thus:

IMPERATORE DOMINO NOSTRO
SEVERO ALEXANDRO PIO
FELICE, AVGVSTO, CONSVLE.

The purport of the inscription now under consideration is this, viz. In the reign of Severus Alexander, Pius, Felix, &c. the cohors prima ælia Hispanorum milliaria equitata put the finishing hand to a building, termed here Basilica equestris exercitatoria, the foundations of which had been laid some time before. This was conducted under the care and direction of Valerianus, the emperor's lieutenant and pro-prætor, at the instance of M. Aurel. Salvius, tribune of the aforesaid company. Basilica is a word of large extent, and commonly signifies what is built for public use, or by public authority. It is therefore frequently

* This plant is the *Nolana prostrata* of Linnæus.

applied to a burse or exchange. The public roads are termed basilicæ: and the Christian writers took this word for their churches. Though this be the common use of the word, it is not the primary. It signifies originally and principally as it does in this inscription, a portico or colonnade, which being very large and considerable in places built for courts of justice, for public auditories and meetings of merchants, it came to pass that the name of the principal was sunk in the adjunct; and all these places called alike basilicæ, from the colonnade, which attended, and perhaps sometimes encompassed them.

XXIX. A Method of Lessening the Quantity of Friction in Engines. By Keane Fitzgerald, Esq., F. R. S. p. 139.

Mechanics, or that branch of mathematics which considers motions and moving powers, their nature and laws, is properly distinguished into rational and practical. A knowledge in rational mechanics, which comprehends the whole theory of motion, on which natural philosophy so greatly depends, is chiefly confined to the learned; and the proper construction of engines and machines, which is the principal object of practical mechanics, though so very necessary to carry on the several branches of husbandry, manufacture, and commerce, on which the riches and power of a nation in a great measure depend, is seldom attended to, but by the mere handicraftsman; who is little acquainted with the principles he works on, and from whom no great improvements can well be expected; yet it has sometimes happened, that excellent contrivances have been invented, for raising heavy weights, and overcoming their resistances, by persons who never took the trouble of examining into the cause of gravity. It often happens that mechanical powers, seemingly demonstrable in theory, are found very deficient in operation, from unexpected obstructions; which, with the expence and trouble that generally attend the reducing speculations of this nature into practice, have probably been the greatest obstacles to improvements in it. One of the greatest obstructions to the mechanical powers of engines proceeds from the friction, or resistance of the parts rubbing on each other; which in general is greater or less, as the rubbing parts bear the greater or less pressure; and yet this obstruction is but little attended to. The theorist makes no allowance on account of friction; and the practical mechanician, who feels the effects, yet, as if unavoidable, seldom takes the trouble of searching for a remedy. Among the few who have endeavoured to ascertain the quantity of friction proceeding from weight, some have deemed it equal to $\frac{1}{3}$, others to $\frac{1}{2}$, and others more or less, according to their different methods, or accuracy in making experiments. Doctor Desaguliers gives an account of some experiments, which show the quantity of friction in a cylinder, to be equal to $\frac{2}{3}$ of the power required to move it, when the surface of the cylinder moves as fast as the power.

In order to examine the quantities of friction proceeding from different weights, Mr. F. had an exact balance made, which weighed 27 oz.; the pivots of the axis were $\frac{1}{2}$ inch diameter, and turned in brass sockets, fixed in a frame for the purpose. At each end of the beam he suspended several pairs of equal weights, and then tried how much added at one end would just move the beam, and also how much would depress the end 2 inches. Whence he infers, that the least power required was equal to $\frac{1}{4}$ the weight on the pivots; and that it required a power nearly equal to the whole weight, to overcome the resistance from friction with but a small degree of velocity.

It is not imagined that these experiments should determine the exact quantity of friction proceeding generally from weight or pressure: which probably can never be ascertained by any experiments, however accurate; for even in engines of equal dimensions, and loaded with equal weights, the quantities of friction may be very unequal, from circumstances differing, which are sometimes imperceptible; such as the firmness, elasticity, roundness and smoothness of the parts rubbing on each other; particularly the roundness, and smoothness of the gudgeons or pivots, which in large engines are seldom turned true, or polished. But it appears from these experiments, that the quantity of friction in large engines may reasonably be estimated at $\frac{1}{2}$ the weight, or pressure, on the rubbing parts; though in such as are small, and finished with exactness, the quantity may probably be about $\frac{1}{3}$.

Mr. F. then endeavours to determine how much friction will be lessened by placing the axis of a wheel on the circumferences of two other, or friction wheels; and also the pivots of these two each on two others, &c.; calculating the diminution of the friction on the ratio of the decrease of velocity in each wheel or axis; hence he says, thus it is evident that by the application of additional wheels, or by enlarging the diameters of these, the resistance from friction may be reduced to less than the resistance of the medium the wheel passes through.

In order to form some estimate of the quantity of weight with which the axis of the lever of a fire-engine is loaded, Mr. F. took the dimensions of the several parts of that at the York-buildings water-works; the lever of which is 27 feet long, 2 feet 6 inches by 2 feet 2 inches in the middle, and 2 feet by 22 inches at the ends. The weight of which, with the archeads, chain, rods, and working frame hanging at one end, and the piston and chain at the other, may be computed at 6 tons, or 12,000 lb. The cylinder is 45 inches diameter, about 1591 square inches; which at 15 lb. per inch pressure of the atmosphere, is 22,274 lb. The column of water to be raised is 10,060 lb., which is not $6\frac{1}{2}$ lb. per inch; so that the remainder of the power is employed in overcoming the resistance from friction in the several parts of the engine, and giving the lever a degree of velocity equal to 120 feet per minute, which it moved in common work.

Mr. F. takes occasion to mention what he calls a general error in the manner of placing the axis of the lever under the beam. A balance, having its centre of motion underneath, and equal weights at each end, being placed horizontally, will remain in that position; as both weights are equidistant from the centre of gravity, which is perpendicular to the centre of motion; but when it is made to incline to either side, it will continue to move on that side, till it becomes parallel to the horizon, with the centre of motion above the balance: for when either end is depressed in the least degree, it becomes more distant from the centre of gravity; and the opposite end, which is raised in proportion, is brought nearer to it, though both ends still continue equidistant from the centre of motion. The lever of this engine is 2 feet 9 inches from the upper part of the beam to the centre of its axis placed underneath; and weighs with its arch-heads about 5 tons. When it was placed in a horizontal position, it required but $93\frac{1}{2}$ lb. to overcome the resistance from friction in the pivots; but when either end was depressed 4 feet below the level, at which distance the springs are fixed, it required 534 lb. to be applied to the opposite end to bring it back again: so that a power $= 440\frac{1}{2}$ was required, on account of the centre of gravity being so much changed by the position of the axis underneath.

To avoid this general error, Mr. F. had the axis placed on the upper side of the lever, and fixed by proper bolts and screws to a bar of iron equally strong, placed underneath: and in order to reduce the quantity of friction, which is in proportion to the space rubbing on a dead surface in equal time, he had them made in a form, by which they are equally strong, though the rubbing part is but $1\frac{1}{2}$ diameter; so that by changing only the form of the pivots, the friction is reduced to $\frac{1}{4}$ of its original quantity. He applied two quadrants, or friction-arches, to each of these pivots, whose radii are 2 feet 6 inches, by which the whole friction of the pivots of the axis of the lever, are transferred to the pivots of the quadrants, which are $1\frac{1}{2}$ inch diameter. These quadrants are equal in effect to wheels 5 feet diameter; the radius of which is $\frac{40}{1}$ to the semidiameter of its pivot, and reduce the friction in the pivots of the quadrants to $\frac{1}{40}$th part of what it was in the pivots of the axis; which multiplied by $\frac{4}{1}$, the reduction made by changing the form of the pivots, gives $\frac{160}{1}$: by which means the friction that was in the pivots of the great axis, which was $= 425$ lb., is reduced to $\frac{1}{160}$, or somewhat less than $2\frac{3}{4}$ lb. On trial, the lever, that before required a power of 95 lb. to overcome the least resistance from friction, was as easily effected by the application of $\frac{3}{4}$ pound; and the resistance from friction occasioned by a weight of 6 tons is of so little consequence, that the lever may be swung with a slight thread, and will continue in a state of vibration for several minutes after. The original quantity of friction in the pivots of the lever A, which when loaded with its full weight 22 tons, required a power $= 425$ lb. to overcome its resist-

ance, is by this method reduced to 2lb. 10oz.; and if there were any need of reducing it further, it might be done by applying two small quadrants to each pivot of the larger, which would reduce it to 1 oz. or less.

It is not easy to determine the quantity of friction that was in the plug frame: but that has also been reduced to $\frac{1}{16}$ by the application of several rollers 5 inches diameter, whose pivots are $\frac{1}{4}$ inch diameter, on which it now moves. But it is evident that a power $= 440\frac{1}{2}$ has been saved by changing the position of the axis of the lever; and a power of 421 lb. 6 oz. by reducing the quantity of friction in the pivots.

The visible effect, with respect to the working of the engine, according to the most exact observations by different persons, both before and after these several alterations were made, is that it now makes 18 strokes at 8 feet per stroke, for 15 that it ever made, with the same, or rather a smaller quantity of fuel; and must therefore discharge $\frac{1}{6}$ more water in equal time; which consequently saves $\frac{1}{6}$ of the fuel. But the effect is found still greater, as to supplying the tenants with water; for the engine performs the same service better now in 5 hours than ever it did before in 6; which can only be accounted for by the extraordinary regularity of its stroke, which does not abate of its full length suddenly, as it used to do when the strength of the fire abated: this Mr. F. takes to be occasioned in a great measure from placing the axis above the lever, by which the centre of gravity becomes reversed to what it was before; so that it requires the same power to keep the end of the lever depressed as low as the springs, that it required before to bring it back, when so much depressed; which is a particular benefit; for the stop or set, generally in large engines, when the ends of the lever come to the springs, is a defect that has been endeavoured to be remedied in some degree by the help of the springs. But when the axis is placed above the lever, and the friction reduced, as above, if one end is brought down to the springs, and let to return, it carries the other end down to the springs without any resistance, and will continue to do so several times, abating somewhat of the length of the stroke each time.

This engine, from several improvements that have been made in the boiler, consumes but 4 bushels of coals in an hour; which is deemed $\frac{1}{3}$ less than others of equal size; and it performs the same work now in 20 hours that it did before in 24 hours, which is a saving in effect of 16 bushels in 24 hours, amounting to 162 chaldrons in a year's constant work; which is a very considerable article, even where coals are to be had at a cheap price.

XXIX. The Difference of Longitude between the Royal Observatories of Greenwich and Paris, determined by the Observations of the Transits of Mercury over the Sun in 1723, 1736, 1743, *and* 1753. *By James Short, M.A., F.R.S.* p. 158.

It will doubtless appear surprizing, Mr. S. says, that he should attempt to determine the difference of longitude between two of the most celebrated observatories in Europe; and in which some of the greatest astronomers that ever lived, have for above 80 years been constantly observing the motions of the heavenly bodies: yet it is most certain that to this day we are ignorant of the said difference of longitude; the English astronomers reckoning it to be = $9^m\ 20^s$, and the French setting it down at $9^m\ 10^s$, which they tell us was found by M. Cassini, by observations of the eclipses of Jupiter's first satellite made by him while in London in the year 1698: we are no where told by what observations the English astronomers have fixed this difference at $9^m\ 20^s$.

In the Memoirs of the Royal Academy of Sciences at Paris for the year 1734, there is an account given of 33 corresponding observations of the eclipses of the first satellite of Jupiter, made at Greenwich and Paris, from the year 1677 to the year 1701: the mean of these 33 observations gives the difference of longitude between Paris and Greenwich = $9^m\ 29^s$.

I have calculated, says Mr. S., and it may be demonstrated, that if we compare the observations of the late transit of Venus made at Greenwich, and by M. de la Lande at Paris, and suppose that the difference of longitude between these 2 places is = $9^m\ 25^s$, it will follow that the sun and Venus are at an infinite distance, which is absurd. Again, if we suppose the difference to be greater, it will follow that the sun and Venus are more than infinitely distant, which is likewise absurd. We are therefore certain, if these observations are to be depended on, that the difference of longitude between Greenwich and Paris is less than $9^m\ 25^s$. If we compare the observations made at Savile-house, with the same observation by M. de la Lande at Paris, and reason in the same manner, we shall find that the difference of longitude between Greenwich and Paris must be less than $9^m\ 33^s$. Thus far then a limit one way is fixed for the difference of longitude between these 2 places.

The late transit of Venus was the only one which had ever been observed at Greenwich and Paris, and by comparing the observation at Greenwich with that made by M. de la Lande at Paris, the difference of longitude comes out = $9^m\ 8^s$. And if we compare the observations at Savile-house (30^s of time west of Greenwich) with that of M. de la Lande,* the said difference of longitude comes out

* M. de la Lande saw the internal contact of Venus with the sun's limb .... at $8^h\ 28^m\ 25^s$
Pere Clouet 8 28 26

$= 9^m\ 16^s$. Since then we have only this one transit of Venus, by which we can determine this difference of longitude, we must have recourse to the transits of Mercury, of which there have been 4 since the year 1723, observed at London, at Greenwich, and at Paris. I have therefore extracted from the Phil. Trans., and the Memoirs of the Royal Academy at Paris, the several observations of the 4 transits of Mercury over the sun in the years 1723, 1736, 1743, and 1753.

The observations in the year 1723 were made by Dr. Halley at Greenwich, by Dr. Bradley at Wansted, and by Mr. George Graham at London, by Messrs. Cassini, Maraldi, and De l'Isle at Paris. Those in the year 1736 were made by Dr. Bevis at Greenwich, and by Messrs. Cassini and Maraldi at Paris. Those in the year 1743 were made by Messrs Cassini, Maraldi, Le Monnier and de la Caille at Paris, and by Dr. Bevis and myself at Mr. Graham's house in Fleet-street, London. Those in the year 1753 were made by Messrs. Cassini, Bouguer, De l'Isle, Merville, Libour, Le Gentil, and De la Lande at Paris, and by Dr. Bevis and myself in Surry-street, London. By means of these observations, Mr. S. got no less than 63 determinations of the difference of longitude between the royal observatories of Greenwich and Paris, and having corrected them by parallax, they are as follow:

1723.
By the internal contact at ingress observed by Dr. Halley.

M. Cassini .... =	9^m	23^s
De l'isle........ =	9	14
De l'isle........ =	9	14
Maraldi........ =	9	23
Dr. Bradley.		
De l'isle........ =	9	12
Cassini .... =	9	21
Maraldi .... =	9	21
De l'isle........ =	9	12
Mr. Graham.		
De l'isle........ =	8	56
Cassini .... =	9	5
Maraldi........ =	9	5
De l'isle .... =	8	56
12)	110	22
Mean of these 12..	9	12

1736.
By the external contact at egress observed by Dr. Bevis.

M. Maraldi .... =	9^m	37^s
Cassini, jun. .... =	9	44
Cassini, sen. .... =	9	14
3)	28	35
Mean of these 3 ..	9	31

1743.
By the internal contact at egress observed by Dr. Bevis.

M. De la Caille.... =	9^m	4^s5
Maraldi ...... =	9	18·5
Le Monnier.... =	8	53.5
Cassini, sen.... =	9	33.5
Cassini, jun.... =	9	27.5
5)	46	17.5
Mean of these 5..	9	15.5

1743.
By the external contact at egress observed by Dr. Bevis.

M. De la Caille.... =	9^m	16^s5
Maraldi .... =	9	36.5
Le Monnier.... =	9	23.5
Cassini, sen.... =	9	20.5
Cassini, jun.... =	9	42.5
5)	47	19.5
Mean of these 5..	9	27.9

1743.
By the internal contact at egress observed by myself.

M. De la Caille.... =	8^m	57^s5
Maraldi .... =	9	11.5
Le Monnier.... =	8	46.5
Cassini, sen ... =	9	26.5
Cassini, jun.... =	9	20.5
5)	45	42.5
Mean of these 5..	9	8.5

M. Meffier .... at	8^h	28^m	27^s
M. Ferner	8	28	29
M. de la Caille	8	28	$37\frac{1}{2}$
M. Maraldi	8	28	42

Since therefore the observations of Messrs. Maraldi and de la Caille differ so much from the observations of the first four gentlemen (who agree very neary together) it is plain that they ought to be rejected; and indeed M. de la Caille says, in a letter to Dr. Bevis, that the telescope he observed with was a bad one, and consequently his observation not to be depended on. M. de la Lande says the same in a letter to Mr. Maskelyne, read at the Royal Society.—Orig.

1743.
By the external contact at egress observed by myself.

M. De la Caille.... =	9^{m}	$18^{s}5$
Maraldi........ =	9	38.5
Le Monnier..... =	9	25.5
Cassini, sen.... =	9	22.5
Cassini, jun.... =	9	44 5
5)	47	29.5
Mean of these 5..	9	29.9

1753.
By the internal contact at egress observed by Dr. Bevis.

M. Cassini........ =	9^{m}	$25^{s}5$
Bouguer ...... =	9	6.5
De l'isle ...... =	9	5.5
Merville ...... =	9	1.5
Libour......... =	9	0.5
Le Gentil...... =	9	9.5
De la Lande.... =	9	3.5
7)	63	52.5
Mean of these 7..	9	7.5

1753.
By the external contact at egress observed by Dr. Bevis.

M. Cassini........ =	9^{m}	$26^{s}5$
Bouguer ...... =	8	57.5
De l'isle ...... =	9	7.5
Merville ...... =	9	19.5
Libour......... =	9	30.5
Le Gentil...... =	9	26.5
De la Lande.... =	9	25.5
7)	65	13.5
Mean of these 7 ..	9	19

1753.
By the internal contact at egress observed by myself.

M. Cassini........ =	9^{m}	$18^{s}5$
Bouguer ...... =	8	59.5
De l'isle ...... =	8	58.5
Merville ...... =	8	54.5
Libour......... =	8	53.5
Le Gentil...... =	9	2.5
De la Lande.... =	8	56.5
7)	63	3.5
Mean of these 7..	9	0.5

1753.
By the internal contact at egress observed by myself.

M. Cassini....... =	9^{m}	$22^{s}5$
Bouguer ...... =	8	53.5
De l'isle ...... =	9	3.5
Merville ...... =	9	15.5
Libour......... =	9	26.5
Le Gentil...... =	9	22.5
De la Lande ... =	9	21.5
7)	64	45.5
Mean of these 7..	9	15.1

The mean of the above 10 mean is.............................. = 9^{m} 16.7^{s}
The mean of the above 63 results of the difference of longitude between Greenwich and Paris is .... = 9 15
The mean of 43 results which differ not more than 15^{s} from the mean of the whole is .... = 9 16
The mean of 19 results which differ less than 15^{s}, and more than 8^{s} from the mean of the whole, is .... = 9 14.2
The mean of 24 results which differ less than 8^{s} from the mean of the whole is..................................... = 9 17.5
The mean of the above 5 means is.......................... = 9 15.8

And even the mean of those 20 results which differ more than 15^{s} from the mean of the whole, and which are rejected, gives the said difference = 9^{m} $12\frac{1}{2}^{s}$, which differing only $3\frac{1}{2}^{s}$ from the 43 results, is a proof of the great accuracy in the determination of the differences of longitudes by observations of the transit of Mercury over the sun.

Let us now examine the limit of the errors in these 10 several sets of determinations, and we shall find that the limit of the errors in the year

1723 is = 27^{s} by the internal contact at ingress.
1736 is = 30 by the external contact at egress.
1743 is = 40* by the internal contact at egress.

* If we reject the observations of M. le Monnier, in which there seems to be some mistake, because it differs considerably from the rest, the limit of the error will be = 29^{s}, agreeing nearly with the other limits.—Orig.

1743 is = 26 by the external contact at egress.
1753 is = 25 by the internal contact at egress.
1753 is = 33 by the external contact at egress.

Hence we may safely conclude, that the difference of longitude between any two places may be determined by one single observation of the contact of Mercury with the sun's limb, made at each place, so that the error in the determination will not exceed 30^s of time from the truth; whereas in the above 33 observations of the eclipses of the first satellite of Jupiter, we find the limit between the errors to amount to $3^m\ 44^s$ of time. If we take a mean of the said observations of the first satellite, the difference of longitude between Greenwich and Paris is = $9^m\ 29^s$, and if we reject those which differ the most from the rest, the mean of the remaining 25 observations gives the said difference = $9^m\ 40^s$, and the mean of those 8 observations which are rejected, gives the said difference = $8^m\ 53^s$, both which last determinations can be proved to be very far from the truth by the observations of the late transit of Venus; for by the said observations of Venus it appears, that the difference of longitude between Greenwich and Paris cannot exceed $9^m\ 33^s$, as before said; and if the said difference is = $8^m\ 53^s$, then the parallax of the sun, by the Savile-house observation compared with that of M. De la Lande at Paris, would amount to 20^s, which we are sure it cannot be.

On the whole therefore we may conclude, that the difference of longitude between the Royal Observatories of Greenwich and Paris, as determined by 63 observations of the contact of Mercury with the sun's limb made at each place, is = $9^m\ 16^s$. This determination would have been perhaps more decisive, if Mr. S. could have had recourse to the books containing the observations of the late astronomer royal, Dr. Bradley. Observations! made by one of the greatest astronomers, and by the best and most accurate observer, assisted by the best and most accurate instruments, which are in any observatory; but alas! the public are hitherto deprived of the use of these most excellent observations.*

In a former paper which Mr. S. gave into the R. S., concerning the parallax of the sun, he assumed the difference of longitude between Greenwich and Paris to be = $9^m\ 10^s$; and as the determination of this difference is now more certain by the transits of Mercury above-mentioned, being found = $9^m\ 16^s$; and as this difference of longitude will make some small difference in the result of the said

* On Thursday following, being the 9th of June, a motion was made, at the meeting of the R. S. by the Rev. Nevil Maskelyne, F. R. S. and unanimously agreed to, recommending it to their council, as visitors of the Royal Observatory, to take proper measures for obtaining and securing the astronomical observations that have been made there in times past, for the benefit of the public; it was also agreed on to publish them, when obtained, at the expence of the Society; and for the future, to publish the observations made at the Royal Observatory annually, in the Philosophical Transactions.—Orig.

parallax from the observations made at all those places* which are to the east of Greenwich, where the late transit of Venus was observed; he has therefore computed them again, and they are as in the following synoptic table.

The time of the internal contact of Venus with the sun's limb observed at the Cape of Good Hope compared with that at

	Sun's parallax		Sun's parallax		Sun's parallax
Greenwich	8.42″	Bologna	8.54″	Hernosand	8.78″
Shirburn castle	8.15	Rome	8.74	Calmar	8.97
Savile house	8.57	Drontheim	8.33	Abo	8.68
Leskeard	8.69	Upsal	8.60	Tornea	8.09
Paris	8.54	Stockholm	8.59	Cajaneburg	8.43

By the mean of these 15 results, the sun's parallax on the day of the transit . . . . = 8.54

And if we reject the 2d, 11th, 12th, and 14th, which differ the most from the rest, the mean of the remaining eleven gives the sun's parallax . . = 8.56

Therefore the mean horizontal parallax of the sun is . . . . = 8.69

XXX. Of a Remarkable Fish,† *taken in King-Road, near Bristol. By Mr. James Ferguson.* p. 170.

This fish fought violently against the fishermen's boat, after they got it in their net, and was killed with great difficulty. Its length is 4 feet 9 inches, and its thickness in proportion. The mouth is a foot in width, and of a squarish form; it has 3 rows of sharp small teeth, very irregularly set; and at some distance from each other; it has no tongue, nor narrow gullet, but is all the way down, as far as one can see, like a great hollow tube; in the back of the mouth within, there are two openings like nostrils; and about 9 inches below the jaw, and under these openings, are 2 large knobs, from which proceed several short teeth; a little below which, on the breast side, is another knob with such teeth. On each side within, and about a foot below the jaws, there are 3 cross ribs, somewhat resembling the straight bars of a chimney grate, about an inch distant from each other; through which we see into a great cavity within the skin, towards the breast; and under the skin, these cavities are kept distended by longitudinal ribs, plain to the touch on the outside. Mr. F. put his arm down through the mouth, quite to his shoulder, but could feel nothing in the way; so that its heart, stomach, and bowels, must lie in a very little compass near its tail, the body thereabout being very small. From the neck proceed 2 long horns, hard

* Because the longitudes of all those places were taken from the Connoissance des Temps, and the Swedish Acts, in which their differences of longitude from Paris are marked down.—Orig.

† The species of *Lophius* here described is the *Long Angler* of Pennant. *Lophius Cornubicus*, Shaw, General Zoology. *La Lophie Ferguson*, Cepede.

and very elastic, not jointed by rings as in lobsters, and on each side of the back there are 2 considerable sharp edged risings, of a black and long substance. Between each eye and the breast, there is a cavity somewhat like the inside of a human ear; but it does not penetrate to the inside. From each shoulder proceeds a strong muscular fin, close by which, towards the breast, is an opening, through which one may thrust his hand and arm quite up through the mouth; and between these fins proceed from the breast 2 short paws, somewhat like the fore half of a human foot, with 5 toes joined together, having the appearance of nails. Near the tail are 2 large fins, one on the back, the other under the belly. The skin is of a dark brown colour, but darker spotted in several places, and entirely without scales.

XXXI. Rules and Examples for Limiting the Cases in which the Rays of Refracted Light may be Reunited into a Colourless Pencil. By P. Murdoch, M.A., F.R.S. p. 173.

Let SO, pl. 17, fig. 1, a small pencil of the solar light, pass through the refracting medium ABCD, whose opposite surfaces, represented by AB, CD, are parallel planes: then the violet rays OV, will, in the 2d refraction into the air, emerge parallel to the red; for both will be parallel to the incident ray SO, and consequently to each other; that is, V*v* will be parallel to R*r*, as is plain from the common principles of optics. If the light after its emergence be received on a screen, placed any where beyond RV, it will be tinged with violet on the side V*v*, and with red towards R*r*: and if the incident pencil SO be exceedingly small, all the intermediate colours will be seen in the same order as when light is refracted by a prism. But if the incident pencil is not very small, or if the luminous body, from which the rays are transmitted through a small opening at O, has a considerable breadth, like that of the sun's disk; then so many rays of every kind will mix towards the middle of the spectrum as to produce a pure white; but at the extremities V*v* and R*r*, it will still be tinged with violet and red; for a violet ray from the uppermost point of the sun's disk will be more refracted than the other can be; and a red ray from the lowest part of his disk, will be less refracted than any other. If BC, the distance of the refracting surfaces, be increased or diminished, RV, the distance of the extreme rays, will be increased or diminished in the same proportion; and if RV approach very near to the aperture O, the colours will become imperceptible.

To reunite these rays, we may place another medium of the same refractive power, and of the same thickness (bc = BC) as in the figure; so as the rays V*v*, R*r*, &c. may enter its surface *cd* at the same angle as they emerged at from CD, or as SO entered AB; and after refraction at the point *o* of the surface *ab*, to which they converge, they will be reunited into *os* the continuation of SO, in a

pencil every way like the incident pencil so, excepting that the light will have been somewhat weakened in its passage through the media.

Other things remaining, let the thickness of the 2d medium be *cp*, less than *cb* or CB, the surface parallel to *cd* being *pe*; and the emergent rays $\omega\sigma$ will be indeed parallel to the incident as formerly, but the spectrum will fall below the place of the screen where so or *os* would fall. It will likewise be coloured, as the rays were not yet united at the point *o*. If the thickness be greater than *cb*, the spectrum will fall above the line so*os*, and the violet and red, after their intersection in *o*, will have changed sides.

Other things remaining, suppose the refractive power of the medium *ac* to be increased, making the extreme rays to intersect before they reach the surface *ab*; in that case, let the medium be turned round an axis perpendicular to the plane of refraction (represented by the plane of the figure) in the order of the letters *a*, *b*, *c*, so that the angle of incidence of the rays V*v*, R*r*, the line *vr*, and the angle *vor*, may be continually decreasing, till the intersection *o* falls into the side *ab*: and the rays will emerge colourless and parallel to the incident pencil so; above, or below, or in the line so*os*, according to the assumed place of the axis of revolution.

If, on the contrary, the refractive power of the medium *ac* be diminished, and, with it, the angle of convergence of the extreme rays; the point where they would intersect falling beyond the surface *ab*; the medium must then revolve the contrary way, in the order *c*, *b*, *a*, to bring the point of intersection to the surface *ab*. But if the refractive power be so small that even when *cd* becomes almost coincident with V*v*, the point of intersection falls still beyond *ba*, in that case the rays cannot be made to emerge colourless, otherwise than by increasing the depth of the medium till its surface passes through the point of intersection. And in like manner, when the refractive power of the second medium *ac* is greater than that of AC, making the rays to meet within the medium, as at *q* a point in the line *pe*; we may, instead of turning the medium round an axis, cut off the part *pa*, leaving the surface *pe* parallel to *cd*; and the emergent light will be colourless.

From these few principles we may determine the phenomena of light transmitted through parallelopipids that are contiguous to the air, their position and refractive powers being given. Or we may dispose them so that the emergent light shall, or shall not, be tinged with colours. And we already see (what shall be more distinctly explained below) that if light be transmitted through whatever number of media (A, B, C, &c.) all the refractions may be corrected by the equal and contrary refractions of the same number of the same media (*c*, *b*, *a*,) similar and similarly situated to the former; provided there be a medium Z interposed

between the two series, thus; A, B, C, Z, c, b, a; and that the rays, in their passage through Z, are parallel to one another.

But to give the rays this parallelism in their passage through Z, and to explain the several phenomena of refracted light, we shall need the following

LEMMA, *a Problem.* Given, in fig. 2, DCB the difference of two angles ACD, ACB, and the ratio of DI the sine of the greater to BH the sine of the lesser being also given, to find the angles.—For DF, the sine of the given difference, write s, and for its cosine CF write c; for the lesser sine BH, the letter z, and let the given ratio of DI to BH, be that of m to n, the radius CB being unity. Then, having drawn FG perpendicular to DI; from the similar triangles in this figure, we shall have CB : CH : : DF : DG, or $1 : \sqrt{1-z^2} : : s : \text{DG} = s\sqrt{1-z^2}$; and CB : BH : : CF : GI, or $1 : z : : c : \text{GI} = cz$. But (by hypoth.) DI : BH : : $m : n$; that is DG + GI, or $s\sqrt{1-z^2} + cz : z : : m : n$; which gives $\sqrt{1-z^2} : z$, or CH : BH, or 1 : tang. ACB : : $m - nc : ns$; that is, tang. ACB $= \frac{ns}{m-nc}$. In words —multiply the sine of the given difference by the least term of the given ratio for a dividend: from the greater term subtract the product of the cosine of the difference and the lesser term for a divisor; and the quotient shall be the tangent of the less angle ACB.—Or, if you prefer a geometrical construction; in the semidiameter CB produced take CM to CB as DI to BH; and in the tangent to the circle at B, make BL to BC, as DF to FM, and BCL shall be the less angle sought. Or you need only join DM and draw the semidiameter CA parallel to it.

But before we apply this solution, it may be proper to give a table of the refractive powers of glass, water and spirit of wine, whether contiguous to the air, or perhaps the fluids contiguous to glass: these being the substances in which experiments may be most conveniently made: and it is also necessary to know the limitations that arise from those several powers.

1. When light passes from air into glass, and the angle of incidence is near 90°, whose sine is unity;

The sine of the refraction of the red rays = $\frac{50}{77}$ is .6493508 = sin. 40° 29′ 33.6″
And of the violet $\frac{50}{78}$ = .6410256 = sin. 39 52 6
And their difference. 0 37 27.6,
is the greatest angle at which the violet and red rays can diverge in the refraction from air into glass, wanting very little of $37\frac{1}{2}$′. And when an unrefracted pencil passes from glass into air, as soon as the angle of incidence exceeds 39° 52′ 6″, the violet rays will begin to be reflected; and when the incidence exceeds 40° 29′ 33.″6 the rays will be totally reflected.

2. From Air into Water. The sine of refraction of the red is .7517905 = s. 48° 44′ 44″
Of the violet .7454080 = s, ... 48 11 39
And the greatest divergence. 0 33 5,
the angle of beginning reflection from water into air being 48° 11′ 39″.

3. From water into glass.

Sin. incid. : *s.* refr. of the red :: 1 : 0.863739 = *s,*	59°	44′	20″$\frac{1}{2}$
Of the violet :: 1 : 0.159966 = *s,*	59	18	45
The difference is the greatest divergence	0	25	35$\frac{1}{2}$

4. From air into spirit of wine.

Sin. incid. : *s.* refr. of the red :: 1 : 0.7334001 = *s.*	47°	10′	20″.2
Of the violet :: 1 : 0.7266366 = *s.*	46	36	18.6
The difference is the greatest divergence	0	34	1.6

5. From spirit of wine into glass.

Sin. incid. : *s.* refr. of the red :: 1 : 0.8853964 = *s,*	62°	18′	0″.1
Of the violet :: 1 : 0.8821802 = *s,*	61	54	24
And their difference is the greatest divergence	0	23	36

These numbers are partly transcribed from Sir Isaac Newton, and partly computed by a rule of Mr. Euler in the Phil. Trans. They are indeed carried on to more decimal places than the experiments hitherto made can well bear: but it is hoped that hereafter methods may be devised to measure the refractions of light to a very great degree of precision.

When a slender pencil SO, is refracted by the surface of a denser medium OT, fig. 3, the extreme rays being OV the violet, and OR the red; we have seen that the surface RVT, at which the rays pass again into the rarer medium, being parallel to the first surface OT, the extreme, and all the intermediate rays, will emerge parallel to each other, and to the pencil SO. But if the last surface RVT cut the former in a line perpendicular to the plane of refraction at the point T, on the side of the radiant point S, then the extreme rays, being refracted at the points V, R, will converge to some point F in the rarer medium: and if the light be received on a screen at F, it will be colourless; if nearer to the refracting medium, or farther from it, it will be tinged, but on different sides. Thus, if the denser medium be water, and the surrounding medium air; the angle of incidence LOS being 20°, the angle of divergence VOR will be 7′ 46″. And OVP the angle of incidence at the 2d refraction for the violet rays being taken of 30°, the angle of convergence RFY will be 14′ 26″.

On the contrary, if the plane VR*t*, fig. 4, which terminates the denser medium, cut the first refracting plane on the other side of the perpendicular OL, the rays will diverge from some point *f* on the other side of the 2d surface: the violet ray OV being more refracted from the perpendicular VP, than the red is from the perpendicular R*p*. And it is evident, that if the distance (OT or O*t*) of the point of incidence from the edge of a prism, the angle of incidence LOS, and the angle of the prism (OTV or OtV,) be given, together with the refractive powers of the media, the lines OV, OR, will be given in magnitude and position. And

thence, the distance VR being given, with the angles of refraction at the 2d surface, the points F or f, to which the rays converge, or from which they diverge, will be given. And their locus, or the curve in which all these points are found, may be assigned; whether the angle of the prism be constant, and the angle of incidence be variable, or the contrary; and whether the rays are refracted, or at a certain obliquity come to be reflected by the 2d plane.

If it be further required, that the extreme, and all the intermediate rays, which meet at F, in fig. 3, should thenceforth remain united in a colourless pencil: through the point of convergence F draw (by the lemma) the line ZX, making the angles ZFR, ZFV, such that their difference RFV being the given angle of convergence, their sines may be as the sines of refraction of the red and violet rays, when they pass from a given denser medium GKH into the air, at a common angle of incidence: and HFG, perpendicular to ZX, will be the line in which the surface of that medium must cut the plane of refraction, when the rays RF, VF, are refracted into the same line FN. And if the medium be terminated on the other side by any plane KN, to which FN is perpendicular, the pencil NY, continued in the air, will remain colourless. For instance, if the medium GK be glass, and the angle RFV be 14′ 26″, ZFR the angle of incidence of the red rays will be found of 17° 54′ 14″; and the angle of refraction XFN, common to all the rays, will be 12° 6′ 34″½. But if the plane HG, to which ZX is perpendicular, pass not through the point of reunion F, but on this or the other side of it; the rays in their passage through the medium, though parallel to each other, will be laterally separated.

Let a ray SOL, fig. 5, of a mean degree of refrangibility be refracted by AB, the side of a glass prism ABC, so that the refracted ray OM may be perpendicular to the side of the prism AC; it is required to apply to this, another prism of a differently refracting substance, as of water, so that the ray Mo being refracted at o, by the side DC, the refracted ray so may be parallel to OS. The angle of incidence SOP, and the refractive power of the glass, being given, the angle SOM, and its supplement LOM, be given, produce Mo to n; and because os is to be parallel to LO, take for the difference of the angles in the lemma, the given angle nos (= LOM), and through the point o draw rop, so that the sine of pon may be to the sine of pos, as the sine of incidence to that of refraction, when a meanly-refrangible ray passes from water into air; and DoC, perpendicular to rp, will be the position of the side required.

We have here supposed the ray SO to be homogeneous, of a mean refrangibility; but if it be a ray from the sun, the image at s will be very much tinged. The colours will have been separated at O; a small matter more at M, but they will diverge very considerably at o; for setting aside the refractions at O and M; that is, supposing a pencil Mo to pass unrefracted in water, till it fall on a sur-

face of air at an angle of incidence of about 47° 32′½, the divergence of the extreme rays will be about 2° 51′½: a small difference of sines answering to a considerable difference of the angles when they approach to 90°: the ultimate difference to which they converge, being (from water into air) 7° 26′¼.

Let a pencil of the solar light so, fig. 6, fall on the surface of water BC, the extreme rays being refracted into OV, OR; it is required to assign the glass prism PN*n* (whose section PN*n* is an isosceles triangle) such, that the base N*n* being parallel to so, and the surface of the water AC being inclined to the base N*n* in the same angle as the surface BC; the extreme rays, in their passage through the glass prism, shall be parallel; and all the rays shall emerge colourless in the line SO*os*; that is, in the incident ray produced through both the media. The angle SOB, and the refractions from air into water being given, the angles VON, RON, and their difference VOR, are given. Draw therefore, by the lemma, the line OG, making the sine of ROG to that of VOG, as the sine of refraction of a red ray, in passing from glass into water, is to the sine of refraction of a violet ray, their angles of incidence being equal, then PN, perpendicular to OG, will be the intersection of the plane of refraction with the side of the prism that is required.

Thus, the angle SOB being 30°, VOR will be 18′ 12″½, VOG = 50° 38′ 4″½, ROG = 50° 19′ 52″½, whose sines are as the sines of refraction of the violet and the red, in passing from glass into water at a common angle of incidence. And therefore the angles of the emergence of the rays OV, OR, in passing from water into glass, will be equal, that is V*v* will in its passage through the glass prism, be parallel to R*r*, and the rays meeting with equal and contrary refractions at the points *v*, *r*, *o*, as they suffered at V, R, O, will emerge colourless at *o*.

Yet we must not be surprized if the pencil *os* is not absolutely pure light (even supposing the matter, the figure, and the disposition, of the media to be faultless) because (1°) perhaps the refractive powers have not been determined with sufficient exactness. (2°) If the glass plate which contains the water be not very thin, the light will have received a slight tincture in passing through it at O: this however may be remedied by confining the water between two glass prisms. And (3°) it is scarcely possible to make experiments of this kind with a pencil of light so slender as the theory prescribes. But proper allowances being made on these accounts, and the refracting planes adjusted as the lemma directs, the light will emerge sufficiently pure to justify the theory. And the refractions of either medium being given, it will appear from the experiments whether those of the other medium have been determined with sufficient accuracy. Observe also, that as in practice we must fit the water to the glass, not the glass to the water, we are to begin by assuming VR of a convenient magnitude; and supposing the rays V*v*, R*r*, &c. to be parallel within the glass, find the point O to which they

converge in the water, through which a plane may be drawn which shall send them out into the air, in a colourless pencil o s.

Remark 1. The 8th experiment in Sir Isaac Newton's Optics, book 1, part 2, seems to have been made under the conditions which are limited by the foregoing problem; though he does not specify these conditions. For it is to be presumed he did not combine his prism and water at random, but adjusted them so as to produce the expected effect. It is observed also, that he does not give us a description of his experiment so particular as, in most instances, he was wont to do. He thought perhaps that the consequences he deduces from it might sufficiently explain his meaning; especially as he had, in the foregoing propositions, fully established the principles of his theory. However this be, several persons of skill and address in optical matters, have produced experiments in contradiction to that of Sir Isaac, and have affixed meanings to his conclusions which he never could intend, without being grossly inconsistent with himself: an imputation from which common candor and decency ought to have protected so great a name.*

For instance, when he says that "light as often as by contrary refractions it is so corrected that it emergeth in lines parallel to those in which it was incident, continues ever after to be white;" can this assertion possibly bear the meaning they would obtrude upon us? Had Sir Isaac so entirely forgot his own doctrine as not to know, that if the glass prism P N*n*, in the last scheme, be any where above V*v*, terminated by a plane to which the pencil S O is perpendicular, the rays V*v*, R*r*, &c. though emerging parallel to S O, will exhibit their several colours? The sense therefore which the experiments affix to Sir Isaac Newton's words being so absurd, had not they done better to look out for one that was consistent with his theory? and such a one they would have found by only drawing a figure like the foregoing; where the rays of the pencil, reunited in *os* as well as when separated within the glass prism, are parallel to each other and to the incident pencil. But if the water be terminated by a plane different from A C, passing through the point *o*, and making the rays (no longer parallel to S O) to diverge, then the light will by degrees, in passing on from *o*, become coloured; which is Sir Isaac's other position. To this meaning his own words ought to have led the objectors. It was light, not separate rays, which emerged in his experiment: and which (being parallel to the incident light) continued to be colourless. He adds farther, "the permanent whiteness argues, that in like incidence of the rays, there is no separation of the emerging rays;" as much as to say, that in his experiment (as in our 6th fig) the pencil, in passing or repassing, is supposed to meet with surfaces of equal refractive powers, similarly situated.

* The reader ought to be told, that it is not here intended to detract from the merit of the late Mr. Dollond's improvement of refracting telescopes; but only to correct a mistake of his concerning that difference of dispersion of rays, which he has so happily applied to use.—Orig.

The other cases in which refracted light may recover its whiteness, though it emerges not parallel to the incident, or may be tinged though parallel to it, Sir Isaac does not treat of; the experiment he had made, being sufficient for the purposes to which he applies it. But he assures his readers, that if they will argue truly on his theory, trying all things with good instruments and sufficient circumspection, the expected event will not be wanting. And the fact is, that in all the experiments which have been made, if none of the necessary data are wanting, the appearance of the emerging light may be certainly predicted.

Remark 2. When a slender pencil of light is refracted at the surface of any medium, the extreme rays, the violet and red, and the several intermediate rays, each of its particular degree of refrangibility, will all diverge from, or converge to the same physical point: or when that point, by altering the position of the plane, is thrown to an infinite distance, will all of them become parallel. And it appears from the foregoing solution, that such parallelism may always be effected whatever be the refracting power of the medium PN*n*, provided that, in a given medium, the quantities *m*, *n*, &c. of the lemma, which represent the sines of refraction of the several sorts of rays, to a common sine of incidence, continue to be in constant ratios to one another. Conversely if, from experiments such as that which Sir Isaac Newton made, it follows that, whatever be the refractive powers of the media, and the angle of incidence of the light, the pencils S O, *so*, may be made to reciprocate with each other, while all the sorts of rays, in passing or repassing through the prism P N *n*, become parallel; if I say, this is confirmed by experiments, it is a proof that, for any given medium, the ratios of those quanties *m*, *n*, &c. are invariable.

Remark 3. And hence Sir Isaac deduces the two theorems subjoined to his 8th experiment; by the first of which he contrives to make the ratios of the sines of refraction belonging to the several sorts of rays, to a common sine of incidence, when they pass from glass into air, to serve for finding the like ratios for the rays passing from water into air, without the trouble of new experiments.

His first theorem may be deduced in this manner; Let all the sorts of rays, whether united in a pencil of light, or separated parallelwise by refraction, have the same angle of incidence, whose sign is I, when they pass from a denser into a rarer medium; and let V and R stand for the sines of refraction of the extreme (or any two sorts of) rays. Then seeing, by the experiments, the ratio of V to I, is given, as also that of R to I; the ratio of V — I to I, as also (invert.) that of I to R — I, and (ex æquo) that of V — I to R — I are given; for this last write the ratio of I to *p*.

In like manner, let the refractive power of the medium from which the rays emerge into the same medium as before, be increased or diminished, as also the common angle of incidence; and we need only write other marks *v* and *r* for the

sines, and i for the common sine of incidence; for we shall have as before $v-i$ to $r-i$ in a given ratio; which call that of 1 to q. And from these two, we have $\frac{V-I}{v-i} = \frac{R-I}{r-i} \times \frac{q}{p}$. But p is always nearly equal to q; in the refractions from glass, and from water into air, their difference is less than $\frac{1}{400}$ part of either; we may therefore put the ratio $\frac{V-I}{v-i}$ equal to $\frac{R-I}{r-i}$ which is the first theorem.

And thence, if one difference $R-I$ become equal to $r-i$, the other differences $V-I$, &c. will be respectively equal to $v-i$, &c. and the same set of differences may be made to serve for several media, provided the sines of incidence are taken in their due proportion. Thus when red rays pass from glass into the air, we have $I : R :: 50 : 77$ and $R - I : I :: 27 : 50$, and when they pass from water into air $i : r - i :: 3 : 1$, and therefore, as we are to make $R - I$ every where equal to $r - i$, we get, ex æquo, $i : I :: 81 : 50$, as Sir Isaac Newton finds it.

Remark 4. But to explain this matter a little further, and obviate some difficulties concerning it, I shall add the following examples.

The refractive powers being marked as above, let red rays fall from glass into air at the angle of incidence 20°, the angle of refraction will be 31° 47′. Again, let them fall from water into air at an angle of 34° 1′, making their angle of refraction 48° 5′. And the difference of the sines of 31° 47′ and 20° will be precisely equal to the difference of the sines of 48° 5′ and 34° 1′. At the same angles of incidence 20° and 34° 1′, let the violet rays fall from glass and water into air, and the angle of refraction from the glass will be 32° 14′½ nearly, and that from the water will be 48° 38′ nearly. And the difference of the sines of 32° 14′¼ and 20° will be equal to the difference of the sines of 48° 38′ and 34° 1′, within .000488, or less than $\frac{1}{2000}$ part. We see likewise that the red and violet rays diverged from the glass medium at an angle of 27′½; but from the water at an angle of 33′; making the difference of divergence in this example 5½′, that is $\frac{1}{5}$ of the whole divergence of the red and violet rays when refracted from glass into air, at incid. 20°. Whence it appears, that though the differences of the sines above specified, or the excesses in Sir Isaac's theorem, may in refractions from different media into the same rarer medium, be made equal, it does by no means follow, that the divergences of the several sorts of rays (or if you chuse to call it their dispersion) will be the same in the 2 refractions; for Sir Isaac's excesses 27, 27$\frac{1}{9}$, &c. are the excesses of sines; not of angles, as some opticians seem to have misapprehended.

Again, let an unrefracted pencil of light fall from common glass into the air, fig. 7, at the incidence 39°, and the angles of refraction will be,

Of the violet	79°	2′	2″
Of the red	75	43	55
And their difference....	3	18	7

is the divergence of

the extreme rays. And the angle of refraction of the mean ray is $77^\circ\ 16'\ 19''$. By mean ray is understood the ray whose sine of refraction is a geometrical mean between the sines of refraction of the extreme rays, the common radius being unity.

Let now the same rays be refracted the contrary way by a surface of water WT; then, to make the mean emergent ray parallel to the incident pencil, its angle of incidence must be $86^\circ\ 37'\frac{4}{5}$: and the extreme rays will now converge at an angle of $20\frac{1}{3}$ minutes, nearly. Through the point of convergence *o*, draw (by the Lemma) a plane *wt*, to terminate the water, and unite all the rays into a colourless pencil *os*: and this emergent pencil will be found to make, with a perpendicular to the terminating surface, an angle of $49^\circ\ 6'\frac{1}{2}$, and will be inclined to the first incident pencil in an angle of $14^\circ\ 28'\ 20''$. Nor is there any other plane besides this which will thus unite the rays. If planes parallel to it cut the rays any where but in their point of convergence, they will be parallel to each other, but exhibiting their several colours. And planes not parallel to it, will every where give a coloured image, excepting only when they pass through the point of convergence; but then the rays having crossed at that point, will thenceforth diverge from one another, and give a coloured spectrum.

From all which it appears, that light refracted through different media may emerge colourless, though its first direction be considerably altered. And that its mean direction may remain the same, though its extremities be sensibly tinged with colours. Positions which, I know not by what mishap, have been deemed paradoxes in Sir Isaac Newton's theory of light, but which are really the necessary consequences of it.

Of Telescopical Object-Glasses giving an Image free from Colours. Fig. 8 and 9.

If the extreme rays, the red and violet, after one or more refractions, diverge from points D and *d*, the distance of the point of divergence of the least refrangible from the lens, being greater than that of the most refrangible, such a semidiameter of the last spherical surface, from which they are to pass into the air, may be assigned, as shall unite the extreme, and all the intermediate rays, in the same focus F; neglecting the aberration from the figure. The Rule is this: For the distances of the points of divergence from the lens, write D the greater, and *d* the least; the semidiameter of any of the given surfaces being assumed for unity: and $\frac{M}{r}$, $\frac{m}{r}$ expressing the ratios of the sines of incidence and refraction, of the violet and red rays, out of air into the last medium whose surface is required; the semidiameter of that surface will be $\frac{(M - m) \times Dd}{MD - md}$; as may be easily demonstrated from a theorem of Dr. Smith, in the remarks subjoined to his Optics. Thus, if the last medium be glass, the semidiameter of the sur-

face from which the rays pass into the air, must be $\frac{Dd}{78D - 77d}$, it being, in this case, $\frac{M}{r} = \frac{78}{50}$, $\frac{m}{r} = \frac{77}{50}$.

EXAMPLE 1.—Let M*p*CNCM, fig. 8, be a double convex lens of water, confined between the plano-concave MTLN, and the meniscus MKNCM, both of glass, and having the radii of their surfaces contiguous to the water, equal to each other, or to unity; and if a ray s*p*, parallel to the common axis of the lenses, after being refracted by the aqueous lens, have its extreme rays, the red and violet, divergent from the points D and *d*; the distance of F, the focus where all the rays can meet, will be 8.898: and when this happens, the exterior surface of the meniscus, that is, the surface represented by MPKN, will have its radius to that of the inner surface MCN, as 139 to 154.

EXAMPLE 2.—When a double concave of glass, the radii of whose surfaces are unity, is inclosed in water, as in fig. 9, the water being confined on one side by a thin glass plate TL, and on the other by a concentric spherical shell MPKN; the semidiameter of this shell must be to unity as 471 to 547; and the focal distance CF, at which the colourless image is formed, will be 4.77$\frac{2}{3}$. In these examples, the thickness of the lenses is neglected; but it may easily be taken into the account, if it be thought necessary.

The same thing may be effected by means of any media of different refractive powers; for the semidiameter of the last refracting surface being determined according to the foregoing rule, the nearer distance of the points of divergence (*d*), of the more refrangible rays, will be so compensated by their greater refrangibility, that all the rays will converge to the same focus F. And this without introducing any new principle into the science of optics, or any dispersion of light different from the refractions discovered by Sir Isaac Newton near a hundred years ago.

END OF VOLUME ELEVENTH.

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